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Journal of Applied Economics and Policy

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The deadline for program submissions/abstracts is September 1, 2009.

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There will be a "Best Paper Award" as well as undergraduate paper sessions and an award for "Best Undergraduate Paper." A completed paper must be submitted no later than September 1 to be considered.

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The Decision to Smoke and the Frequency of Smoking by Age and Gender[1]

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Abstract

Previous studies have documented the impact of price on cigarette purchases, fewer have examined the joint impact of price and other tobacco control policies by gender and age, especially for those above age 40. This paper focuses on the effect of policies by gender and age, using a nationally representative data set covering the 1993-2002 period. Our work extends the literature as we examine how tobacco control policies (price, clean air laws, and media) relate not only to smoking prevalence, but also to smoking frequency, and the quantity smoked. We find that price impacts smoking prevalence most significantly for smokers 65 and above and females 25-34 and 50-64. We also find evidence that these policies reduce smoking prevalence and the frequency of smoking. These results are robust in that they are supported by a large nationally representative data set over a ten year span and are of interest to policy makers interested in reducing smoking, as we isolate age and gender responses to policy variables.

[1]Funding was obtained from the Substance Abuse Program for Research and Policy of the Robert Wood Johnson Foundation. David Levy also obtained funding from the National Cancer Institute.

I. Introduction

Smoking is the single most *preventable* cause of premature mortality, increasing the risk of lung cancer, emphysema, heart disease, stroke and other diseases (U.S. DHHS 1989; U.S. DHHS 1990). Approximately 440,000 deaths are attributable to smoking each year in the U.S., which results in 5.6 million years of potential life lost, $75 billion in direct medical costs, and $82 billion in lost productivity (CDC 2002). In 2000, approximately 8.6 million people in the U.S. had an estimated 12.7 million smoking-attributable serious chronic illnesses (CDC 2003a).

Recognizing the human and economic costs of smoking, many public interest groups are advocating polices that will reduce smoking including federal and state tax rates that raise the market price of cigarettes, work and public place bans on smoking, and media campaigns. Empirical studies have linked tobacco control policies such as price increases, selective smoking bans or clean air laws, and media campaigns to the reduction in smoking prevalence (CDC 2000)(U.S. DHHS 2000b)(Levy et al. 2004)(Levy et al 2005).

One broad initiative with the support of the federal government is the Healthy People 2010 Project (U.S. DHHS 2000a), which has as its goal a reduction in the smoking prevalence rate, measuring the percentage of the U.S. population that smokes, to 12%. The prevalence rate for adults in the U.S. fell from 24.7 to 20.9 between 1997 and 2005, a 15% relative decline. This decline was not uniform when viewed by age and gender groups. Those ages 18-24 and ages 25-44 observed 15% declines, with almost all the decline between 2002 and 2004. Between 1997 and 2005, smoking rates of those ages 45-64 declined only 10% (from 24.4 to 21.9) while those ages 65 and above declined by 28% (from 12.0 to 8.2), with much of the decline for each of these groups between 1997 and 2002. Men and women overall experienced similar declines (15% vs. 16%) between 1997 and 2004, but women experienced larger declines for the age groups less than 65 and the reverse was true for the 65 and above age groups. In further reducing smoking rates and meeting Healthy People 2010 goals, it will be useful to know why some age and gender groups declined more than others, especially why smoking rates among seniors have declined most in relative terms, and it will be important to know the role of policies.

This paper focuses on the effect of tobacco control policies by age and by gender, using the largest nationally representative data set, the Tobacco Use Supplement to the Current Population Survey (TUS-CPS). We use 4 waves covering a 10-year span, —1992-93, 1995-96, 1998-99, and 2001-02, using more recent data than most prior studies. Using the individual as the unit of observation, we examine how tobacco control policies are associated with smoking prevalence.. In considering quantity smoked, we separately consider whether the individual is a someday (less than every day in the last month) vs. everyday smoker, as well as the quantity smoked by everyday smokers. We consider the role of prices, clean air laws and media. We compare these effects among age groups at different points in time as well as over time.

II. Review of the Literature

The most consistent finding from previous empirical studies of tobacco control policies is that cigarette consumption is inversely related to the price of cigarettes. Many of the previous studies have employed aggregate level data (either time series for one geographic unit or pooled cross-sectional time series for multiple geographic units) and consider aggregate measures of consumption. These studies obtain elasticity estimates ranging from –0.14 to –1.12, with a majority of the estimates falling in a narrower range of –0.30 to –0.50 (Zimring and Nelson 1995)(Chaloupka and Warner 1999)(Levy et al. 2000) (U.S. DHHS 2000b)(Hopkins et al. 2001) (Levy et al. 2004). Differences in the price elasticity estimates can be attributed to differences in the population considered, data used, and modeling techniques.

A growing number of studies have employed micro-level data to examine the determinants of cigarette consumption. Using micro–level data allows researchers to analyze the effects of prices and other policy variables on the probability that an individual smokes and smoking frequency. Recent studies decomposing the effects of price into the effects on smoking prevalence (i.e., participation) rates and the effects on the quantity of cigarettes consumed (i.e., conditional) by those who continue to smoke (Farrelly and Bray 1998)(Chaloupka and Warner 1999) have generally found that about half of the effects result from reductions in prevalence.

While many studies have examined the effect of price on youth and young adults, a limited number of studies have considered how tobacco control policies affect those above age 24. Lewitt (1982), Evans and Farrelly (1998) and Farrelly and Bray (1998) considered price effects in different age groups and found that those smokers at higher ages were less responsive to price. These studies grouped together all individuals age 40 and above. Using a sample of 1990-2002 BRFSS (Behavioral Risk Factor Surveillance System) data, Sloan and Trogdon (2004) obtained participation elasticities of -0.3 for those ages 18-20, -0.1 for ages 21-64, and of -0.25 for ages 65 and above. Ahmad (2005), using 1990-2000 BRFSS data obtained participation elasticities of -0.3 for ages 18-29, -0.2 for ages 30-64 and -0.3 for ages 65 and above. While there is evidence of differing participation elasticities by age, none of the studies consider quantity effects and the studies by age generally do not consider gender effects.

While numerous studies of the effects of price on cigarette smoking have been completed in recent years, a much smaller number of studies have examined the impact other tobacco control policies, such as smoke-free air laws and media campaigns, on smoking behavior. Studies have shown that relatively comprehensive restrictions on smoking in public places are associated both with lower smoking prevalence and lower average daily cigarette consumption by adult smokers, as reviewed by Levy and Friend (2003). Except for youth, differences by age and gender in the effects of other tobacco control policies have received less attention than price policies. In a study of clean air laws (as distinct from smoking bans in private firms), Emont et al (1993) found roughly the same relationship of clean air laws to prevalence and quantity smoked. After accounting for the potential endogeneity of smoking restrictions, Ohsfeldt, Boyle, and Capilouto (1999) concluded that the strongest restrictions on cigarette smoking lead to significant decreases in smoking prevalence. They also found greater effects for those ages 25-44 than other ages and for males than females. After accounting for the potential self selection of workers, Evans, Farrelly, and Montgomery (1999) concluded that workplace smoking bans reduce the probability of adult smoking by 5% and reduce the average daily cigarette consumption of smokers by 10%. Farrelly, Evans and Sfekas (1999) observed less of a relationship between smoking restrictions and smoking rates of those aged 18-24 compared to those aged 40-65. Finally, after controlling for the possibility that unobserved state level sentiment toward smoking may be driving both the creation of new smoke-free air laws and adult smoking rates to decrease, Tauras (2006) found that more restrictive smoke free air laws decreased average quantity smoked by adult smokers, but have little impact on the prevalence of smoking by adults.

Studies of media campaigns generally focus on broader tobacco control campaign expenditure or programs. For example, Farrelly et al. (2003b) using a cross section of states over time estimated that tobacco control expenditures at high levels would reduce per capita tobacco consumption by 8%. A recent meta-analysis (Snyder et al. 2004) found that media campaigns (most of which were generally part of a more comprehensive tobacco control program) yielded a 5% reduction in smoking prevalence. Studies of states with active tobacco control policies, such as California, Massachusetts and Arizona, have seen particularly large reductions in smoking prevalence (U.S. DHHS 2000b). However, no studies have examined the impact of state comprehensive programs by age group nor their impact on smoking frequency.

Previous studies have focused on price, clear air restrictions and media campaigns separately or with two of the policies. This study goes beyond the previous literature by simultaneously considering the effect of all three policies using multivariate analysis. We also estimate separate equations to consider not only how policies affect the decision to smoke but how they affect the quantity smoked, both in terms of smoking frequency (someday versus everyday) and the quantity smoked per day. In addition, we estimate separate equations to consider how the effects differ by age and gender, and how they vary over time. As such, we provide one of the most comprehensive studies of tobacco control policies.

III. Hypothesis and Statistical Methods

Statistical Analysis

The focus of the study is a multivariate analysis of the relationship of three sequential smoking behavior measures to policy variables. The analysis is motivated by first examining mean prevalence rates by age and gender. We consider behavior as a sequence of decisions, expanding on the two part model of demand (Cragg 1971) that is commonly used in health demand studies. The decision to smoke is followed by the decision of whether to smoke everyday or somedays, and then by the decision on the number of cigarettes smoked per day by everyday smokers.

Each multivariate model includes a set of variables (TOBCON) that measure three tobacco control policies: cigarette prices, laws restricting smoking in public places, and the presence of a media campaign. In addition, the model controls for individual demographic and socio-economic characteristics (SOCDEM), such as age, gender, racial/ethnic group, income and education. We also control for state level differences in smoking sentiment (SENTIMENT), underlying geographic factors that might affect the type of tobacco control policies in a state as well the propensity to smoke and the quantity smoked by citizens. Following Tauras (2006), we allowed for geographic differences by presenting results using both regional (the Census Divisions) and state variables. The inclusion of state variables on the one hand leads to multicollinearity with the TOBCON variables, but also provides confidence in the ability to distinguish the role of public policies from other state level factors influencing the decision to smoke.

Formally, the decision to smoke is modelled as:

$$\text{SMOKE}_i = \alpha + \beta_1\text{TOBCON}_i + \beta_2\text{SOCDEM}_{i,} + \beta_3\text{SENTIMENT}_i + \varepsilon_i, \qquad (1)$$

where SMOKE = 1 if a smoker and 0 otherwise.

The sample is all individuals with the requisite data. Stricter tobacco control policies (e.g., higher prices due to higher taxes, stricter clean air laws, and the existence of media campaigns) are expected to be negatively related to the decision to smoke. Equation (1) is estimated using logistic regression.

The second decision stage considers only smokers and models the decision of smokers to smoke somedays or everyday.

$$\text{SOMEDAY}_i = \alpha + \beta_1\text{TOBCON}_i + \beta_2\text{SOCDEM}_i + \beta_3\text{SENTIMENT}_i + \varepsilon_i \qquad (2)$$

where SOMEDAY = 1 if a someday smoker and 0 if an everyday smoker.

The sample is all smokers, and thus represents the decision to smoke some or all days conditional on the decision to smoke. Stricter tobacco control policies are expected to be positively related to the decision to smoke somedays as compared to everyday. Using the someday/everyday dichotomy, we estimate the logistic form of the equation.

The final stage is the decision of how much to smoke by everyday smokers. Unlike the previous two equation where the dependent variable was binary, we now use count data. We estimate:

$$\text{CIGS}_i = \alpha + \beta_1\text{TOBCON}_i + \beta_2\text{SOCDEM}_i + \beta_3\text{SENTIMENT}_i + \varepsilon_{i,} \qquad (3)$$

where CIGS = the number of cigarettes smoked per day by everyday smokers.

The sample is everyday smokers, and thus represents the decision of how many cigarettes to smoke conditional on the decision to smoke everyday. Stricter tobacco control policies are expected to be negatively related to CIGS.

We employ a negative binomial regression to estimate the forms of the conditional demand equation, EQ (3). The negative binomial is specifically designed to model over-dispersed count outcomes such as cigarettes per day. We compared the goodness of fit of the negative binomial model with that of a poisson regression and other generalized linear models (GLM) with exponential distributions. Specifically, we employed maximum likelihood negative binomial regressions to obtain estimates of the overdispersion parameter for each of the specifications described below. In each instance, the estimates indicated evidence of overdispersion. We then used the maximum likelihood overdispersion estimates in subsequent GLM based negative binomial regressions. The value of using the GLM based negative binomial algorithm is that the aforementioned models can all be compared based on goodness of fit criteria. The negative binomial regression yielded the best fitting model with deviance, Akaike information criterion (AIC), and Bayesian information criterion (BIC) statistics lower than the other models. For these models, we report incidence rate ratios (IRR) instead of coefficient estimates to be more consistent with the prevalence estimates (i.e. the prevalence estimates reported as odds ratios), since they are exponentiated coefficients.

For the analyses, we use Stata 9.2 (StataCorp, 2006, College Station, TX) and individual weights to adjust for the survey design. Since the number of observations is not proportional to their presence in the population, the equations are estimated weighted by the population probability weights. For the quantity equations, we also considered the analysis where the standard errors of the estimates are cluster corrected at the state level, and obtained very similar results (not reported). Models are estimated for the total population, by gender, and separately by age (18-24, 25-34, 35-49, 50-64, 65 and above) and gender. Using the estimated equations, price elasticities are calculated using techniques for non-linear equations using estimates at the mean values of the independent variables.

IV. Data and Variables

Individual Level Data

Four waves of the TUS-CPS—1992/93, 1995/96, 1998/99, and 2001-02, each with three sample months (September, January and May, except in 2001-02) —were analyzed in this study. Each wave is a separate sample, i.e., the data is not longitudinal. The probability sample for each wave was based on stratified clusters of households drawn from an initial sampling frame that covers the civilian non-institutionalized population ages 15 and older. Primary data collection was conducted by telephone but about 30 percent of interviews were conducted in-person in the household. We limited the sample to individuals ages 18 and older who were self respondents.

Tobacco Measures

Individual respondents were first screened for tobacco use with an "ever use" screening measure. Respondents who reported that they had smoked at least 100 cigarettes in their lifetime were asked about their current smoking status. Current smokers were queried about the level of their current use in categorical terms (individuals who report now smoking everyday or somedays). A *current smoker* is defined as someone who had smoked at least 100 cigarettes and who was smoking some or all days at the time of the survey. Current everyday smokers were asked how many cigarettes they smoked on the average day. All current smokers, regardless of the frequency or quantity defining their smoking behavior, were included as eligible for this analysis, as long as they had information on the necessary variables.

Socioeconomic Status and other Respondent Characteristics

Using socio-demographic information included in the TUS-CPS data, the sample was divided into five age ranges (18-24, 25-34, 35-49, 50-64, 65 and above), gender and five racial/ethnic groups (White, African American, Asian/Pacific Islander, Hispanic, and Native American/Aleut Eskimos and other). Educational levels were classified into three groups (less than high school, high school graduate/some college, and college graduate/graduate schooling). Five family income levels (less than $10,000, $10,000-$19,999, $20,000-$29,999, 30,000-$74,999, and greater than $74,999) were distinguished. Marital status was distinguished by four separate variables for married or not married, which includes married (the reference group), single, widowed/divorced, separated and never married. Indicator variables were created for each of these classifications, as well as for the state of residence and the nine Census Divisions.

State Level Data: Tobacco Control Policies

Cigarette prices compiled by Orzechowski and Walker (2005) measured the average state level prices of cigarettes, including generics. We adjusted the price indices of the different waves for inflation using the consumer price index from the Bureau of Labor Statistics (www.bls.gov). We also adjusted for state tax changes and price changes at the national level (using the BLS cigarette price index) that occurred between sample months of the four waves. These data represent a snapshot of the state price and excise tax rate on a pack of cigarettes corresponding with the timing of each survey wave.

Clean air laws were represented by an index of state-level clean air regulations informed by CDC, American Lung Association, and NCI (National Cancer Institute 1993) (National Cancer Institute 2000). We initially constructed separate indices for three types of laws: worksite, restaurant, and others (shopping malls, retail stores, enclosed arenas, and public transit). Based on studies of relative impacts (Levy and Friend 2003) states with "no smoking allowed (100% smoke-free)" were counted as 100% of the effect, with "no smoking allowed or designated smoking areas allowed if separately ventilated" as a 50% effect, and with "designated smoking areas required or allowed" as a 25% effect. We used separate indices by type of law, and settled on an aggregate weighted index, with worksite laws weighted by 50%, restaurant laws by 30%, and laws for other public places by 20%. Most of the developments in clean air regulations at the state level occurred after 2001.

For media/comprehensive tobacco control campaigns at the state level, we developed an indicator variable. California and Massachusetts were the earliest states to institute comprehensive campaigns and are thus marked "1" for the full duration of the study period. Between 1994 and early 1999, Arizona and Oregon (initiated in 1995 and 1996 respectively), and Florida and Utah (1997) implemented campaigns. Between late 1999 and 2002, Hawaii, Indiana, Maine, Maryland, Minnesota, Mississippi, New Jersey, New York, Ohio, Vermont, Washington, and Wisconsin instituted programs. We distinguished Florida, Mississippi, and Utah as having youth programs rather than programs targeted to the full population by assigning those states a value of 0.5 instead of 1.0, indicating that these policies are likely to be have a smaller effect (Levy and Friend 2002). The logic of using 0.5 for youth is adopted since these programs target only a portion of the population. Programs initiated in the earlier years have been described in the Surgeon General's report *Reducing Tobacco Use* (U.S. DHHS 2000b). For more recent programs, we considered information in Farrelly et al (2003a) and expenditures on tobacco control programs available through CDC. We included states that spent more than 70% of the CDC goals in 2001 and 2002.

V. Results

<u>Smoking Prevalence</u>

Table 1 presents background data in understanding the differences of age and gender groups on the three steps described by Equations (1) to (3) above. Between 1993 and 2002, there were statistically significant absolute reductions (at the .05 level or greater) for each of the categories. In relative terms, the smoking prevalence for adults fell 14.1% in relative terms. Larger declines were experience by females (15.1%) than males (13.3%). The largest declines were experienced by male and female smokers ages 65 and older, both over 23%, with large declines also for females ages 25-34 (23.5%) and females ages 50-64 (19.2%). Males smoking rates for those ages 25-34, 35-49 and 50-64 all declined between 12% and 15%. Females ages 35-49 declined by only 6% and males and females ages 18-24 both increased by about 4-5%.

<u>Decision to Smoke</u>

Table 2 Panel A and Table 3 Panel A show the estimation results for Eq. (1). Table 2 breaks out male and female smokers and includes equations estimated including both regional and state designators. Table 3 shows age groups by gender and includes regional designators.

The estimates in Table 2 Panel A show that when including only regional controls, all three policy variables are significant and decrease the odds that an individual smokes with a price participation elasticity of -0.06, and with an 11% lower odds of smoking with full clean air laws, and a 6% reduction associated with comprehensive media campaigns. Similar effects are seen for the estimated equations with complete state indicators, except that media campaigns become insignificant, due to the high correlation with state indicators. In addition, the price elasticity increases to -0.13. Similar results were observed in the male and female equations, except that price was insignificant in the female equation with regional indicators, and clean air laws become insignificant in the female equation with state indicators. A higher price elasticity was observed for males (-0.09 and -0.13) than females (-0.03 and -0.12). The estimated coefficients of the non-policy variables (SOCDEM) were as expected and consistent with the literature.

Table 3 Panel A shows the estimation results for age groups by gender including only regional variables, since the versions with state variables were similar. Significant price effects were found for males and females age 65 and above and for females ages 18-24.[2] Clean air negatively impacted smoking prevalence for both males and females between 25 and 49 years of age, for younger males and for females ages 50-64. Media campaigns were significant for females in the three central age groups and marginally significant for males aged 50-64.

Someday vs. Everyday Smokers

The summary data in Table 1 regarding the decision to be a someday or everyday smoker provides an interesting backdrop to the estimations of Equation (2) above. Overall the proportion of the population reporting becoming a someday smoker increased, in relative terms by 10.1% with all of the increase between 1996 and 2002. Larger increases were experienced by males (13.9%) than females (6.8%). In contrast to smoking prevalence, the largest increases were experienced by younger male and female smokers ages 18-24 and 25-34 years of age, with smaller increases at older ages, except among males ages 65 and older. Among females ages 65 and older, the percent of someday smokers declined by 9.2%. The increases in absolute terms were statistically significant for all age and gender categories, except males age 50-64 and females ages 35-49 and ages 50-64.

Table 2 Panel B and Table 3 Panel B show the estimation results for Eq. (2). Table 2 breaks out male and female smokers and includes equations estimated using both regional (Census Division) and state designators. Table 3 shows age groups by gender and includes regional designators.

In the model with the regional indicator variables, price and clean air were positively associated with the decision to smoke somedays, with a price elasticity of 0.23 and with a 30% higher likelihood of being a someday smoker with full clean air laws. Similar results are observed in the male and female equations, except that price was only significant in the female equation with an elasticity of 0.34. In the equation with complete state indicators, only clean air laws was significant with a 40% higher likelihood of being a someday smoker. The estimated coefficients of the non-policy variables were as expected and consistent with the literature.

Table 3 Panel B shows the equations estimated by age and gender groups, and includes regional indicators There are significant price effects for males ages 35-49 and 50-

[2]In the estimated equations with state effects price was significant for both males and females age 65 and above, for males ages 25-34 and for females age 18-24, 50-64 and ages 65 and above.

64 with elasticities of 0.39 and 0.47. For females, price has a significant effect for ages 25-34 with an elasticity of 0.45, ages 35-49 with an elasticity of 0.31, and 50-64 with an elasticity of 0.47. Clean air effects were significant in the male and female regional models for ages 25-34, 35-44 and 50-64, but only in the state model for males ages 50-64 and females 25-34. Clean air also has a significant positive effect in the regional model for females ages 18-24. Media campaigns were only for females ages 50-64.

<u>The Quantity Smoked by Everyday Smokers</u>

Table 1 indicates that the quantity smoked by everyday smokers has declined 9.1% from 20.2 cigarettes per day (cpd) in 1993 to 18.3 cpd in 2002, a 9.1%. Similar decreases were experienced by males (9.1%) and females (9.2%).

Table 2 Panel C and Table 3 Panel C show the estimation results of the conditional decision regarding the quantity smoked per day by everyday smokers. Table 3 includes separate estimations for specifications with both regional and state indicators. Price is significant with price elasticities near -0.1. Similar results are observed in the male and female equations. Clean air laws also have a significant effect in the equations for all male and all female smokers, except in the equation for females with state indicators. For females, media campaigns has a negative and significant effect in the equation with regional indicators.

In the equations by age and gender shown in Table 3 Panel C with regional indicator variables, the price variable is significant for most age groups, although not for senior citizens of either gender. Clean air measures are more also significant for younger males. Media efforts are only significant for young males, 18-24 years and females 590-64 years.

In addition to the equations above, we considered other measures for smoking restrictions besides the simple clean air index. When we added separate variables for restaurant clean air laws and worksite laws, we obtained more consistent results for the restaurant effects. When we included a variable from the TUS-CPS marking indoor workers who were subject to a smoking ban, we found that this variable indicated significant negative effects for each of the subpopulations studied. However, the unusually high log odds ratios with these variables may reflect endogeneity arising from smokers avoiding employment at firms with strict smoking bans. More refined measures of clean air laws measuring local policy variations and enforcement may yield more precise results.

VI. Conclusion

Using 4 waves of a large nationally representative dataset covering a 10-year span, we consider the effect of tobacco control policies by age and by gender. In terms of smokng participation, we found that the largest decreases in relative terms were for males and females ages 65 and above, and for females ages 25-34 and 50-64. Smoking rates for those ages 18-24 increased slightly. The percent of smokers that smoke somedays increased for all age groups, with generally larger increases by males, younger smokers and males above age 65. Everyday smokers also decreased the quantity smoked per day.

We also examined the role of three policies for which there was consistent evidence of affecting smoking prevalence (Hopkins et al. 2001; Levy et al. 2004). We found consistent price effects for models with different specifications and for males and females. We also found more prominent effects for those ages 18-24 and for those ages 65 and above. The former result is consistent with other studies (Hopkins et al. 2001; Levy et al. 2004) and the latter result is consistent with recent studies by Sloan (2004) and Ahmad (2005). Not considered in previous studies, fairly consistent evidence indicated that higher prices increase the odds that an individual who smokes is a someday (as opposed to an everyday) smoker. We found some indication that those in the ages 50-64 may be more likely to reduce quantity consumed in response to high prices, perhaps leading to the higher propensity to quit smoking by those ages 65 and above.

Our analyses suggests that clean air laws also play a role in reducing smoking prevalence and also reducing quantity consumed both by reducing the number of days that the individual smokes and the quantity. Overall we found that having stronger state clean air laws could reduce the smoking prevalence by 11%, with more prominent effects for males and for those between the ages of 25-39, when labor participation is particularly high. We found that strong clean air laws could increase the percent of someday smokers by as much as 30%, with more prominent effects for those ages 25-64. For quantity, more prominent effects were observed for younger smokers. State media/comprehensive campaigns also were found to be associated with lower smoking prevalence and frequency of smoking, although the results were less consistent than for the other policy variables. There was some indication that media campaigns have a greater effect on smoking rates for females than males, especially those between the ages of 25 and 64.

Another interesting finding is that policies appear to affect the quantity decision by those ages 50-64, and the decision to smoke by those over age 64, suggesting that policies may first reduce consumption by smokers which later paves the way for quitting. The implication of reductions in smoking frequency by 18-24 years associated with tobacco control policies also merits attention.

Several limitations of this study should be kept in mind. The policy measure for clean air laws and media campaigns are rather crude, as these policies can take a variety of forms, with the effect dependent on the form of policy. We considered other forms for the clean air laws, and obtained fairly consistent results. We considered age and gender variations, but we did not consider differences by socio-economic status (Levy et al. 2006) and race (Farrelly et al. 1998).

Some recent evidence suggests that the reductions in smoking prevalence may have leveled off in recent years (CDC 2006). The evidence in this paper indicates that implementing stricter policies is important in reducing smoking rates and that there are important differences by age and gender in the effect of tobacco control policies on smoking rates. Policies appear particularly effective for younger adults and seniors. In addition, the effects differ for the decision to smoke and the frequency of smoking, suggesting that effects of policies may unfold over time. These differences may be important in targeting different age and gender groups and developing comprehensive campaigns to reduce smoking rates. Further analysis of smoking rates by age and gender, as well as other socio-demographic characteristics is clearly warranted.

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Table 1:
Smoking Prevalence, Percent Someday Smokers, and Cigarettes Per Day
By Gender and Age, Sample Years

Panel A: Total Sample and Male and Female

Group	Measure	1993 Mean	1996 Mean	1999 Mean	2002 Mean	Rel. change 1993-2002
All	Smoking prevalence	24.2%	23.3%	21.8%	20.8%	-14.1%
	Someday Smokers	17.2%	17.0%	18.3%	19.0%	10.1%
	Average # of cigs.	20.2	19.9	19.2	18.3	-9.1%
Male	Smoking prevalence	26.5%	25.5%	24.0%	23.0%	-13.3%
	Someday Smokers	16.9%	16.9%	18.4%	19.3%	13.9%
	Average # of cigs.	22.1	21.9	21.1	20.0	-9.4%
Female	Smoking prevalence	22.5%	21.7%	20.1%	19.1%	-15.1%
	Someday Smokers	17.5%	17.1%	18.3%	18.7%	6.8%
	Average # of cigs.	18.5	18.2	17.5	16.8	-9.2%

Source: Compiled by Authors using CPS-TUS data.
Total sample size = 703,720

Panel B: Gender Group By Age

Group	Measure	1993 Mean	1996 Mean	1999 Mean	2002 Mean	Rel. change 1993-2002
Male						
18-24	**Smoking prevalence**	27.9%	28.9%	28.9%	29.2%	4.9%
	Someday Smokers	21.9%	23.4%	25.0%	25.9%	18.4%
	Average # of cigs.	18.1	17.7	16.9	16.0	-11.6%
25-34	**Smoking prevalence**	30.4%	28.5%	26.8%	25.8%	-15.0%
	Someday Smokers	19.7%	21.0%	23.0%	24.3%	23.4%
	Average # of cigs.	20.5	20.1	19.2	17.9	-12.5%
35-49	**Smoking prevalence**	30.5%	29.8%	28.3%	26.8%	-12.3%
	Someday Smokers	15.4%	14.6%	16.7%	17.3%	12.3%
	Average # of cigs.	23.6	23.2	21.9	20.9	-11.3%
50-64	**Smoking prevalence**	25.8%	24.8%	23.5%	22.5%	-12.7%
	Someday Smokers	13.9%	13.9%	14.5%	14.6%	5.6%
	Average # of cigs.	24.1	23.9	23.5	22.3	-7.7%
65+	**Smoking prevalence**	13.1%	12.7%	10.8%	10.0%	-23.5%
	Someday Smokers	15.2%	14.7%	15.1%	18.7%	22.6%
	Average # of cigs.	20.1	20.2	20.6	19.2	-4.6%
Female						
18-24	**Smoking prevalence**	24.3%	24.2%	24.3%	25.2%	3.6%
	Someday Smokers	19.8%	19.9%	22.5%	23.8%	20.3%
	Average # of cigs.	15.8	15.3	14.3	13.3	-16.1%
25-34	**Smoking prevalence**	28.2%	25.8%	23.1%	21.6%	-23.5%
	Someday Smokers	18.5%	18.2%	21.4%	22.5%	21.6%
	Average # of cigs.	17.6	17.2	16.1	15.6	-11.6%
35-49	**Smoking prevalence**	25.1%	25.1%	24.1%	23.6%	-6.0%
	Someday Smokers	16.5%	16.4%	16.8%	17.1%	3.8%
	Average # of cigs.	19.9	19.4	18.5	17.6	-11.5%
50-64	**Smoking prevalence**	22.5%	22.2%	20.1%	18.2%	-19.2%
	Someday Smokers	15.5%	14.8%	15.7%	15.6%	1.0%
	Average # of cigs.	19.5	19.4	18.8	18.3	-6.3%
65+	**Smoking prevalence**	11.3%	10.5%	9.5%	8.5%	-24.2%
	Someday Smokers	19.8%	18.5%	18.2%	18.0%	-9.2%
	Average # of cigs.	16.7	16.8	16.8	16.3	-2.2%

Source: Compiled by Authors using CPS-TUS data.
Total sample size = 703,720

Table 2:
Multivariate Equation Results: Total and By Gender

	Panel A: Logistic Estimates: Decision to Smoke (EQ 1)			Panel B: Logistic Estimates: Smoking Frequency (EQ 2)			Panel C: Negative Binomial Estimates: # Cigarettes (EQ 3)		
	Price	Clean air	Media	Price	Clean air	Media	Price	Clean air	Media
Estimation Group									
Male & Female									
Odds Ratio	0.93***	0.89***	0.94***	1.32***	1.32***	1.01	1.00***	0.92***	1.00
t-stat	-2.34	-4.58	-4.31	3.77	5.31	0.33	-6.57	-6.44	-0.64
Elasticity[1]	-0.06	-0.08	-0.04	0.23	0.23	0.01	-0.10	-0.02	-0.001
Male & Female With State Indicators									
Odds Ratio	0.85***	0.87***	0.99	0.96	1.4***	0.97	1.00***	0.96	1.01
t-stat	-3.36	-2.8	-0.46	0.1	0.15	0.04	-3.72	-1.57	0.92
Elasticity[1]	-0.13	-0.11	-0.006	-0.35	3.11	-0.76	-0.80	-0.01	0.001
Male									
Odds Ratio	0.89***	0.91***	0.96***	1.1	1.32***	0.96	1.0***	0.92***	1.01
t-stat	-2.32	-2.7	-2.05	0.89	3.69	-0.92	-5.10	-5.03	0.70
Elasticity[1]	-0.09	-0.07	-0.03	0.08	0.23	-0.03	-0.11	-0.02	0.001
Male With State Indicators									
Odds Ratio	0.84***	0.83***	1.02***	0.9	1.38**	0.91*	1.00***	0.95	1.02
t-stat	-2.54	-2.62	0.61	-0.67	2.13	-1.77	-3.13	-1.60	1.82
Elasticity[1]	-0.13	-0.16	0.01	-0.08	0.26	-0.7	-0.10	-0.01	0.004
Female									
Odds Ratio	0.96	0.87***	0.92***	1.53***	1.33***	1.07	1.00***	0.94***	0.98*
t-stat	-0.95	-4.1	-4.3	4.28	3.8	1.62	-4.13	-3.83	-1.76
Elasticity[1]	-0.034	-0.011	-0.6	0.34	0.23	0.05	-0.09	-0.10	-0.003
Female With State Indicators									
Odds Ratio	0.88**	0.95	0.97	1	1.39**	1.05	1.00***	0.98	0.99
t-stat	-1.97	-0.75	-1.29	0.02	2.15	1.04	-2.09	-0.49	-0.80
Elasticity[1]	-0.12	-0.06	-0.02	0	0.27	0.04	-0.06	-0.003	-0.001

[1]For the negative binomial estimates an IRR is reported, not an odds ratio, and a Z Score is reported instead of a t score**.** Elasticity computed based on estimated parameters using mean values for all variables
** denotes coefficient significance at the 95% level
*** denotes coefficient significance at the 97.5% level
Controlling for Socio-demographic variables: racial/ethnic group, education level, income level, and marital status. The sample consisted of 703,720 for the decision to smoke equation, 160,236 in the decision to smoke some or every day, and 130,589 in the quantity equation

Table 3:
Multivariate Estimation Results: Gender by Age Group

	Panel A: Logistic Estimates: Decision to Smoke (EQ 1)			Panel B: Logistic Estimates: Smoking Frequency (EQ 2)			Panel C: Negative Binomial Estimates: # Cigarettes (EQ 3		
Male	**Price**	**Clean air**	**Media**	**Price**	**Clean air**	**Media**	**Price**	**Clean air**	**Media**
18-24 years									
Odds Ratio	0.86	0.83*	0.94	0.71	0.98	0.92	1.00***	0.89***	1.11***
Elasticity	-0.11	-0.13	-0.04	-0.26	-0.01	-0.06	-0.15	-0.02	0.02
25-34 years									
Odds Ratio	0.92	0.87**	1.04	0.86	1.36**	0.93	1.00	0.90***	0.99
Elasticity	-0.6	-0.1	0.02	-0.11	0.24	-0.5	-0.05	-0.02	-0.003
35-49 years									
Odds Ratio	0.9	0.88***	0.96	1.36*	1.49***	0.95	1.00***	0.93***	0.99
Elasticity	-0.8	-0.1	-0.02	0.26	0.33	-0.04	-0.16	-0.01	-0.003
50-64 years									
Odds Ratio	0.93	1.04	0.93*	1.58*	1.41*	1.04	1.00***	0.93***	0.98
Elasticity	-0.05	0.03	-0.5	0.39	0.29	0.03	-0.10	-0.02	-0.001
65 and older									
Odds Ratio	0.71**	1.02	0.91	1.76	1	1.09	1.00	0.90	1.03
Elasticity	-0.29	0.01	-0.9	0.47	0	0.07	-0.02	-0.02	0.01
Female	**Price**	**Clean air**	**Media**	**Price**	**Clean air**	**Media**	**Price**	**Clean air**	**Media**
18-24 years									
Odds Ratio	0.73**	0.98	0.92	1.34	1.49*	1.05	1.00	0.85***	1.00
Elasticity	-0.24	-0.01	-0.06	0.23	0.31	0.04	-0.10	-0.34	-0.0005
25-34 years									
Odds Ratio	0.99	0.83***	0.9***	1.74***	1.41**	1.01	1.00***	0.96	0.98
Elasticity	-0.1	-0.15	-0.08	0.45	0.28	0.01	-0.11	-0.01	-0.003
35-49 years									
Odds Ratio	1.08	0.84***	0.89***	1.44**	1.33**	1.05	1.00***	0.94***	0.99
Elasticity	0.05	-0.13	-0.09	0.31	0.23	0.04	-0.08	-0.01	-0.001
50-64 years									
Odds Ratio	0.98	0.75***	0.92***	1.73***	1.34*	1.23**	1.00***	0.98	0.95***
Elasticity	-0.02	-0.23	-0.07	0.47	0.24	0.17	-0.12	-0.004	-0.01
65 and older									
Odds Ratio	0.75**	1.22**	1.09	1.43	0.79	1.02	1.00	-0.91	1.02
Elasticity	-0.26	0.18	0.1	0.29	-0.19	0.01	0.09	-0.02	0.004

[1]For the negative binomial estimates an IRR is reported, not an odds ratio,
* denotes coefficient significance at the 90% level.
** denotes coefficient significance at the 95% level
*** denotes coefficient significance at the 97.5% level

Elasticity computed based on estimated parameters using mean values for all variables.

Controlling for Socio-demographic variables: racial/ethnic group, education level, income level, and marital status

CONTENTS

Assessing the Effectiveness of Financial Fitness for Life in Eastern Kentucky[1]

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Abstract

In 2005, the Center for Economic Education at Eastern Kentucky University partnered with Eastern Kentucky University and the Kentucky Council on Economic Education to test the effectiveness of the National Council on Economic Education (NCEE) curriculum, Financial Fitness for Life (FFFL), in an underprivileged region of Kentucky. We recruited local teachers at three different levels to teach the curriculum to their students and used a test instrument developed by NCEE to measure learning. We find that the use of FFFL does increase student performance on a posttest assessment when compared with a pretest of those same students. When demographic statistics are added, both an OLS regression and an analysis-of-variance model comparing test results from a control group when FFFL is not used and the test group when the curriculum is used show an increase in financial literacy when using FFFL. Comparing the test group to a subset of the control group that used materials other than FFFL to teach financial concepts also shows an increase in financial literacy for the FFFL group instead of whatever other curricula were used.

I. Introduction

As shown in a number of studies, most notably the annual surveys of high school seniors conducted by the JumpStart Coalition (2006), the financial knowledge and abilities of today's students are deficient. Today's youth are faced with more and more choices involving financial management, and financial literacy is increasingly important for them to make good life decisions. Because of the results of the JumpStart Coalition surveys in recent years showing that high school seniors' financial literacy is low, there has been increasing interest in teaching financial concepts as more states have begun teaching financial literacy in public schools. Encouragingly, the 2004 survey showed that, for the first time since 1997, high school students demonstrated more knowledge about financial matters (Federal Reserve, 2004). This positive turnaround continued to be evident in the JumpStart Coalition's 2006 survey results as well (JumpStart, 2006). Perhaps this can be attributed to the

[1]This project was made possible by the National Council on Economic Education through funding from the United States Department of Education Office of Innovation and Improvement.

implementation of financial curricula such as *Financial Fitness for Life (FFFL)*. The survey results showed that having parents involved plays an important role in financial education, and one of the differentiating components of the *FFFL* curriculum is that it includes a Parent Guide containing activities and discussion prompts that involve parents in their children's financial education. In this study, we test the effectiveness of this curriculum in improving students' knowledge of economic and financial concepts.

We focused on an economically challenged area of the Commonwealth of Kentucky where financial education is greatly needed. According to the 2004 JumpStart Coalition survey results, students whose parents did not have college degrees performed worse on the survey as did students who reported that they were not college bound. Kentucky lags behind the rest of the country in the percent of the population with college education (Council on Postsecondary Education, 2007), and we specifically targeted an area of the state where a large percentage of the parents are not college educated and where students are unsure of future educational plans. According to a 2004 FDIC report, people who have less formal education and lower household incomes need more financial education. This is the group that we targeted in our study.

We recruited teachers and students in upper elementary, middle- and high-school grades. In 5th, 8th, and 11th grades, economic and financial concepts are covered on Kentucky's standardized tests. Thus, teachers in these grades would likely be most interested in implementing a new economics curriculum and devoting the class time required to cover the concepts. Personal finance concepts such as budgeting and goal setting are covered in the Kentucky practical living core content. Because today's teachers are focused on high stakes assessment and teaching specific core content to improve their school's scores, offering training and materials that could help them succeed would be a significant benefit to them. Also, we anticipated that teaching the *FFFL* curriculum would improve schools' practical living scores on Kentucky's standardized tests. While not testing for improved practical living scores explicitly due to data constraints, the hypothesis of this study is that utilizing *FFFL* will improve students' financial literacy.

In an overview of studies on the effectiveness of financial literacy programs, Braunstein and Welch (2002) find that financial education has been somewhat successful, depending on the goal. The more specific education programs have been more successful than general financial education programs, and *FFFL* provides students with specific tools needed to make good financial decisions, increasing financial literacy.

We test the effectiveness of *FFFL* on student achievement as exhibited by scores on test instruments that were developed by the National Council on Economic Education (NCEE). These tests are titled *Financial Fitness for Life: High School Test* (FFFL-HS Test), *Financial Fitness for Life: Middle School Test* (FFFL-MS Test), and *Financial Fitness for Life: Upper Elementary Test* (FFFL-UE Test). These tests were developed and administered in trials during 2002-2004. A National Advisory Committee was formed to develop each test. Field testing was done in 2003, and the final versions were used in Texas during the 2003-2004 school year. Each test contains "theme tests" that correlate with the themes in the *FFFL* curriculum. There is a 10-question, multiple-choice test for each theme. For detailed information about the tests, refer to the following 2005 NCEE publications: *FFFL High School Test Examiner's Manual*, *FFFL Middle School Test Examiner's Manual*, and *FFFL Upper Elementary Test Examiner's Manual*. In these manuals, the preparers report satisfactory validity and reliability results for the entire test. The authors also discuss the possibility of using a subset of the questions – for classes where it is not possible to cover every lesson in the *FFFL* curriculum. They show that the tests are still reliable if teachers use only some of the theme tests and not the entire test. This is what we did in our study because our teachers were not able to teach the entire curriculum during the testing period.

II. Methodology and Analysis

During spring, 2005, we identified and recruited teachers for participation in the project. We primarily recruited within the counties of the Eastern Kentucky University service region. This region is a 22-county area of southeastern Kentucky which is largely poor.[2] Seventeen of the counties are classified as "Distressed Counties" by the Appalachian Regional Commission, placing them among the poorest 10% of counties nationally (Appalachian Regional Commission, 2008a)(Appalachian Regional Commission, 2008b). Of the 21 counties in Appalachia, 16 of them have college completion rates below 10%, and only one has a college completion rate which is even half as large as the national college completion rate (Appalachian Regional Commission, 2008c). This is the region targeted for the study. Eventually, however, some teachers from counties bordering the region and from counties east of the region were also included to increase the number of observations. Several of these teachers came from counties which have comparable rates of poverty and educational attainment.

To generate interest, teachers were offered a $250 stipend and the *FFFL* Teachers Resource Package (normally sold for $80), along with related materials and training. Studies such as these take time from teachers' schedules to gather the data. Class periods are used for testing, and time is used to learn and implement the new curriculum. Because of this, random samples are generally not attainable as teachers must be recruited. One drawback of this is that the teachers who participate are usually ones who are interested in and enthusiastic about the topic, or they feel they are underprepared to teach topics their school requires of them. The teachers do not receive a reward or punishment based on how well the students score on the tests. We want an honest effort, but we do not want to alter behavior from the normal teaching of a class.

Through introductory sessions held either in person or online through a Blackboard course website, the requirements and rewards for participation in the study were explained to them. The teachers also previewed the curriculum. In addition, we asked the teachers if they foresaw any significant changes in the composition of the classes they taught that spring and the classes they would teach in the fall of 2005 in terms of student academic ability and demographic characteristics. If teachers expected to change the grade level they taught, they

[2]The Kentucky counties within EKU's service region are Bell, Boyle, Casey, Clay, Estill, Garrard, Harlan, Jackson, Knox, Laurel, Lee, Leslie, Lincoln, Madison, McCreary, Owsley, Perry, Powell, Pulaski, Rockcastle, Wayne, and Whitley.

did not stay in the study. After the introductory sessions were completed, we had 40 teachers participating in the study representing 20 Kentucky counties.

At the introductory sessions we also discussed which lessons would be most useful and relevant based on the Kentucky Department of Education's core content requirements since it was clear that every teacher would not be able to teach all of the lessons in a single semester. Following these discussions, we chose eight lessons for each group to teach. These lessons were from three subsets or themes of the curriculum: Saving, Spending and Credit, and Money Management. Therefore, the teachers gave a test composed of a subset of their respective *FFFL* tests that included only questions from those three themes.

The tests described above were sent to the participants during the spring, and they were asked to test the students they were currently teaching – before they received training and copies of the *FFFL* curriculum but after they had taught whatever financial curriculum they were using that spring. We received test results from 15 elementary teachers and 21 middle- or high-school teachers while 5 teachers dropped out of the project without giving their students the tests (one of the teachers taught both middle- and high-school and tested both groups). For each of the participating teachers' students, we obtained the results of an in-class examination, an assessment from the teacher of each student's overall academic ability, and self-reported demographic information. Because the teachers tested their students after completing their teaching of financial topics, these tests are posttests (even though no pretests were given in the spring).

We initially planned to get student scores on a standardized test as a measure of student academic ability, but we were not able to do that for such a wide range of grade levels because the scores would not be comparable across grades. In Kentucky, school students take part in the Commonwealth Accountability Testing System (CATS), a system of exams which is used to assess schools and districts. These exams measure achievement instead of aptitude. With this system, the students in each grade take a different set of tests than students in other grades. For example, fifth graders are tested on economics and practical living, among other subjects, but fourth graders are not. So, a score for a fifth grader is not comparable to one for a fourth grader. Because of the burden of CATS, we decided to forego a standardized test of academic ability and instead ask the teachers to report, for each student, whether the student's academic ability was below average, average, or above average. This provides some indication of each student's academic ability based on the experience of the participating teachers.

During the summer of 2005, we held workshops to train the teachers in the use of the *FFFL* curriculum and provided grade-appropriate copies of the curriculum to all of the participants. We offered the training on two different days in two different locations in order to accommodate teachers who were spread over a large geographic area. Thirty-three participants completed the summer training.

In the fall, these teachers taught from the curriculum, being certain to include at least the eight specific lessons chosen by the project leaders. These students were given the examination as a pretest and again as a posttest, and the same data as the spring semester were collected. Twenty-seven teachers ultimately completed the project – 9 elementary teachers, 7 middle-school teachers, and 11 high-school teachers. The 27 teachers were from 20 different counties.

We examine two separate questions using the data. First, we perform a test of the difference in mean test scores on the pretest and posttest instruments for the students in the fall only to see if there are significant improvements. The statistical methods we employ are the same as those used in Harter, Becker, and Watts (2004) where mean results on surveys administered in 1995 and 2000 are compared and discussed. Second, we investigate the effect of using the *FFFL* curriculum on students' test scores. For the dependent variable, we combined scores on the spring test with the scores on the fall posttest for the dependent variable and use ordinary least squares to regress this variable on the following variables:

- a student academic ability variable that equals 0 if average or below average and 1 if the student is above average;
- a student gender variable that equals 1 if the student is female and 0 if not;
- a student race variable that equals 1 if the student's race is white and 0 if not;
- a dummy variable denoting student's grade level (for elementary the dummy variable equals 1 for 5th grade and 0 if not, for middle the dummy variable equals 1 for 8th grade and 0 if not, for high school the dummy variable equals 1 for 12th grade and 0 if not); and,
- an *FFFL* dummy variable that equals 1 if the student is in a class that used the *FFFL* curriculum and 0 if not.

This research method follows the work of others who have tested whether a particular teaching method or resource, such as new technology, is beneficial. For example, Agarwal and Day (1998) find that internet use does have a positive effect on both TUCE III scores and final grades in introductory economics. Rankin and Hoaas (2001) study whether computer-assisted instruction improves student performance, finding no such improvement. They also find no effect on student attitudes and teaching evaluations. Harter and Harter (2004) test the effectiveness of online quizzes, finding no link between the use of the technology and student performance on examinations.

A difference in this study of the effectiveness of the *FFFL* curriculum is that all of our independent variables are categorical variables. Because of this, we also address the second question using an analysis-of-variance model to examine whether student test scores are influenced by student characteristics of gender, academic ability, grade level, and race as well as having been in a class where *FFFL* was used.

III. Results

A primary result of the study is that the *FFFL* curriculum does increase student scores on the assessment instrument. We find this result when examining only the fall group and doing difference of means tests to compare scores on the posttest versus the pretest during the fall of 2005 for each set of data. These results are presented in Table 1. The tests show that there was a statistically significant improvement for all three levels of our study. Thus, *FFFL* does cause an increase in financial literacy.

Determining that result is one of two main objectives of the study. The other is to test whether *FFFL* yields greater financial literacy than whatever curricula the teachers were previously using. In order to investigate this question, we combine test results from the spring group with posttest results from the fall group. Student descriptive statistics are given in Table 2. This table includes all students in the study, including some who did not complete all of the assessments and are not included in the regressions. As we expected, there was a large percentage of 5th-grade teachers in the elementary group; however, we were surprised to see so many 7th-grade students in the middle-school group and so many students from lower high-school grades, particularly 9th and 10th grades since economics is not tested on statewide standardized assessments in those grades. This might best be explained by the discovery that financial concepts are integrated into students' studies in a variety of ways in Kentucky – through activities led by guidance counselors and librarians as well as through more traditional business education, social studies, and civics classes – and at various grade levels. It is also evident from Table 2 that our student population is mostly white and about half female while teachers rated 20%-30% of their students to be above average in academic ability.

Combining the spring and fall test scores for each of the three sets of data (elementary, middle, and high), we used Pearson's Chi-Squared tests to investigate whether the student characteristics for our spring and fall groups were similarly distributed. We found some differences between the spring group and the fall group for all three grade levels. We found that the distribution of what grades the students were in was statistically significantly different for all three sets of data. There were statistically significantly more 3rd graders, more 7th graders, fewer 12th graders, and more 9th graders in spring than fall. Also, there were statistically significantly more females in the spring for the middle-school data.

We suspected that some of these differences were attributable to the fact that there were eight teachers who gave the spring test and then dropped out of the study. So, we confined our analyses to data from only those teachers who completed the entire study. We conjectured that the spring group and the fall group would be more similar under those circumstances. In fact, as part of our introductory questionnaire, we asked the teachers if they foresaw any significant differences in the make-up of the classes they were teaching in spring and the classes they would teach in fall. All of the participants answered, "No," and if teachers switched grade levels, for example, they did not stay in the study. We tried to ensure that the fall group and spring group would be similarly distributed in terms of student abilities and demographics.

After confining the data to teachers who participated in both spring and fall, we repeated the Pearson's Chi-Squared tests and still found some differences.[3] However, the

[3]In the elementary group, there were still statistically significantly more 3rd graders in the spring group. In the middle-school group, there were still more 7th graders in the spring and more females in the spring. In the high-school group, there were still more 9th graders and fewer 12th graders in the spring.

differences do not constitute significant changes that affect the statistical results. If we confined the data to specific grades that were not statistically significantly distributed differently in terms of race, gender, and ability, the overall results did not change. Therefore, we report the results using all of the classes where the teachers completed the entire study.

Results from the OLS regressions are reported in Table 3. As expected, academic ability is a strong, significant predictor of test score for all groups. For the elementary students, being in a 5th-grade class as compared to a 3rd- or 4th-grade class is a positive predictor of test score as is being in a class that used *FFFL*. Gender and race are not significant for the elementary students. For the middle-school students, being in an 8th-grade class as compared to a 7th-grade class is a positive predictor of test score as is being in a class that used *FFFL*. In this model, having a race of "White" is also a positive and significant predictor of test score while gender is still not significant. For the high school group, we also find that being in a 12th-grade class as opposed to a lower grade and being in a class that used *FFFL* are positive and significant at the .000 level. Being white is a positive, significant predictor of test scores, and, interestingly, being female is a negative, significant predictor of test scores. This gender effect, which has been widely examined (Siegfried, 1979)(Walstad and Soper, 1989)(Heath, 1989) does not show up until the high school group, and the race effect (Walstad and Soper, 1988)(Walstad and Soper, 1989) shows up in middle school and high school but not in the elementary grades.

As further evidence of the impact of using the *FFFL* curriculum on student financial literacy, we used a subset of our data that included teachers from the spring who taught financial concepts using materials other than *FFFL*.[4] That is, we omitted all of the teachers who did not teach any financial concepts and also the teachers who did use *FFFL* during Spring, 2005. When we used these data, we still found that using *FFFL* was a positive and significant predictor of test score for the elementary students and the high school students. It was not significant for the middle school group, but there were only two teachers in this group who taught financial concepts and did not use *FFFL* and a total of 165 students.

As described earlier, because all of our independent variables are categorical, we also use Analysis of Variance (ANOVA) tests to examine the differences in test scores by different groups – according to race, gender, academic ability, and whether or not the class had been taught the required lessons from the *FFFL* curriculum. We use the same dependent

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[4]When we asked the teachers what materials they used during Spring, 2005, most reported "bits and pieces" or "this and that" for the materials they used. In addition, they listed a variety of items including miscellaneous lessons gathered from the internet and elsewhere such as *Introduction to Business* textbook, *Making Money Management Moves*, Junior Achievement, *Banking and Financial Systems* textbook, *Dollars and Sense*, *Personal Living* textbook, bank program on checkbook writing, balancing checkbook and using credit cards, materials from cooperative education service, attorney general consumer fraud presentation, *Life Smarts* from University of Montana, insurance presentation, textbook – Glencoe *Consumer Education and Economics*, *Get a Grip on Your Money* (by Larry Burkett-Crown Financial), basic definitions of budgeting, checking, and values, Ford Credit Corp. programs called "What Is Credit?" and "Credit Drives America," Thompson Business Math textbook, *Wallet Wisdom*, *Practical Money Skills for Life*, *Learning From the Market*, Stock Market Game, Textbook titled *Working - Career Success for the 21st Century* by Thomson/Southwestern Publishing, *Skills for Consumer Success*, and *Econ and Me*.

variable as in the OLS regressions, scores on the spring test and scores on the fall posttest. The independent variables are as follows:

- a student academic ability variable that equals 0 if the student is below average, 1 if the student is average, and 2 if the student is above average;
- a student grade level variable (3, 4, or 5 for elementary, 7 or 8 for middle, 9, 10, 11, or 12 for high)
- a student gender variable that equals 1 if the student is female and 0 if not;
- a student race variable that equals 0 if the student's race is "American Indian or Alaska Native," 1 if "Asian," 2 if "Black or African-American," 3 if "Native Hawaiian or Other Pacific Islander," 4 if "White," and 5 if "Some Other Race";
- an *FFFL* dummy variable that equals 1 if the student is in a class that used the *FFFL* curriculum and 0 if not.

The ANOVA test specifically tests for the difference between the means of two or more groups, and these results are provided in Table 4. Again, it is evident that the three variables by which mean test scores differ are academic ability, grade level, and use of *FFFL*. Race is significant for the middle school group, and gender is significant for the high school group. These results reinforce the regression results.

IV. Conclusion

The primary objective of the study has been to test the effectiveness of the *FFFL* curriculum. We provide evidence that this curriculum does increase financial literacy in grades 3-12. We also show that, at least for elementary and high school students, student financial literacy is higher from using *FFFL* than the level of financial literacy resulting from whatever curricula the teachers were previously using to teach financial concepts.

There are several reasons why this study is important. The result that the *FFFL* curriculum is an effective method of increasing financial literacy is useful for teachers, parents, and students, as well as for supporters of economic and financial education. Schools have limited funds to spend on materials, and evaluation and assessment results can help them choose wisely. These results can also be used when designing materials and resources intended to improve economic and financial literacy. Subsidiary benefits (that are not testable within the constraints of this study) are that teachers will continue to use the curriculum in the future and impact more students than those involved in our study and that students may make better financial decisions throughout their lifetimes. Also, teachers and students may become more comfortable with the concepts and more knowledgeable in their own decision making. [5] Increasing financial literacy prior to high school graduation might help students understand these concepts better or more readily when exposed to them during college or employment, but the primary focus of the curriculum is to help them make good

[5]The teachers in this study answered an informal survey after using the curriculum. Over ninety percent who participated in the survey indicated satisfaction with the curriculum, with only two expressing dissatisfaction. Students also were asked their impressions of the curriculum. At least two-thirds liked the curriculum and thought it taught lessons which would be useful in their lives.

financial decisions that are part of daily life – and these may include the decision to attend college.

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Table 1:
Difference of Means Test Results

	Fall Pretest Mean	Fall Posttest Mean	Difference of Means	t-Statistic	p-Value
Elementary	12.79	18.06	5.27	17.01	0.000
Middle	11.67	13.21	1.54	6.75	0.000
High	11.39	14.11	2.72	9.07	0.000

Table 2:
Mean Values and Variable Definitions

Mean Values (standard deviations in parentheses)

	Elementary Grades 3-5		Middle Grades 7,8		High Grades 9-12	
Variable	**n**	**Mean**	**n**	**Mean**	**n**	**Mean**
Spring Test	651	15.14 (5.34)	400	11.86 (4.68)	605	12.20 (4.83)
Fall Pretest	335	12.79 (4.47)	314	11.67 (4.26)	433	11.39 (3.80)
Fall Posttest	346	18.06 (6.19)	356	13.21 (5.60)	447	14.11 (6.43)
Fifth	765	0.77 (0.42)	na	na	na	na
Eighth	na	na	722	0.49 (0.50)	na	na
Twelfth	na	na	na	na	1004	0.15 (0.36)
Ability	775	0.30 (0.46)	715	0.21 (0.41)	1014	0.27 (0.44)
Female	756	0.49 (0.50)	724	0.50 (0.50)	1010	0.51 (0.50)
White	759	0.91 (0.29)	722	0.94 (0.24)	1011	0.88 (0.33)

Variable Definition

Spring Test: results from *FFFL* test given at the end of the Spring, 2005, semester

Fall Pretest: results from *FFFL* pretest given at the beginning of the Fall, 2005, semester

Fall Posttest: results from *FFFL* posttest given at the end of the Fall, 2005, semester

Fifth: dummy variable equal to 1 if elementary student is in 5th grade and 0 otherwise

Eighth: dummy variable equal to 1 if middle-school student is in 8th grade and 0 otherwise

Twelfth: dummy variable equal to 1 if high-school student is in 12th grade and 0 otherwise

Ability: dummy variable equal to 1 if teacher rated student's academic ability Above Average and 0 otherwise

Female: dummy variable equal to 1 if student is female and 0 otherwise

White: dummy variable equal to 1 if student is White and 0 otherwise

Table 3A:
OLS Regression Results for Elementary Grades 3, 4, and 5
Dependent Variable: Post-test Score on Themes 2, 3, and 4 of FFFL-UE Test

Number of Observations = 744 Adjusted R-squared = 0.33

Variable	Coefficient Estimate	t-statistic	p-value
Above Average Academic Ability	5.355	13.717	0.000
Fifth Grade	4.861	11.346	0.000
Female	0.221	0.621	0.535
White	0.845	1.369	0.171
FFFL	2.601	7.259	0.000
Constant	9.171	13.491	0.000

Table 3B:
OLS Regression Results for Middle School Grades 7 and 8
Dependent Variable: Post-test Score on Themes 3, 4, and 5 of FFFL-MS Test

Number of Observations = 699 Adjusted R-squared = 0.17

Variable	Coefficient Estimate	t-statistic	p-value
Above Average Academic Ability	3.653	8.879	0.000
Eighth Grade	2.395	7.064	0.000
Female	0.168	0.494	0.622
White	2.479	3.514	0.000
FFFL	1.057	3.107	0.002
Constant	7.406	9.718	0.000

Table 3C:
OLS Regression Results for High School Grades 9, 10, 11, and 12
Dependent Variable: Post-test Score on Themes 3, 4, and 5 of FFFL-HS Test

Number of Observations = 995 Adjusted R-squared = 0.15

Variable	Coefficient Estimate	t-statistic	p-value
Above Average Academic Ability	3.778	9.956	0.000
Twelfth Grade	2.456	5.300	0.000
Female	-1.547	-4.594	0.000
White	0.986	1.920	0.055
FFFL	1.556	4.616	0.000
Constant	10.902	21.038	0.000

Table 4A:
ANOVA Results for Elementary Grades 3, 4, and 5
Dependent Variable: Post-test Score on Themes 2, 3, and 4 of FFFL-UE Test

Number of Observations = 744 Adjusted R-squared = 0.37

Variable	Degrees Freedom	F-Statistic	Prob > F
Overall Model	10	44.61	0.000
Academic Ability	2	121.21	0.000
Grade Level	2	67.38	0.000
Gender	1	0.08	0.777
Race	4	2.35	0.053
FFFL	1	57.13	0.000

Table 4B:
ANOVA Results for Middle School Grades 7 and 8
Dependent Variable: Post-test Score on Themes 3, 4, and 5 of FFFL-MS Test

Number of Observations = 699 Adjusted R-squared = 0.21

Variable	Degrees Freedom	F-Statistic	Prob > F
Overall Model	10	19.18	0.000
Academic Ability	2	58.67	0.000
Grade Level	1	59.28	0.000
Gender	1	0.03	0.854
Race	5	2.75	0.018
FFFL	1	8.51	0.004

Table 4C:
ANOVA Results for High School Grades 9, 10, 11, and 12
Dependent Variable: Post-test Score on Themes 3, 4, and 5 of FFFL-HS Test

Number of Observations = 995 Adjusted R-squared = 0.22

Variable	Degrees Freedom	F-Statistic	Prob > F
Overall Model	12	23.82	0.000
Academic Ability	2	75.19	0.000
Grade Level	3	14.84	0.000
Gender	1	20.84	0.000
Race	5	1.78	0.114
FFFL	1	17.72	0.000

CONTENTS

Voluntary Pollution Control and Local Economic Conditions as Determinants of Environmental Monitoring and Enforcement: Evidence from the 1990s

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Abstract

In this paper I attempt to determine 1) if regulators are responsive to efforts by firms to control unregulated pollutant releases by reducing subsequent inspection and enforcement efforts, and 2) whether or not regulators are sensitive to local economic conditions when making inspection and enforcement decisions. The results strongly suggest that both inspections (state and federal) and enforcement actions are positively impacted by unregulated pollutant releases per unit output. As far as local employment conditions are concerned, I find that fewer enforcement actions are levied against plants that employ relatively more workers as well as plants located in counties with relatively high unemployment rates. However, inspections are largely not sensitive to local employment conditions.

I. Introduction

Despite evidence showing that the monitoring and enforcement (M&E) of environmental law in the United States and abroad is lax (Russell, 1990b)(Cohen, 1998) many studies find that M&E does indeed impact firms' compliance behavior (Laplante and Rilstone, 1996)(Gray and Deily, 1996)(Nadeau, 1997)(Decker and Pope, 2005). To better understand firm compliance with environmental law, it is essential to understand what determines inspection and enforcement behavior.

The focus of this paper is on what influences environmental regulators to conduct costly inspections (labor, equipment, site visit transportation costs, etc.), and to undertake potentially costly enforcement actions (court and other litigation costs). In doing so, my primary concerns are two-fold. First, it has been suggested that regulators are responsive to good-faith efforts put forth by some firms to limit releases of pollutants not currently regulated or to limit releases of pollutants beyond what is required by statute or permit

(Hemphill, 1993/94)(Cothran, 1993) (Maxwell and Decker, 2006).[1] This responsiveness can manifest itself in the form of reduced inspection activity or more lax enforcement (fewer formal enforcement actions and potentially lower-than-average penalties). Building on existing research, I empirically investigate this hypothesis in detail below.

There is some research that has investigated this issue by focusing on inspection activity (Decker, 2005). However, no research exists addressing the potential differences that may arise between inspection behavior and enforcement behavior within the same modeling framework. It is quite possible, for instance, that those regulators responsible for inspecting facilities for environmental compliance respond differently to local economic conditions than do regulators responsible for enforcing environmental regulations.[2] One might then ask, for example, if responsive regulation does occur, does it occur (and to what degree) during the inspections stage or the enforcement stage? Hence, my purpose is to empirically address whether or not investigating inspection and enforcement activities separately using a statute-specific, consistent dataset can shed any light on inspections and enforcement behavior specifically. This is a necessary research direction to pursue because, as is illustrated below, there are certain peculiar inconsistencies that have arisen in the existing empirical literature that have looked at inspections and enforcement.[3]

The remainder of this paper is structured as follows. In Section II, I briefly discuss characteristics and examples of responsive regulation. In Section III, I address key aspects of the existing empirical literature that has looked at inspection and enforcement behavior. In Section IV, I discuss the econometric model and the nature of the data used in the empirical analysis. In Section V, I discuss the econometric methodology employed in this study as well as other technical issues that arise in the data. The empirical results are presented in Section VI and section VII concludes.

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[1] Hemphill writes: "The implementation of corporate environmental audits and the active amelioration of deficiencies is viewed by federal and state law enforcement agencies in a positive light when they investigate for criminal prosecutions and may also be helpful in civil and administrative proceedings." (p. 151).

[2] See Goodstein (2008) for a detailed discussion of the distinction between monitoring and enforcement activities.

[3] While the existing literature is relatively small, there are some key papers that offer a good picture of our current understanding of issues addressed here, including Magat and Viscusi (1990), Helland (1998), and Firestone (2002). See Cohen (1998) for an extensive survey. For the most part, these papers attempt to empirically evaluate various theories of regulation. There are many such theories, the most popular being economic (or positive) theory of regulation (Stigler, 1971)(Noll, 1985) where the regulatory agency seeks to maximize net support from interested stakeholders by imposing the least amount of regulatory burden on those groups that are concentrated and well organized. Therefore, enforcement activity should then be sensitive to local economic conditions such as regional unemployment and income conditions, and importance of the plant to the local economy. A second primary theory of regulation is often referred to as the pubic interest (or normative) theory (Posner, 1974)(Dion, Lanoie, and Laplante, 1998). With respect to environmental regulation, regulators under this view are seen as wishing to allocate its resources so as to minimize overall environmental damages (Noll, 1989)(Posner, 1974). Hence, some noncompliance is acceptable in areas where the harm caused by noncompliance is relatively small. Given this interpretation of regulation, the regulator's monitoring strategy would be influenced by the fact that damages are heterogeneous and would allow higher discharges in locations where damages are smaller.

II. Responsive Regulation

With respect to environmental regulation, the manner in which enforcers interrelate with regulated firms appears to have evolved since the original inception of the legislation. For instance, under the United States Environmental Protection Agency's (EPA) National Environmental Performance Track program, participating firms are required to establish in-house, self-auditing compliance programs, conduct self-audits, institute corrective actions should noncompliance problems be discovered, and submit reports to both the EPA office and the public. In return for these efforts, participating firms are rewarded in a number of ways, one of which is that they will be considered "a lower inspection priority."[4] In a similar light, recent changes to the US EPA's audit practices are increasingly designed to encourage regulated firms to voluntarily discover, disclose, and correct any violations of existing environmental statutes. In exchange for these efforts, the EPA states that it will, among other things, refrain from additional audit requests.[5] Moreover, both Cothran (1993) and Hemphill (1993/94) offer additional examples where state and/or local authorities explicitly take into consideration a firm's environmental record when making regulatory enforcement decisions.

These examples suggest responsive regulation in that firms undertake a costly action to improve compliance and, in response, the regulator relaxes subsequent enforcement. Regulators can respond by reducing subsequent monitoring activity, reducing enforcement activity (either by filing fewer subsequent formal enforcement actions or limiting penalties, etc.), or through other means, such as reducing bureaucratic red tape associated with obtaining permission to build new plants (Maxwell and Decker, 2006). While these few examples suggest responsive regulation, they do not indicate widespread adoption of such practices nor do they indicate how responsiveness manifests itself. I attempt to address this issue empirically.

III. Inspection and Enforcement Literature

As stated above, the second issue I attempt to address is whether or not investigating inspection and enforcement activities separately using a statute-specific, consistent dataset can shed any light on certain inconsistencies that have arisen in the existing empirical literature.

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[4]For further information on the National Environmental Performance Track program, visit Hhttp://www.epa.gov/region1/pr/2000/071000.htmlH.

[5] It is the case that in order for a firm to realize these benefits, firms must document completely its compliance efforts. Firms must establish a systematic management plan to achieve and maintain compliance, prepare and publicize annually a comprehensive compliance report, and engage in third-party verification and review to certify compliance efforts annually. Effective management along these lines is likely to be quite costly for firms seeking enforcement relief under these audit policy stipulations. That said, however, there is evidence that the EPA does follow through on its claim towards reduced enforcement. For instance, in 1997, the EPA's Region V office in Chicago, Illinois, encouraged a number of small steel mills to participate in this new audit policy plan. According to EPA reports, approximately half of those so encouraged undertook the necessary efforts to improve compliance. Those that did not choose to participate were inspected with much greater frequency (see EPA, 1998).

To be sure, there are some fairly consistent results that are quite intuitive. Deily and Gray (1991) and Helland (1998), for instance, find that enforcement is weaker when there is a higher likelihood that a discovered violation, if enforced, will result in a plant closing. Dion, Lanoie, and Laplante (1998) find that inspections are more frequent at production facilities whose pollution by-products are more harmful to the surrounding environment. All studies find that a plant's historical compliance record influences subsequent monitoring activity.

However, there are a number of inconsistencies in these studies as well. For instance, Deily and Gray (1991), in an investigation addressing whether or not there is empirical support for the economic theory of regulation (Stigler, 1971)(Noll, 1985), find that, consistent with theory, iron and steel plants that employ a larger share of the local labor force are monitored less frequently for clean air regulatory compliance, ceteris paribus. Helland (1998) finds a similar result. However, contrary to theory, they find that plants in high unemployment areas are more likely to be inspected.

Yet, using a similar model, Dion, Lanoie, and Laplante (1998) analyze inspection activity for clean water compliance within the Canadian pulp and paper industry. They find that inspections increase with plant employment. Larger plants, they argue, are more visible and therefore an informed public might view inspections here as mitigating potential environmental harm. They address the inconsistency between their results and those of Deily and Gray (1991) by suggesting that the key distinction between the two studies is that Dion, Lanoie, and Laplante (1998) focus only on inspection activity, while Deily and Gray (1991) consider enforcement. They contend that inspections might represent signals to local communities and other stakeholders that regulators are putting forth efforts to safeguard the public from environmental harm (consistent with their results) but are at the same time hesitant to levy costly sanctions on violators for fear of causing economic hardship in a locality (consistent with Deily and Gray).

Of course, these differences can easily arise because each study is focusing attention on different statutes (air vs. water regulation), in different industries (steel vs. pulp and paper), in different jurisdictions (US vs. Canada). Moreover, the rationale Dion, Lanoie, and Laplante (1998) offer seems to encounter some difficulty when considering that Nadeau (1997) finds that larger pulp and paper plants in the United States (measured as plant production capacity) receive more regulatory scrutiny in the form of *both* increased inspection and enforcement activity, although he does not address the employment affect.

IV. The Empirical Model and Data

To address these and other issues, I have developed a dataset consisting of a sample of plants competing in four EPA-designated high priority manufacturing industries; pulp and paper (SIC 26), chemical manufacturing (SIC 28), iron and steel (SIC33), and petroleum refining (SIC 29).[6] This dataset contains information on inspections (both state and federally

[6]The dataset employed here only classified plants based on the two-digit SIC classification. This is potentially limiting in that it prohibits the application of data with greater classification precision which could improve the

conducted) and enforcement actions logged for the US Clean Air Act (CAA). This will facilitate comparisons between inspection and enforcement activity more effectively than comparing different studies that employ different independent variables, different econometric models and analysis of different industries. In addition, unlike much of the existing work, the monitoring activity I focus on will be inspections conducted by both state and federal authorities separately. While it is true that, as pointed out by Russell (1990a) and borne out by the data, that most inspection activity occurs at the state level, the determinants of state inspection activity may differ from those of federal inspection activity. Finally, in the spirit of Nadeau (1997), I separate inspections and enforcement activity to see if there are indeed systematic differences between monitoring and sanctioning.

Two inspection equations are estimated. The dependent variable in the first equation, SICAA, measures the number of plant-level, state-conducted inspections during the period 1997-98. The dependent variable in the second equation, FICAA, measures the number of plant-level, federally-conducted inspections during the same period.[7] Borrowing primarily from Deily and Gray (1991), the estimated equation for each type of inspections is:

$$\begin{aligned}&xICAA_i = f(ENCAA95\_96, ENCAA92\_94, \ln(PROD_i), \ln(EMIT_i / PROD_i),\\&\ln(TRIREL96_i / EMIT_i), \ln(POP3MILES_i), \ln(EMP\_PLANT_i / MANEMPST_s),\\&\ln(COUNEMP_c / STUNEMP_s), \ln(EAIRQUAL_s / NUMPLANTS_s),\\&NONATTAIN_c, \ln(MEDINC_c), e_i),\end{aligned} \tag{1}$$

where x = S for state inspections or F for federal inspections. The subscript "*i*" "indicates plant data, "*s*" indicates state-level data where the plant is located, and "*c*" indicates county data where the plant is located.

The estimated equation explaining the number of enforcement actions levied against plants between 1997 and 1998, ENCAA, is similar to the inspection equations except that it includes as additional variable discussed below.[8] Specifically, enforcement actions will be modeled as follows:

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explanatory power of the models estimated. Future work may benefit from obtaining more detailed industry classification. That said, the results presented here still offer some compelling implication for regulatory behavior.

[7]The two-year cross section was recommended by the EPA's IDEA database management staff and it was in that form that the data was supplied. Since inspections are relatively infrequent, one obtains more non-zero observations and more variation in inspections when looking over a two-year period. This can help make statistical inference more reliable. However, since timing is an important consideration, this can present some difficulties to be sure. There indeed may be variation between 1997 inspections and 1998 inspections that certain model variables may influence. This point is addressed in more detail in the discussion to follow.

[8] Only one enforcement action equation is specified since enforcement actions are not broken down by state and federal actions.

$$\begin{aligned}&\mathrm{ENCAA}_i = f(\mathrm{ENCAA95\_96}, \mathrm{ENCAA92\_94}, \ln(\mathrm{PROD}_i), \ln(\mathrm{EMIT}_i / \mathrm{PROD}_i),\\ &\ln(\mathrm{TRIREL96}_i / \mathrm{EMIT}_i), \ln(\mathrm{POP3MILES}_i), \ln(\mathrm{EMP\_PLANT}_i / \mathrm{MANEMPST}_s),\\ &\ln(\mathrm{COUNEMP}_c / \mathrm{STUNEMP}_s), \ln(\mathrm{EAIRQUAL}_s / \mathrm{NUMPLANTS}_s),\\ &\mathrm{NONATTAIN}_c, \ln(\mathrm{MEDINC}_c), \mathrm{ICAA95\_96}_i, e_i).\end{aligned} \tag{2}$$

Most of the data for this study, notably the inspections and enforcement data, comes from the US EPA's Office of Enforcement and Compliance Assurance (OECA) Integrated Data for Enforcement Analysis (IDEA) data system. These data contain information on plant inspection and enforcement activity, compliance records and penalties assessed for noncompliance. To these data, I add plant-level production, plant-level employment and various state and county-level economic data. Table 1 provides definitions for each of these variables and Table 2 provides descriptive statistics of each variable and the relevant data sources.[9]

The variable PROD measures the physical output or capacity at a particular plant in 1996 and thus controls for plant size.[10] Larger plants are likely to be larger polluters and, therefore, may pose a greater risk to the environment. Hence, one hypothesis would be that larger plants are likely to be inspected more often and enforced more aggressively so the estimated coefficient is expected to be positive. However, larger plants are also likely to benefit from economies of scale. To the extent that increased production efficiency also translates into lower pollutant releases, it is conceivable that inspectors may recognize these efficiency gains and inspect larger plants less frequently (and perhaps enforced less aggressively). In this case, the estimated coefficient would be negative.[11] Finally, if the goal is only to maximize overall compliance with environmental law, then inspections should be independent of plant size. The empirical analysis should support one of these conjectures.

The variable EMIT/PROD measures plant releases of CAA regulated releases (in tons) per ton of output (capacity) of product manufactured in 1996. Consistent with other studies it is hypothesized that historically larger per unit output pollutant releases prompt a higher degree of regulatory attention.[12] The emissions data comes directly from the EPA and the production data was collected from a variety of industry publications (see Table 2).

[9]It is worth pointing out that, consistent with observations made by Russell (1990a) and others, inspection rates are relatively low, averaging about 4 state-conducted and 0.3 federally-conducted visits per two-year period. Consistent with other studies, these statistics confirm that most of the inspection activity undertaken in the US is conducted by state environmental authorities. Moreover, the infrequency of inspections suggests that the underlying data-generating process for SICAA is likely to be discrete in nature. As discussed in detail below, this will have significant ramifications for the econometric specification employed.

[10]Production capacity data for the chemical manufacturing industry is generally available only for select years between 1996 and 1998.

[11]Moreover, under the economic theory of regulation, one would also predict larger plants command more resources and make a larger contribution to the local economy. Therefore, they may be able to "capture" the regulator, in which case the estimated coefficient should be negative.

[12]The subscripts have been dropped for notational convenience.

As an indicator of a plant's compliance record, I use two variables; the plant's historical enforcement actions over the two-year period 1995-1996 (ENCAA95_96) and actions counted over the three-year period 1992-1994 (ENCAA92_94). An enforcement action is any formal administrative or civil action taken against a violating plant. These data do not include criminal sanctions nor do they count informal enforcement actions such as "notices of violations" (NOVs).[13] They simply count any actions that require either a monetary fine, a cleanup requirement, or both. Unfortunately, the data does not distinguish between actions brought against a violating plant by state authorities or federal authorities. Typically, however, enforcement actions involve both state Attorneys General and federal authorities at the EPA or the US Department of Justice.

These two variables are included in equations (1) and (2) to capture "reputation" effects. We would expect that those plants with a greater number of historical enforcement actions to be inspected more frequently or potentially subject to more subsequent enforcement than those plants with fewer enforcement actions recorded. The rationale for breaking up historical noncompliance into two separate variables is to test whether or not regulators are more sensitive to recent noncompliance rather than noncompliance recorded over a longer term.

There may be some concern that these variables are strongly correlated with EMIT/PROD. In fact, the correlations with EMIT/PROD are -0.06 and -0.01 for ENCAA95_96 and ENCAA92_94, respectively. Generally speaking, it would seem advisable to include all three variables in the regressions since it seems reasonable to presume that large polluters would be inspected more frequently and enforced more aggressively, even if those plants have a reasonably good compliance record.

The voluntary pollution efficiency variable designed to test the "regulatory responsiveness" hypothesis is TRIREL96. TRIREL96 is obtained from the EPA measured level (total tons) of the Toxic Release Inventory (TRI) listed chemicals released by the plant in 1996 per ton of plant output or capacity in 1996.[14] Many studies often treat TRI releases as a measure of voluntary pollution control (Konar and Cohen, 2001). With respect to my analysis, the use of the TRI data as a measure of a plant's voluntary environmental commitment is motivated in the following way. Under the TRI program, facilities meeting certain criteria are required to report the total amount of certain chemicals (roughly 650 currently) both released on-site and transferred to other sites, on an annual basis. The data is self-reported and is, therefore, not the result of direct monitoring by environmental

[13]It is possible that an inspection that discovers noncompliance may result in a referral or an informal enforcement action where the plant will be required to correct any discovered problems within a mutually agreed upon time frame. It will not, however, be recorded as an enforcement action against the violating plant. While this suggests that the number of recorded enforcement actions may not be a perfect indicator of environmental non-compliance, informal enforcement actions are simply not observed. Hence, from an empirical perspective, I will assume that the enforcement actions serve as an indicator of the plant's historical proclivity to violate environmental law.

[14]By way of background, the TRI program was established as part of the US EPA's Emergency Planning and Community Right to Know Act. At the time of this writing, data was available from 1987 through 1996. That is, while (truthful) reporting is required, level releases are not generally regulated. While the truthful nature of the data might be subject to some question, it is the case that plants face severe penalties for failure to report and report truthfully. Hence, I will assume that the level of reported TRI release is credible.

enforcement authorities. Moreover, these chemical releases are not, in and of themselves, an indicator of either compliance or violation. Indeed, most of the chemicals reported under the TRI are unregulated, meaning that the actual level of release is not likely to be subject to any federal or state regulatory statute.

It is these "voluntary" characteristics of the TRI data that are potentially useful in testing the "regulatory responsiveness" hypothesis. Direct empirical verification would require, at minimum, plant level expenditures on pollution abatement and, more importantly, a measure of how much such investment was necessary for compliance and how much was in fact voluntary. To my knowledge, such data does not exist. I implicitly assume that a plant's historical environmental investments are ultimately realized through lower TRI releases. Hence, since these investments make the firm more "pollution" efficient, the regulator's subsequent monitoring and enforcement intensity should fall.

Note the importance of timing and commitment here. As suggested by Cothran (1993), Hemphill (1994), and Maxwell and Decker (2006), regulatory responsiveness occurs only when a firm pre-commits to an observable, voluntary (fixed) environmental investment, realized through TRI pollutant releases, prior to the regulator's inspection and enforcement effort. The "regulatory responsiveness" hypothesis will be accepted, then, if the estimated coefficient on TRI releases per unit output is positive. Again, since timing is essential, note that pollution efficiency is as of 1996 and inspections are as of 1997-98. Therefore, the investment made to control TRI releases has already occurred prior to the regulator's inspection activity.[15]

Theoretical issues aside, there does seem to be good reason to believe that enforcement agencies are using the TRI data in an effort to direct enforcement activities. According to the EPA (2003), one of the principle uses of the TRI data by governmental authorities is environmental targeting. Hence, it appears to be the case that environmental regulators do care about TRI releases to the point that they may use it to target regulated plants.

[15]Again, an important issue arises here with respect to the nature of the inspections and enforcement data. Based on theory, it seems reasonable to presume that inspections (enforcement actions) in 1997 are influenced by TRI releases in 1996. However, while it is reasonable to assume that 1998 inspections (enforcement actions) are also sensitive to 1996 TRI releases, they might also be influenced by TRI releases in 1997. The impact such releases might have on 1998 inspections (enforcement actions) is not known. However, if there is sufficient variation in the data, particularly in inspections and enforcement actions, annually over time then the estimated impact that TRI (and indeed other model variables) has on regulatory behavior may over- or under-estimate true effects (I thank an anonymous referee for making this observation). Given the nature of the data and the limited time series available, it is difficult to judge definitively the potential magnitude of the bias. However, while such a possibility certainly exists, what limited time series data is available does not suggest too much variation over time in the data. For instance, state and federal inspections averaged 4 over the 1997-98 period and 3.1 over the 1995-96 period. TRI data for these specific plants was not collected over time. However, according to the US Environmental Protection Agency, for the petroleum, pulp and paper, iron and steel, and chemical industries as a whole, TRI releases increased from roughly 553 million tons to 557 million tons, or about 0.7 percent between 1995 and 1996. While there could be more variation over time at the plant level, the industry numbers suggest less variation in the time dimension. That said, reasonable caution should be exercised in interpreting magnitude effects. Clearly, however, more time-series analysis of these data is an important avenue for future research.

A potentially serious issue arises with the inclusion of both the TRI and the regulated emissions data, EMIT in equations (1) and (2). Although to my knowledge it has not been established with any degree of certainty, it is reasonable to suggest that those plants that emit a large quantity of regulated pollutants per unit output are also likely to produce a large amount of TRI designated chemicals. To address this potential collinearity issue, the TRI data enters the empirical model as the ratio of TRI releases to regulated pollutant emission releases: TRIREL96/EMIT. The "responsive regulation" hypothesis is empirically implemented then by testing whether or not inspections and enforcement actions are responsive to voluntary pollution releases, as measured by the TRI data, which represent a greater proportion of regulated pollutant releases. Hence, the expected effect of TRIREL96/EMIT on inspections and enforcement actions should be positive.

Turning attention to some of the political influence variables as used by Deily and Gray (1991) and Dion, Lanoie, and Laplante (1998), (EMP_PLANT/MANEMPST) measures the plant's total employment relative to the states total manufacturing employees in the state where the plant is located. MANEMPST is total state manufacturing employment for the year 1993 and comes from the Department of Commerce's Regional Economic Information System (REIS). EMP_PLANT measures total employment by plant and comes from Marketing Economics Institute's *Marketing Economics, Key Plants, 1993 edition.*[16] Since most of the remaining data covers more recent years, there may be some difficulties in using this information. However, this variable only enters the empirical model as the ratio of plant employment to state employment. The implicit assumption I am necessarily making, then, is that the share of plant employment in 1993 to state employment in 1993 is constant (at least over the period 1993 through 1996).[17]

We might expect the estimated coefficient to be negative if it is believed that a larger plant that employs a greater share of an area's economy exercises a larger degree of influence on local regulators than do smaller plants. This interpretation is roughly consistent with Stigler's Economic Theory of Regulation. However, a larger plant may be more visible and thus garner more attention than a smaller plant. Indeed, many environmental actions are prompted by disgruntled former employees that may be seeking a type of retribution against the company owning the plant. Hence, we might expect a positive effect under such a scenario.

We might then expect a relatively higher local area unemployment rate, COUNEMP96/STUNEMP96, to negatively impact inspections. Specifically, this variable, obtained from the US Bureau of Labor Statistics, measures county-level unemployment rates relative to state-level unemployment in 1996 and can be thought of in our context as representing the opportunity cost of employment reductions when a plant must either shut down or divert expenditures towards compliance. For instance, in high unemployment counties, relative to the state as a whole, one might expect new job opportunities to be

[16]This appears to be the latest edition.

[17]This obviously may be difficult to support since, if anything, manufacturing's share of total employment has been declining. An alternative way to proceed might be to assume plant level employment remains fixed between 1993 and 1996 and consider the ratio of plant employment in 1993 with state employment in 1996. This was tried but the results were similar to the results presented in this paper.

limited. Thus, a plant layoff, prompted by the levying of a costly enforcement action for instance, can generate more serious economic and social problems here than in other regions where greater job opportunities exist. Therefore, if regulators value the avoidance of such economic disruptions, inspections and enforcement actions may become less prevalent. Of course, it is possible that areas with higher unemployment might represent more traditional, highly industrial areas that, by the nature of the industries located there, are more heavily scrutinized.

The variable MEDINC95 measures median family income by county in 1995 and was obtained from the County and City Databook published by the US Department of Commerce. It is included as a measure of local affluence. The hypothesis here is that areas with higher incomes will have higher demand (and the means necessary to influence local politics) for clean neighborhoods and therefore may be a potential source of political influence.

EAIRQUAL/NUMPLANTS is the total expenditures by states for air quality per total number of manufacturing plants located in that state. EAIRQUAL comes from the Council for State Governments' 1996 publication *Resource Guide to State Environmental Management, 4th ed.* (1996) and measures state expenditures on air quality for fiscal year 1994. NUMPLANTS counts the total number of manufacturing facilities in a state and comes from the 1992 US Census' Census of Manufacturers. Including EAIRQUAL/NUMPLANTS attempts to measure a state's aggressiveness directed towards environmental compliance. The estimated coefficient should be positive.[18]

POP3MILES, made available by the US EPA, measures the population density within a three-mile radius of the plant. If plants are noncompliant in more heavily populated areas, then a greater number of people would be exposed to potentially harmful emissions, etc. One would expect that more inspections would take place in more populated areas; that is, where the risk exposure is higher. Finally, NONATTAIN is a dummy variable that equals 1 if the plant is located in a county that was considered by the EPA to be in nonattainment with National Ambient Air Quality Standards (NAAQS) as of 1998. I expect greater monitoring and enforcement activity in such areas. Therefore, the estimated coefficient should be positive.

In addition to the above variables, I include industry dummy variables (DMYCHEM, DMYPET, and DMYSTL) to capture any potential systematic differences in inspection and enforcement activities between these sectors and the pulp and paper industry (the effect of which is subsumed within the constant term). Additionally, since many of the existing papers have stressed the employment effect on regulation, I interact these dummy variables with ln(EMP_PLANT/MANEMPST) to test whether or not the employment effects in these three sectors are systematically different from the pulp and paper employment effect.[19]

[18]Assuming that state authorities exhaust their budgets, then this term might reasonably be interpreted as a budget limitation on states as well. Note that this variable does not appear in the federal inspections equation.

[19]My decision to include a constant term and omit the pulp and paper dummy variable was arbitrary. I did systematically drop each of the other industry dummy variables but this had little impact of the overall results presented here.

One final issue needs to be addressed. Note that I include an additional variable, ICAA95_96, in the enforcement actions equations. This variable measures the number of inspections conducted by state and federal regulatory authorities in 1995 and 1996. The implication is that some of those inspections may have lead to a discovery of non-compliance, which may ultimately result in a formal enforcement action in the 1997-98 period. As outlined by Goodstein (2008, 293-296) and Russell (1990a, 248-253), there is a complex regulatory process that ensues after an inspection reveals non-compliance. There is an initial drafting of an inspection report by the regulatory body responsible for discovering the violation that specifically outlines the nature of the violation. A "Notice of Violation" is then typically sent to the violating facility which outlines the corrections that need to be undertaken and a time frame (typically more than 30 days) for corrections to be made and reported back to the regulatory body. If continued non-compliance occurs, only then will a formal enforcement action likely be issued. As Russell (1990a, 252) reports, in some instances it can take years for an initial discovery of non-compliance to result in a formal enforcement action being brought (if at all). Hence, the lagged inspections data here should be determinant of subsequent enforcement actions.[20]

V. Econometric Methodology and Issues

In principle, the inspection data could be analyzed using ordinary least squares (OLS) in much the same way as Deily and Gray (1991) proceed. However, as can be seen in Table 2, the low averages and the preponderance of zeros in the inspection variables highlight the discrete nature of the data. This suggests that we could improve on OLS by using a count model that specifically accounts for these characteristics. It is also the case that there are a number of zeros in the dependent variables, particularly when looking at federally conducted inspections and enforcement actions. So two procedures will be adopted here. One treats each dependent variable as a binary variable, equal to one if at any time between 1997 and 1998 at least one state inspection, one federal inspection and one enforcement action was recorded.[21] For these, Probit models are used to estimate equations (1) and (2). These results are presented in Table 4. The second procedure treats each dependent variable as a count, indicating the frequency with which inspections and enforcement actions are recorded over the two-year period. However, with count models, there are some important econometric issues that are worthy of some brief discussion.

The most basic count model utilizes the Poisson density function to perform maximum likelihood estimation of the β coefficients. Typically, when maximum likelihood estimation is performed on count data using the Poisson (or any other) distribution, the

[20]To be sure, it is quite possible that an inspection in, say, 1997, could lead to an enforcement action in 1998, suggesting that more contemporaneous inspection activity may be useful in determining enforcement actions. While this is not unlikely, based on Russell (1990a)) and Goodstein (2008), it's not too likely. Moreover, the determinants of inspections (equation (1)), are also determinants of enforcement actions so some of the behavior explaining inspections will also be explaining enforcement actions. Finally, while evidence from my dataset is limited on this point, there does appear to be some correlation between inspections in 1997-98 and 1995-96. Hence, lagged inspections can serve as a reasonable instrument to employ in the enforcement equation as well.

[21]This is a common procedure used in many studies of environmental inspection activity. Dion, Lanoie, and Laplante (1998), for instance, treat inspections as a binary variable.

independent variables defining the conditional mean of the dependent variable enter the log-likelihood form of the chosen density function in the following way:

$$y = g(y/\mathbf{x}, \beta) = \exp(\mathbf{x}'\beta), \tag{3}$$

where *g(.)* is the function defining the conditional density of y, **x** is a matrix of dependent variables as defined in (1) and β is a vector of estimated coefficients.[22] Therefore, it is readily apparent that the resulting estimated coefficients can be interpreted as semi-elasticities since, for a given independent variable *i*:

$$\frac{\partial y / y}{\partial x_i} = \beta_i \,. \tag{4}$$

Consistent with much of the existing literature, if we wish to obtain elasticities, the variable defining **x** can be incorporated, as I do in equation (1), into the estimation in their logarithmic representation.[23]

The Poisson density function, however, has the defining characteristic that the conditional mean of the outcome is equal to the conditional variance, a characteristic rarely exhibited in applied analysis. It is most often the case that the data is over-dispersed; that is, the conditional variance exceeds the conditional mean. Failure of the equi-dispersion assumption inherent in the Poisson distribution has consequences for the estimated standard errors in the β coefficients similar to those that result when heteroskedasticity is present in standard linear regression models. That is, the estimated variances on the vector of coefficient estimates will be biased estimators of the true variance of these estimated parameters, thus making statistical inference unreliable.[24] Under such a scenario, the Poisson model is usually rejected in favor of the Negative Binomial (NB) regression model whose distributional properties allow for over-dispersion.[25]

There are several ways of testing for over-dispersion. Here, I employ a simple technique suggested by Cameron and Trivedi (1990).[26] Table 3 presents the results of the

[22]See Cameron and Trivedi (1990) and Greene (1993) for details regarding such econometric procedures.

[23]The exception is ENFORCECAA92_96. As can be seen in Table 2, the variable has a very low mean, even over a two-year period and there are a number of zero-enforcement action observations (a finding consistent with data from other studies on environmental enforcement). Therefore, given the high preponderance of zeros, I was hesitant to transform this variable into its log representation.

[24]In fact, Cameron and Trivedi (1998) illustrate that the magnitude of the standard error bias in a count model that fails to correct for over-dispersion can be much larger than a standard regression model that fails to correct for heteroskedasticity.

[25]It can be shown that the Poisson density function is a special case of the NB density (Cameron and Trivedi, 1998).

[26]To carry out this test, I first estimate each equation under the Poisson distribution restriction, i.e. that the conditional mean and variance are equal, and obtain fitted values for the dependent variable $yhat$ (number of inspections). The over-dispersion test is based on an auxiliary OLS regression of the squared residuals minus the actual values of the dependent variable on the squared fitted values of the dependent variable (without a constant): $(y - yhat)^2 - y = \lambda yhat^2 + u$. where $u \sim N(0,1)$. A standard one-tailed *t*-test is conducted on the

over-dispersion tests. In two of the three estimated equations, state conducted and federally conducted inspections, there is evidence of over-dispersion in the data. Hence, I will estimate those equations using the NB density function. Since the enforcement action data did not seem to exhibit over-dispersion, I have chosen to estimate that sector using the Poisson distribution. These results are presented in Table 5.

VI. Empirical Results

Table 4 presents the Probit model results where the reported values measure the marginal effect that each independent variable has on the probability of an inspection (enforcement action) occurring in 1997-98.[27] Table 5 reports the count model results. In terms of model significance, since all of the likelihood ratio (L-R) statistics in Tables 4 and 5 are well in excess of the relevant critical values, we can safely reject the null hypothesis that all the slope coefficients in the estimated equations are jointly zero and therefore conclude that the results are statistically meaningful.[28]

In terms of goodness of fit measures are generally low but in line with similar studies that employ plant-level data.[29] The binary models (Table 4) generally report McFadden - R^2's between 0.08 for federal inspections and 0.22 for enforcement actions. The count models (Table 5) generally report Pseudo - R^2's varying between 0.07 and 0.21, with the federal inspection equations performing the poorest and state inspection equations performing the best.

Focusing attention on state conducted inspections, the probability of a plant being inspected in 1997 or 1998 (Table 4) is positively related to historical noncompliance, both near term and long term. The estimated coefficients on both ENCAA95_96 and ENCAA92_94 are positive and significant at the 10 percent level or better. Also, larger polluters, both in terms of regulated pollutants, ln(EMIT/PROD) and TRI releases. TRIREL96/EMIT has a positive and significant effect on the subsequent probability of a state inspection occurring. Little else seems to impact the probability of a state inspection occurring. However, the results presented in Table 5 suggest that the frequency of state

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estimated coefficient, $yhat$, under the null hypothesis that no over-dispersion exists in the model: $H_0 : \lambda = 0$. The alternative hypothesis is over-dispersion: $H_a : \lambda > 0$. Failure to reject the null indicates a failure to reject the Poisson whereas rejection of the null suggests evidence of over-dispersion in the sample.

[27]The estimated coefficients in a binary model cannot be interpreted as the marginal effect that variable has on the dependent variable. The marginal effect of an independent variable, x_j on the conditional probability of an event is $\frac{\partial E(y \mid x, \beta)}{\partial x_j} = f(x'\beta)\beta_j$, where β_j is the estimated coefficient from the Probit estimation and f(.) is the density function for the normal distribution.

[28]At a significance level of five percent, the critical values from a χ^2 distribution for 16, 17 and 18 degrees of freedom are 26.30, 27.59, and 28.87, respectively. Our L-R statistics are significantly greater than these values. Note that the degrees of freedom may vary between equations because for those equations estimated using a NB, the over-dispersion parameter has to be estimated as well.

[29]A popular measure for nonlinear models, like the Poisson and NB employed here, is the Pseudo - R^2 (Cameron and Windmeiher, 1996). For binary models the McFadden - R^2 is often considered. Each are bound between 0 and 1 and can be interpreted in the same manner in which R^2s from OLS regressions are interpreted.

inspections, while still impacted by (long term) historical noncompliance, and regulated pollutant releases, is also positively impacted by the size of the plant, ln(PROD), and negatively impacted by the relative number of workers employed (ln(EMP_PLANT/MANEMPST)). It also appears to be the case that chemical and iron and steel plants are inspected more frequently by state regulators (relative to pulp and paper mills) and that both chemical and steel plants that employ more workers are inspected more frequently as well (relative to pulp and paper mills).[30] Finally, while TRIREL96/EMIT is not statistically significant at the 10 percent level or better, the estimated effect is positive and the associated p-value is 11.9 percent. Given that TRI releases do impact the probability of inspections (Table 4), it is reasonable to have some confidence that TRI releases are influencing, to some degree, the frequency of state conducted inspections.

As far as federally conducted inspections are concerned, regulators seem particularly interested in long-term historical noncompliance (ln(ENCAA92_94)), facility size (ln(PROD)), regulated pollutant releases (ln(EMIT/PROD)), and TRI releases (TRIREL96/EMIT). From Table 4 we find that the probability of a federally conducted inspection increase with ln(EMIT/PROD), ln(PROD), and TRIREL96/EMIT. Similarly, the frequency of federal inspections (Table 5) increases with long term historical noncompliance (ENCAA92_94), production (PROD), regulated emissions (EMIT/PROD), and unregulated emissions (TRIREL96/EMIT). While ENCAA92_94 is not statistically significant in the Probit model, the associated p-value is 12.3 percent. Given that it is significant in the count model, it is likely the case that the probability of federally conducted inspection does increase with ENCAA92_94. Moreover, federal regulators seem particularly concerned with petroleum refining plants. Both DMYPET and the interactive term DMYPET*ln(EMP_PLANT/MANEMPST) are positive and significant. There's little evidence that federally conducted inspections are systematically different across the other industries studied here and there is no evidence that federal inspectors are sensitive to local economic conditions or pressure groups.

Focusing on enforcement, the results from Table 4 suggest that the probability of an enforcement action being levied in 1997-98 is positively related to the frequency of inspections (both state and federal) conducted during 1995 and 1996. ICAA95_96 has a positive and significant effect (at the 10 percent level) on ENCAA. Moreover, the noncompliance history, both near-term (ENCAA95_96) and long term (ENCAA92_94) seems to increase the probability of subsequent enforcement. Moreover, plants producing more output, more regulated emissions, and those with higher per unit output TRI releases experience a higher probability of subsequent.

As far as the frequency of enforcement is concerned (Table 5), inspections conducted in 1995 and 1996, and historical noncompliance, both near-term and long-term, increase the

[30]This employment impact is somewhat interesting in that it tends to support Deily and Gray's (1991) findings, at least for pulp and paper mills, even though the dependent variable here measures state inspections. Therefore, it may not be the case that inspections are less prone to political influence as Dion, Lanoie, and Laplante (1998) suggest. However, their theory cannot be dispensed with entirely since ln(EMP_PLANT/MANEMPST) is generally not negative and significant in the other inspection equations, nor are the interactive terms negative and significant.

number of subsequent enforcement actions. Moreover, larger producing facilities, and facilities located in nonattainment areas experience a larger number of enforcement actions. Chemical plants appear to be more aggressively enforced since both DMYCHEM and the interactive term, DMYCHEM*(EMP_PLANT/MANEMPST), have a positive and significant impact on the number of enforcement actions levied.

As far as voluntary pollution releases are concerned, it appears that when considering enforcement actions, regulators are sensitive to TRI releases. The results suggest that reductions in such releases of 10 percent will tend to reduce subsequent enforcement actions by about 0.833 percent. While a small effect, it does appear that when it comes to enforcement, regulators are responsive to good-faith efforts on the part of facilities to reduce TRI releases.

It is also interesting that plants that employ more workers and operate in counties where the unemployment rate relative to the rest of the state is higher, tend to experience less aggressive enforcement. Given that these variables are rarely significant in determining inspections patterns (in fact, only plant employment is significant in the state inspection count model equation), this result may suggest that the argument put forth by Dion, Lanoie, and Laplante (1998) may be true. Indeed, inspectors may not be too concerned with conducting inspections at plants that employ more of a local workforce since there is some discretion in the way any discoveries of noncompliance can be handled, such as notices of violations, which carry no sanctions, or informal agreements to correct any problems without levying a penalty. However, when considering an enforcement action, regulators may be pressured to exercise restraint in levying fines against plants that make a larger contribution to the economy.

Finally, consistent with other studies, state expenditures on air quality and median income levels are never significant determinants of inspection or enforcement activities. Local population density (POP3MILES), too is rarely significant. In fact, it is only significant in the enforcement actions count model, and there the estimated coefficient has a counter-intuitive sign. This may indicate that the variable is picking up some other socio-economic effect related to, say, race or some other environmental justice issue, since it is not significant in any other regression, its likely not a major determinant of enforcement.

VII. Conclusion

In this paper I have attempted to determine 1) if regulators are responsive to efforts by firms to control unregulated pollutant releases by reducing subsequent inspection and enforcement efforts, and 2) whether or not regulators are sensitive to local economic conditions when making inspection and enforcement decisions.

The results strongly suggest that both inspections (state and federal) and enforcement actions are positively impacted by TRI releases per unit output. In every equation save one, the coefficient on this variable is positive and significant at the 10 percent level or better. The only equation where TRIREL96/EMIT is not significant is in the state inspections count model, where the p-value is still 11.9 percent.

As far as local employment conditions are concerned, the results aren't nearly as conclusive. However, in general there isn't overwhelming support for the notion that, ceteris paribus, inspections are less likely or frequent at plants that employ relatively more workers (although the number of state conducted inspections appears to be sensitive to plant employment). However, there appears to be some evidence suggesting that, ceteris paribus, fewer enforcement actions are levied against plants that employ relatively more workers as well as plants located in counties with relatively high unemployment rates. As stated above, this result tends to verify Dion, Lanoie, and Laplante's (1998) contention that while inspections may not be sensitive to local employment because a discovery of noncompliance can be dealt with in a variety of ways, regulators may be hesitant to levy costly enforcement actions against those plants that account for a greater proportion of local economic activity. Again, this result was obtained using a consistent data set, focusing on a particular set of industries for a particular environmental law within a particular jurisdiction.

In addition, the findings here support the notion that subsequent inspection and enforcement activity is generally sensitive to long-term historical noncompliance, plant production size, and regulated pollutant releases per unit output. Federal inspectors seem more concerned with petroleum refining plants while they seem to inspect chemical and steel plants more frequently than plants in other sectors.

Additional research might involve a more refined measure of state environmental enforcement budgets that would be necessary to control for budget influences on inspection rates. Additional information on current local air and water quality would enhance the results. Another interesting avenue for future research would be to use an alternative measure, other than TRI release data, to test for regulatory responsiveness. For instance, one approach might be to generate a dummy variable indicating whether or not the firm being inspected has a record of participating in government sponsored voluntary pollution control efforts such as the EPA's 33/50 program of the early 1990s. One would expect under the "regulatory responsiveness" hypothesis that participation should lead to fewer subsequent inspections. Such considerations await future research.

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Table 1:
Variable definitions

Variable Name	Definition
SICAA	Number of inspections for CAA compliance conducted by state environmental authorities in 1997 and 1998.
FICAA	Number of inspections for CAA compliance conducted by federal environmental authorities in 1997 and 1998.
ENCAA	Number of enforcement actions brought against the plant for CAA violations in 1997 and 1998.
ENECAA95_96	Number of enforcement actions brought against the plant for CAA violations between 1995 and 1996.
ENCAA92_94	Number of enforcement actions brought against the plant for CAA violations between 1992 and 1994.
ICAA95_96	Number of inspections for CAA compliance conducted by state and federal environmental authorities in 1995 and 1996.
EMIT	Total air releases (in tons) of Clean Air Act Regulated pollutants in 1996. (1)
TRIREL96	Total TRI chemical releases and transfers (in tons) in 1996.
PROD	Total production or capacity (in tons) in 1996. (2)
POP3MILES	Number of residents living within a 3-mile radius of the plant.
EMP_PLANT	Number employed by the plant in 1993.
MANEMPST	Total number of manufacturing jobs in the state where the plant is located in 1993.
EAIRQUAL	State level expenditures (measured in $'s) on air quality.
COUNEMP	County unemployment rate in 1996.
STUNEMP	State unemployment rate in 1996.
NUMPLANTS	Total number of manufacturing plants in the state where the plant is located in1992.
MEDINC95	County median family income (measured in $) in 1995.
NONATTAIN	Dummy variable equaling 1 if the facility is located in a county that is not meeting current ambient air quality standards.

(1) - Pollutants include carbon monoxide (CO), nitrogen oxides (NOx), sulfur dioxide (SO2), volatile organic compounds (VOCs), ammonia (NH3), and particulates.

(2) - For the chemical manufacturing sector, production data varied somewhat by year. See Table 2.

Table 2:
Summary statistics

sample size: 412	mean	standard deviation
SICAA (1)	3.7621	3.9011
FICAA (1)	0.3252	0.7155
ENCAA (1)	1.0073	3.0347
ENCAA95_96 (1)	0.8859	3.8264
ENCAA92_94 (1)	0.6505	1.5766
ICAA95_96 (1)	3.0878	5.8628
EMIT/PROD (2)	0.0123	0.0230
TRIREL96/PROD (3)	0.0035	0.0123
PROD (4)	1,166,266.0000	2,378,940.0000
EMP_PLANT/MANEMPST (5), (6)	0.0020	0.0050
COUNEMP/STUNEMP (7)	1.1639	0.4008
POP3MILES (1)	24,382.0600	46,113.4000
MEDINC95 (8)	33,874.0600	7,013.3700
EAIRQUAL/NUMPLANTS (9)	2,109.1800	2,209.8300
NONATTAIN (10)	0.3374	0.4734

(1) - EPA's Office of Enforcement and Compliance Assurance (OECA).

(2) - US Environmental Protection Agency (EPA): http://www.epa.gov/air/data/netemis.html.

(3) - Toxic Release Inventory Data (for 1996): http://www.epa.gov/tri/.

(4) - For steel production, the data source is *Iron and Steel Directory; Steel Manufacturers Association Membership Directory*. Units are short tons per year. For Pulp Manufacturing, the data source is the *Lockwood-Post Directory*. Units were converted by author from short tons per day to short tons per year by multiplying production by 360. For Chemical Manufacturing, the data source is www.ChemExpo.com. Units are in short tons per year
Note that production capacity data here generally available for select years between 1996 and 1998.
The year varies from plant to plant.
For Petroleum Refining, the data source is the US Department of Energy, Energy Information Administration.
Units are million barrels per year.

(5) - Employment data comes from *Marketing Economics: Key Plants*, Marketing Economics, Ltd. 1993.

(6) - The Bureau of Economic Analysis Regional Economic Information Service (REIS): http://www.bea.doc.gov/bea/regional/reis/.

(7) - US Department of Labor, Bureau of Labor Statistics.

(8) - County and City Data Book, US Department of Commerce.

(9) - EAIRQUAL came from *Resource Guide to State Environmental Management*, Council of State Governments, 1996
NUMPLANTS came from the US Census' 1992 *Census of Manufactures: http://www.census.gov/epcd/www/92result.html*

(10) - Taken from the EPA' Greenbook online at http://www.epa.gov/oar/oaqps/greenbk/.

Table 3:
Results of over-dispersion test

Dependent Variable: $(y\text{-}yhat)^2\text{-}y$		State Inspections	Federal Inspections	Enforcement Actions
$yhat^2$	*coeff.*	1.1140 ***	0.4922 ***	-0.0052
	std. error	(0.0884)	(0.1191)	(0.0040)

* - Significance at the 10% level. ** - Significance at the 5% level. *** - Significance at the 1% level.

Table 4:
Probit Model Results (marginal effects reported)

	State Inspections: SICAA	Federal Inspections: FICAA	Enforcement Actions: ENCAA
CONSTANT	0.663	0.082	1.416
	(6.515)	(5.414)	(5.318)
ICAA95_96	-------	-------	0.009 *
	-------	-------	(0.015)
ENCAA95_96	0.027 *	-0.009	0.042 **
	(0.164)	(0.030)	(0.048)
ENCAA92_94	0.073 ***	0.021	0.082 ***
	(0.268)	(0.047)	(0.068)
ln(EMIT/PROD)	0.013 ***	0.029 **	0.024 *
	(0.042)	(0.042)	(0.040)
ln(PROD)	0.001	0.047 ***	0.125 **
	(0.071)	(0.061)	(0.059)
ln(TRIREL96/EMIT)	0.005 **	0.015 *	0.020 *
	(0.030)	(0.032)	(0.030)
ln(EMP_PLANT/MANEMPST)	-0.010	-0.017	0.020
	(0.147)	(0.116)	(0.107)
ln(COUNEMP1996/STUNEMP1996)	0.011	-0.001	-0.062
	(0.357)	(0.295)	(0.288)
ln(POP3MILES)	-0.005	-0.106	-0.015
	(0.049)	(0.502)	(0.043)
ln(EAIRQUAL/NUMPLANTS)	-0.010	-------	-0.039
	(0.120)	-------	(0.080)
ln(MEDINC95)	-0.041	-0.106	-0.161
	(0.596)	(0.502)	(0.491)
NONATTAIN	-0.032	0.033	0.133 *
	(0.221)	(0.186)	(0.185)
DMYCHEM	0.071	0.255	0.782
	(1.242)	(1.380)	(1.333)
DMYPET	-0.181	1.112 *	0.814
	(1.733)	(1.587)	(1.653)
DMYSTL	0.167	0.245	0.077
	(1.936)	(1.426)	(1.365)
DMYCHEM*ln(EMP_PLANT/MANEMPST)	0.013	-0.003	0.094 *
	(0.174)	(0.150)	(0.141)
DMYPET*ln(EMP_PLANT/MANEMPST)	-0.026	0.105 **	0.065
	(0.237)	(0.184)	(0.185)
DMYSTL*ln(EMP_PLANT/MANEMPST)	0.020	-0.043	-0.010
	(0.264)	(0.196)	(0.191)
L-R Statistic	69.055 ***	38.346 ***	107.351 ***
d.f.	17	16	18
McFadden - R^2	0.208	0.086	0.202

Marginal effects reported.
Standard errors shown in parentheses.
* - Significant at the 10 percent level. ** - Significant at the 5 percent level. *** - Significant at the 1 percent level.

Table 5:
Count Model Results

	State Inspections: SICAA – Negative Binomial	Federal Inspections: FICAA – Negative Binomial	Enforcement Actions: ENCAA – Poisson
CONSTANT	0.489	-9.294	-1.940
	(2.905)	(7.832)	(4.400)
ICAA95_96	-------	-------	0.021 ***
	-------	-------	(0.007)
ENCAA95_96	0.001	-0.071	0.036 ***
	(0.009)	(0.052)	(0.005)
ENCAA92_94	0.075 ***	0.114 *	0.195 ***
	(0.023)	(0.059)	(0.017)
ln(EMIT/PROD)	0.117 ***	0.152 **	0.071 *
	(0.021)	(0.066)	(0.037)
ln(PROD)	0.147 ***	0.328 ***	0.187 ***
	(0.031)	(0.092)	(0.047)
ln(TRIREL96/EMIT)	0.024	0.105 **	0.083 **
	(0.015)	(0.052)	(0.030)
ln(EMP_PLANT/MANEMPST)	-0.146 ***	-0.253	-0.235 *
	(0.056)	(0.190)	(0.123)
ln(COUNEMP1996/STUNEMP1996)	0.085	0.113	-0.442 *
	(0.156)	(0.440)	(0.247)
ln(POP3MILES)	-0.014	-0.026	-0.059 ***
	(0.024)	(0.059)	(0.033)
ln(EAIRQUAL/NUMPLANTS)	0.038	-------	0.058
	(0.045)	-------	(0.091)
ln(MEDINC95)	-0.155	0.270	-0.236
	(0.271)	(0.725)	(0.399)
NONATTAIN	-0.128	-0.030	0.461 ***
	(0.100)	(0.267)	(0.158)
DMYCHEM	1.421 ***	1.590	4.420 ***
	(0.525)	(2.077)	(0.998)
DMYPET	0.450	4.037 *	0.863
	(0.719)	(2.198)	(1.045)
DMYSTL	2.543 ***	0.384	0.605
	(0.708)	(2.202)	(1.513)
DMYCHEM*ln(EMP_PLANT/MANEMPST)	0.208 ***	0.165	0.646 ***
	(0.074)	(0.244)	(0.143)
DMYPET*ln(EMP_PLANT/MANEMPST)	0.058	0.450 *	-0.004
	(0.096)	(0.267)	(0.139)
DMYSTL*ln(EMP_PLANT/MANEMPST)	0.308 ***	-0.097	0.122
	(0.099)	(0.299)	(0.205)
L-R Statistic	503.290 ***	47.185 ***	847.898 ***
d.f.	17	16	18
Pseudo- R^2	0.195	0.098	0.674

Standard errors shown in parentheses.
* - Significant at the 10 percent level. ** - Significant at the 5 percent level. *** - Significant at the 1 percent level.

CONTENTS

Outracing the Market: A NASCAR Portfolio as a Test Case of Returns and Diversification

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Abstract

This paper examines an equity portfolio comprised of publicly traded firms that serve as the primary sponsor of a NASCAR race team to determine whether such a "specialty fund" could diversify risk as effectively as a more carefully chosen portfolio. We calculate risk adjusted return measures and find that the NASCAR portfolio consistently outperforms market benchmarks. We also find that over longer time periods (greater than three years) the constructed portfolio exhibits lower risk than a market benchmark. We contend that NASCAR sponsorship may serve as a signal to the market of a firm's financial health.

I. Introduction

While investors develop portfolios with a few primary objectives in mind (namely to reduce unsystematic risk and/or to enhance portfolio returns) there are often underlying objectives of secondary importance. As an example, socially responsible funds attract investors who seek to align their personal investment strategies with their religious, social, or political beliefs. The funds have become extremely popular since the first such fund was introduced in 1971. In addition, there are many other "specialty" funds that invest solely in sectors, such as multimedia, energy, financial, healthcare, leisure industry, life science, etc.

In this paper we developed a specialty fund comprised only of firms that serve as a primary sponsor for cars in the top racing series of the National Association of Stock Car Racing (NASCAR), what is now known as the Sprint Cup Series. NASCAR popularity has skyrocketed in recent years and the sport enjoys tremendous fan support and loyalty. We compare the risk-adjusted return performance of this portfolio with that of more established benchmarks. This paper has broad importance and practical significance in that investors may be better able to earn higher risk-adjusted returns by including this specialty fund in their asset allocation strategy. This paper will also provide insight into whether targeted diversification (by investing in a range of firms that have some common denominator) can be effective in reducing portfolio risk.

Over the last several years NASCAR sanctioned auto racing has become one of the most popular spectator sports in the United States. This is particularly true of race events at the body's top level, Sprint (formerly Winston, formerly Nextel) Cup. NASCAR is a sponsor-driven sport with the cars, drivers, and crew adorned with the colors and logos of a number of sponsors. Fortune Magazine reported that for 2004, NASCAR had sponsorship revenue of $1.5 billion, more than the National Football League and Major League Baseball combined (O'Keefe, 2005). In addition to monetary investment by the automobile companies, sponsors are drawn from a wide range of products including alcohol (Budweiser, Miller Lite and Coors Lite are long-time sponsors), home and consumer products (Tide, M&M's, Office Depot) as well as building supplies (Home Depot, Lowe's and DeWalt).

This paper seeks to answer three portfolio related questions using financial data from firms who sponsor NASCAR race cars. First, is it possible to build a simple investment portfolio of publicly traded companies who invest in sponsoring NASCAR race cars and outperform established benchmarks on a risk-adjusted basis? Second, is it possible for the NASCAR portfolio to diversify risk as effectively as a more broad selection of stocks? Finally, does full vs. partial sponsorship lead to differences in excess returns? We hope to use the answers to these three questions to provide insight into whether sponsorship serves as a signal for strong companies.

II. Sponsorship Basics

There are many ways for a company to be involved in NASCAR racing. In this paper we focus on those companies who have chosen to be the primary sponsor of a race car at some point during a NASCAR Sprint Cup racing season. An online article posted on Jeff Gordon's official website provides a user friendly overview of sponsorship (Jeff Gordon online, 2005). The cost for the primary sponsor position on a car, which provides space on both rear quarter panels of the car, hood, team transporter, and team uniforms, ranges between $8 million and $21 million per year. Primary sponsors also typically pay for signage at the track as well as hospitality and other related costs, some of which may double sponsor financial involvement (O'Keefe et al. 2005).

Even at these costs, firms are eager to contribute. Part of this can be attributed to the unique role sponsors have in racing. Unlike most televised sports where sponsor messages are secondary to the telecast of the event, the telecast of the race provides air time for the sponsors. Each time a car is shown on television the sponsors receive on-air exposure. One return on a sponsor's investment is this "free" television exposure. Joyce Julius and Associates, Inc. estimated that Lowe's received nearly $20 million of in-broadcast exposure during the 2006 Daytona 500 won by Jimmie Johnson, who drives the car sponsored by Lowe's (Joyce Julius and Associates, Inc., 2006). An estimated $11.6 million of this came from the display of the primary sponsor logo on the hood of the car. On average, primary sponsors received $1.4 million in television broadcast race exposure for each of the 36 races in the 2005 season. Additionally, merchandise sold to the fans typically also includes the sponsor's name and colors as part of the merchandise.

Market research has also shown that NASCAR fans are quite loyal to the brands that sponsor their favorite driver. Prior to their entry into NASCAR sponsorship, Office Depot found that "forty four percent of NASCAR fans who shopped at a competitor would switch to Office Depot" as a result of their sponsorship of a car (Daniel, 2006). O'Keefe et al. reported that Home Depot saw a double-digit increase in ladder sales after offering a 10% discount to anyone who brought in an ad featuring Tony Stewart climbing the fence at the Daytona Motor Speedway after a July 2005 victory at the track. (The ad copy read "Hey Tony, we have ladders").

That advertising dollars translate into economic benefits for firm shareholders is well documented. Reilly, McGann, and Marquardt find a positive relationship between substantial advertising expenditures and the relative wealth position of the firm's owners (Reilly, McGann, and Marquardt, 1977). Schonfeld and Boyd report that corporate advertising has a positive and statistically significant effect on stock prices (Schonfeld and Boyd, 1982). They find that it is advertising that affects stock prices, not vice versa. Further, their results are robust and are consistent over two different time periods.

Ben-Zion used a regression framework to highlight the effect of advertising dollars on returns to shareholders (Ben-Zion, 1978). He regressed advertising and promotions dollars on current stock price. He concluded that the estimated coefficient represents the present value of future cash inflows attributed to this period's advertising and promotion dollars. Erickson and Jacobson propose an information asymmetry argument (Erickson and Jacobson, 1992). They suggest that increases in a firm's advertising and promotions budget may send a positive signal to the market that the firm has discretionary cash flows available for such expenditures.

Other studies have employed an event student methodology to document capital market reactions that result from specific marking events such as slogan changes, brand introductions, and celebrity endorsements (Agrawal and Kamakura 1995)(Conchar, Kinkhan, and Bodkin 2003)(Kim and Morris 2003)(Mathur and Mathur 1995, 1996, 2000)(Mathur, Mathur, and Rangan 1997)(Lane and Jacobson 1995). These and other studies document the positive relationship that exists between levels of adverting and promotional spending and the market value of the firm. Marketing activities (specifically advertising and promotions spending) are generally expected to deliver future positive cash flows and result in increases in shareholder wealth.

III. Methodology and Data

To carry out this analysis we examine several equally-weighted portfolios consisting of equity from all publicly traded firms who sponsored cars at the Sprint Cup level of NASCAR in the years 2000-2005, regardless of the level or amount of sponsorship. (During the time period under consideration the Sprint Cup Series was known as the Winston Cup Series). The investment strategy in each portfolio is to purchase and hold equity in the firms who are the primary sponsor in at least one race during the purchase year. Our analysis consists of six, five, four, three, two, and one-year holding periods. For multi-year holding periods the portfolios were re-balanced each year (by dropping and adding firms) to include only those firms active in sponsorship for the new season. Thus, the two year holding period (2004-2005) includes all of the firms active in sponsorship for the 2004 season, and all of the firms active in sponsorship for the 2005 season. Specifically, if a firm was active in sponsorship for both years, then its return was used in the calculation of the portfolio return for both years. If a firm sponsored races in 2004 but did not continue to do so in 2005, then the firm's return was used to calculate the portfolio return for 2004 only. If the firm did not sponsor races in 2004 but did so in 2005, then its return was used to calculate the portfolio return for 2005 only. In most cases, firms continue to sponsor cars year after year. However, our portfolio construction ensures that firms who drop out of sponsorship are not erroneously included in risk and return measures for multi-year holding periods. For each portfolio, we calculate risk-adjusted return measures. We will examine the monthly returns of holding this portfolio from purchase on the first trading day of the month until the last trading day of the month. The risk and annualized return results of these portfolios are compared to results from larger equity index measures. We ignore transactions costs in the computation of rates of return.

The data consist of stock price data collected for all of the publicly traded companies which served as the primary sponsor of a NASCAR Sprint Cup car during the period 2000-2005. The data on NASCAR sponsorship details on a race-by-race basis was taken from race information at www.racing-reference.info. Returns were calculated using the adjusted share prices for a given company. The historical prices used were the monthly adjusted closing prices provided by Commodity Systems Inc. and reported by finance.yahoo.com, which represented the closing price for a particular company on the last day of every month, specifically adjusted for dividends and splits.

IV. Results

Monthly returns were calculated for each firm that sponsored at least one car (for any number of races) during the racing season. The number of firms comprising the sample for each period of observation are reported in Table 1.

For each period, the compound annual return was calculated, as well as the portfolio standard deviation and beta. Summary statistics may be found in Table 2.

The results indicate that the NASCAR portfolio consistently earned higher returns than the S&P 500. Additionally for the two longest holding periods, the NASCAR portfolio had a lower standard deviation than did the S&P 500. For shorter periods, the S&P 500 had a much lower standard deviation. Additionally portfolio beta suggests that for longer holding periods the NASCAR portfolio is less volatile than the market. It is reasonable that a small sample of firms would be more volatile in the short run than a larger market basket. There was a statistically significant difference in the returns between the portfolio for both the 2000-2005 period and the 2001-2005 period regardless of how standard deviations were calculated. When the monthly standard deviations were annualized the 2002-2005 and the

2003-2005 periods also saw a statistically significant difference between the NASCAR portfolio and the S&P 500. It is not surprising that t-statistics became smaller the shorter the time period under consideration.

To assess the risk-adjusted performance of the two portfolios three widely-recognized measures were calculated. They include the Sharpe ratio, Treynor measure, and alpha (Sharpe, 1966)(Treynor, 1965)(Jensen, 1969). Investors and financial advisors find these tools to be useful when ranking portfolios in terms of their risk-adjusted performance.

The Sharpe measure is the ratio of excess portfolio return divided by the portfolio standard deviation. It is a relative measure of risk-adjusted performance:

$$S = \frac{R_P - R_f}{\sigma_P} \tag{1}$$

In this measure, R_p is the return from our NASCAR portfolio while R_f represents the risk-free rate of return. We use the return on a 90-day Treasury bill as our measure of risk-free returns. The σ_p in the denominator is the standard deviation of the NASCAR portfolio.

The Treynor measure is also a relative measure of risk-adjusted performance. The numerator is identical to that of the Sharpe ratio, that is, portfolio return in excess of the risk-free rate of return. The denominator, however, is the portfolio beta coefficient:

$$T = \frac{R_P - R_f}{\beta_P} \tag{2}$$

The difference in the two performance measures, therefore, is that the Sharpe ratio adjusts for total risk (measured by standard deviation) while the Treynor measure adjusts for market risk only (measured by beta).

Jensen's alpha is an absolute measure of risk-adjusted performance. Alpha is estimated through a regression of excess portfolio return on excess market returns:

$$ER_{pt} = \propto_p + \beta_p ER_{mt} + \varepsilon_{pt} \tag{3}$$

where ER_{pt} is the excess portfolio return (this is the return on the portfolio in month t minus the risk-free rate during month t); β_p is the portfolio beta, ER_{mt} is the excess return on the market portfolio during month t, ε_{pt} is the residual term during month t, and α_p is the risk-adjusted excess return earned over the time period.

Following Reilly and Norton, we also computed another performance measurement tool that is a variation of the traditional Sharpe ratio (Reilly and Norton, 2003). The New Sharpe ratio examines the differential returns of a portfolio against its benchmark.

$$\bar{S} = \frac{R_P - R_{mt}}{\sigma_D} \tag{4}$$

where R_{pt} is the portfolio return in month t, R_{mt} is the return on the market portfolio during month t, and σ_D is the standard deviation of the differential return over the time period. Like Jensen's alpha, this is an absolute measure of risk-adjusted performance. This measure allows for direct comparison against a benchmark portfolio. They differ, however, in that Jensen's alpha adjusts for systematic risk while the new Sharpe ratio adjusts for total risk.

Table 3 reports the risk-adjusted performance measures for the six periods observed.

Both of our relative measures indicate that the NASCAR portfolio outperformed our market benchmark in all six periods of analysis. Our absolute measures of performance also indicate that on a risk adjusted excess return basis the NASCAR portfolio typically outperforms the market portfolio. It should be noted that for the three shortest portfolio periods Jensen's Alpha was not statistically different from zero.

Over the time period of our analysis some firms (such as Budweiser) consistently sponsored a car. Others engaged in a partial sponsorship plan in which they shared the primary sponsor role with other companies. Office Depot's previously mentioned sponsorship of Carl Edwards would be an example. In addition, some firms made very brief appearances sponsoring cars on a very infrequent, inconsistent basis. We would like to know whether excess returns to sponsoring firms are related to the decision to sponsor a car at all, or whether full season sponsorship is required to see excess returns. To facilitate the comparison, the following regression was estimated on the periods of observation:

$$R_t^* = \alpha + \alpha^* D_{st} + \beta R_{mt} + \delta_i Year_i + e_t \tag{5}$$

where

R^*_t = the stacked vector of company excess returns (return in excess of the risk-free rate)
D_{st} = shift dummy variable that takes on a value of 0 if the firm sponsored all races, 1 if the firm sponsored less than 100% of the races
R_{mt} = excess market returns (market return minus the risk-free rate)
α^* = the shift in the estimate of excess returns due to the firm sponsoring less than all of the races
β^* = a measure of the firm's systematic risk
$Year_i$ = a dummy variable for the year i where i ranges from 2000 to 2004.

In the data 1,968 observations come from firms which sponsored all the races while ninety six (96) observations are from firms which engaged in partial sponsorship. The results from this regression are reported in Tables 4 and 5.

The coefficient on the dummy variable for partial sponsorship is positive and statistically significant and indicates that firms which sponsor a team for some, but not all, of the races in a season have excess returns 1.98 percentage points higher than those firms that sponsor a car for the entire season. As expected, the coefficient for excess market returns is positive and statistically significant. This outcome supports the market model that argues that the returns on a security are linearly related to the returns on a market portfolio.

We have included dummy variables for each year, omitting 2005, to control for any year specific effects on return levels. With the exception of 2002 none of these dummy variables are statistically significant.

These results suggest that firms who sponsor a car for less than a full season earn a higher excess return than those firms who sponsor a car for the entire season. This suggests that excess returns are a pre-cursor to a firm sponsoring a NASCAR car. This would lend credence to the idea that firms that enter into race sponsorship agreements are in fact strong firms relative to others in the market and that sponsorship may serve as a signal of a firm's financial health.

To test whether these results are affected by the racing season we also estimated the model using a dummy variable for observations in the months of December and January when there are no NASCAR races. [1] This dummy variable was not statistically significant and its inclusion did not qualitatively change the coefficient estimates presented above. Additionally we estimated a model containing a dummy variable indicating that the sponsors car finished in the top five in points in the previous year. The coefficient on this dummy variable was also not statistically significant.

V. Conclusion

With this paper we hoped to answer three portfolio related questions. First, is it possible to build a simple investment portfolio which will outperform established benchmarks on a risk-adjusted basis? Our analysis using relative and absolute risk performance measures suggests the answer to this question is yes. The NASCAR portfolio outperformed the S&P 500 in all periods using relative measures, and nine of twelve cases using absolute measures.

Second, can the NASCAR portfolio diversify risk as effectively as a more broad selection of stocks. Here our results were mixed. The NASCAR portfolio has a portfolio beta of less than one and a lower standard deviation than the S&P 500 for the 2000-2005 and the 2001-2005 time periods. For shorter time periods the portfolio beta is greater than one, and the portfolio standard deviation is higher than that for the S&P 500.

These are important results as the equity which makes up our NASCAR portfolio was not chosen based on careful financial analysis, but instead because of their participation in race sponsorship. That a portfolio constructed in this way can lower risk beyond the market benchmark over relatively short (three years and beyond) periods is a quite interesting finding.

Finally, we wanted to know whether NASCAR sponsorship was a signal for excess returns. Our results may also suggest that sponsorship serves as a signal of high performing firms. This would suggest that firms that self-select into NASCAR sponsorship do so in part

[1] In 2001 the season began on February 11 and continued to November 23.

because of the potential gains in customer loyalty, but that these firms were likely already in solid financial shape prior to entering into sponsorship agreements. As a result, higher risk adjusted returns are not likely caused by sponsorship, but sponsorship signals firms that are already likely to earn higher returns. The result that even brief sponsorship leads to excess returns would lend credence to this view.

Table 1:

Number of Public Companies as Primary Car Sponsors in Each Portfolio

Year	Number
2000-2005	26
2001-2005	32
2002-2005	30
2003-2005	29
2004-2005	27
2005	28

Table 2:
Risk and Return Measures: NASCAR Portfolio vs. S&P 500

Period	Compound Annual Return		Standard Deviation		NASCAR
	NASCAR	S&P 500	NASCAR	S&P 500	Portfolio Beta
2000-2005	0.0793 *	-0.0268 **	0.370	0.372	0.826
2001-2005	0.0945 *	-0.0112**	0.330	0.333	0.878
2002-2005	0.1079 *	0.0211	0.300	0.270	1.015
2003-2005	0.1696 *	0.1237	0.218	0.158	1.281
2004-2005	0.0879	0.0595	0.145	0.105	1.212
2005	0.0301	0.0300	0.124	0.078	1.475

Note:
The * indicates that the difference between the NASCAR return and the S&P 500 return is statistically different from zero at the 10% level (two-tailed) using annualized standard deviations.

The ** indicates that the difference between the NASCAR return and the S&P 500 return is statistically different from zero at the 10% level (two-tailed) using normalized standard deviations.

Table 3:
Risk-adjusted Return Measures, NASCAR Portfolio and S&P 500

	Sharpe Ratio		**Treynor Ratio**		**Jensen's Alpha**	**New Sharpe**
Period	**NASCAR**	**S&P 500**	**NASCAR**	**S&P 500**	**NASCAR**	**NASCAR**
2000-2005	0.1413	-0.1448	0.0632	-0.0539	0.008007	0.040
2001-2005	0.2229	-0.0965	0.0837	-0.0321	0.008239	0.054
2002-2005	0.3008	0.0124	0.0888	0.0033	0.006976	0.058
2003-2005	0.6931	0.6665	0.1180	0.1053	0.001256*	0.040
2004-2005	0.4497	0.3527	0.0539	0.0370	0.001722*	0.033
2005	-0.0111	-0.0185	-0.0009	-0.0014	0.000302*	0.006

*Alphas not significantly different from zero for the periods 2003-2005, 2004-2005,2005

Table 4:
Regression Statistics for Excess Returns Model

Regression Statistics	
Multiple R	0.397
R Square	0.157
Adjusted R Square	0.154
Standard Error	0.085
Observations	2,064

Table 5:
Regression Results for Excess Returns Model

	Coefficients	Standard Error	t Stat	P-value
Intercept	-0.0003	0.0046	-0.07	0.944
Partial Sponsor Dummy	0.0198	0.0093	2.13	0.033
Excess Market Returns	0.8387	0.0448	19.69	0.000
Year 2000	0.0070	0.0068	1.04	0.300
Year 2001	0.0094	0.0065	1.44	0.148
Year 2002	0.0131	0.0066	1.99	0.046
Year 2003	0.0096	0.0066	1.45	0.147
Year 2004	0.0058	0.0067	0.87	0.386

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CONTENTS

Does Income Inequality Affect School Enrollment?

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Abstract

In this paper, panel data are used to study whether income inequality leads to lower school enrollment (primary, secondary and tertiary). The answer to this question is mixed and depends on the level of schooling and on whether we are looking at cross-country differences or variation over time. The main findings can be summarized as follows: (1) Countries with higher income inequality have lower enrollment in secondary and tertiary schools. (2) There is little evidence that changes in income inequality within a country lead to changes in secondary or tertiary enrollment. (3) In cross-country regressions, Net Primary Enrollment is not correlated with income inequality. Gross Primary Enrollment is positively correlated with income inequality. (4) There is some evidence that changes in income inequality within a country might lead to lower primary-school enrollment (both gross and net). (5) Other variables that were found to be statistically significant are proportion of urban population and per capita consumption expenditure. Public expenditure on schools appears to have little effect on enrollment. The only exception is tertiary enrollment, which is found to be negatively associated with government expenditure on schooling.

I. Introduction

Economic development literature is abundant with papers studying the effects of income and wealth inequality on growth and vice versa. Most empirical papers document that income inequality is negatively and robustly correlated with growth and that there is an inverted-U relationship between per capita income and income distribution. Theoretical research in this area has attempted to pinpoint the channels through which inequality and growth affect each other. Proposed explanations range from credit constraints to political instability, but the main focus is on inequality as an impediment to investment in human and physical capital.

This paper contributes to the development literature by further exploring the link between income inequality and school enrollment, which is viewed as one of the main channels of investment in human capital. In this paper, panel data on school enrollment (primary, secondary and tertiary) are used to study whether income inequality leads to lower school enrollment as suggested by theoretical models. The answer to this question is mixed and depends on the level of schooling and on whether we are looking at cross-country differences or variation over time.

The main findings can be summarized as follows: (1) There is strong evidence from cross-country regressions that countries with higher inequality have lower enrollment in secondary and tertiary schools. (2) There is little to no evidence that, over time, changes in income inequality within a country lead to immediate changes in secondary or tertiary enrollment. (3) In cross-country regressions, Net Primary Enrollment (the number of primary-school-age pupils divided by the number of all children of primary-school age) is not correlated with income inequality. Gross Primary Enrollment (the number of all students enrolled in primary schools divided by the number of all children of primary-school age) is *positively* correlated with income inequality. This seemingly counterintuitive finding can be explained by the fact that non-OECD countries often report Gross Primary Enrollment in excess of 100%. Non-OECD countries also tend to have greater income inequality and lower incomes. Thus, income inequality seems to delay enrollment in primary schools but does not necessarily prevent pupils from attaining primary education. (4) There is some evidence that changes in income inequality within a country might lead to lower primary-school enrollment (both gross and net). However, this negative correlation between enrollment and inequality can be offset by increasing public spending on primary schools. (5) Other variables that were found to be statistically significant are proportion of urban population and per capita consumption expenditure. Surprisingly, public expenditure on schools appears to have little effect on primary or secondary enrollment. Tertiary enrollment is found to be *negatively* correlated with public expenditure on higher education -- but only in panel regressions.

The paper is structured in the following way: The first section contains a short review of the literature exploring the effects of income distribution on the process of human capital accumulation. Second section outlines the model linking school enrollment with income inequality and describes how the model is estimated. In the third section, empirical results for the model are presented. Concluding remarks summarize the findings presented in the paper and discuss some of the policy implications of the results.

II. Review of the Literature

Most of the theoretical models on this topic treat the process of human capital accumulation much like the process of physical capital accumulation: Individuals are endowed at birth with a certain amount of human capital (whose level can vary across individuals) and may acquire more capital later in life. However, when individuals have limited access to credit markets, they would not be able to borrow against their future income to finance their education. In a country with high income (or wealth) inequality, an even greater proportion of the population would not have access to credit markets to finance its education. Inability to borrow to finance one's education can be overcome, supposedly, by some income redistribution scheme that would allow wider access to education. This argument is often used as a justification for public spending on education, which will lead to higher productivity and, eventually, higher output. The papers discussed below study what kind of effects these redistribution schemes would have on long-run income, income inequality and education levels. The papers also address how a country as a whole would choose between privately financed or public education.

Glomm and Ravikumar (1992) present a model of endogenous economic growth with heterogeneous (in income and skills) agents. In their model, education is the main channel of investment in human capital. The authors compare the implications of the model in two different environments: public schools and private schools. In the former, a decision about investments in education is made through majority voting. In the latter, each household independently decides on quality (which is a function of expenditure) of education. The model suggests that inequality is reduced quicker in a public education environment. On the other hand, private education should yield higher per capita incomes.

The authors also examine the model where the choice of an educational system is endogenous. Their result shows that if a majority of voting agents has a below-average income, the choice will be made in favor of public education. Their model also implies that two economies with public education and different income distributions will have different per capita incomes in future periods – the economy with more equal income distribution will have higher per capita incomes.

Lin (2003) modifies the models of Glomm and Ravikumar (1992) by allowing private and public schools to coexist in the economy. The agents are allowed to opt out of public education and choose private schooling. Naturally, in such a society, the initial choice of educational system will depend on the distribution of wealth endowment. Over time, Lin (2003) points out, income inequality should decline.

Galor and Zeira (1993) consider a model in which the initial distribution of wealth affects the long-run level of income. They assume investment in human capital to be indivisible (i.e. a person receives either full education or no education at all), and this results in the polarization of a society between high-income educated individuals and low-income uneducated ones. Thus, a long-run aggregate income level depends both on initial income distribution and aggregate income level. This model implies the existence of multiple equilibria. To which equilibrium the system will converge depends on the initial values of distribution of income and level of income. The authors obtain such a result because their model implies a threshold value of income below which agents choose not to invest in human capital. So, for instance, a country with perfect equality but a low level of aggregate income will converge to a steady state where everyone is a low-income uneducated type.

Chiu (1998) presents a model in which greater income equality implies higher investment in human capital. He points out that "in a free market economy the material possession one is born with does have a determining effect on how one's talent is developed and used." Chiu's model is an overlapping generations (OLG) model in which agents are heterogeneous in income and ability. According to his model, only the most talented children from poor families have a chance of going to college because only they have high enough rates of return to justify the investment in human capital. On the other hand, there will be children with lower talent from richer families who will be sent to college. Consequently, their rates of return will be lower than children from poor families. This model implies that, with some mechanism of wealth redistribution, aggregate human capital will increase in all subsequent periods because the rich will stop sending the least talented children to college while more talented children from poor families will be able to afford education. Thus, to the extent that human capital affects growth, more equal income distribution should imply higher growth rates. To demonstrate that point, Chiu (1998) shows that, in his OLG model, an exogenous decrease in inequality in one generation's income increases human capital accumulated by that generation and raises initial income for all subsequent generations.

Empirical studies of the effects of income inequality on education are not nearly as abundant as theoretical studies. One of the few attempts to establish an empirical link between inequality and education is by Checchi (2003), who finds evidence that inequality negatively affects enrollment in secondary schools. He also finds evidence that inequality reduces enrollment in higher education for males and in primary education for females. Bergh and Fink (2008) approach the issue from a different angle: They study whether enrollment in higher education is increased by higher public expenditure on education and whether education reduces inequality. They find no evidence that government expenditure increases enrollment, but they do find some evidence that higher tertiary enrollment leads to a subsequent reduction in inequality. So, existing studies (as well as this paper) do confirm some of the predictions of theoretical models.

III. Description of the Model and Data

The main goal of this paper is to find out whether enrollment in primary, secondary and tertiary schools is affected by income inequality. In general terms, the theoretical models discussed in the previous section describe the relationship between enrollment and other economic variables as follows:

$$Enrollment=f(Income, Inequality, Ed.\ Spending, Institutions), \tag{1}$$

where *Income* is some measure of per capita income in a country, *Inequality* represents a measure of income inequality, *Ed. Spending* is public expenditure on education at the relevant level and *Institutions* are a broader measure of institutional development (such as democracy or rule of law) in a country. Income is expected to have a positive effect on enrollment simply because higher income would make education more affordable. All models predict that inequality will have a negative effect on educational attainment (and, consequently, on enrollment). Because public expenditure can counterbalance the effect of inequality, it is expected to have a positive effect on enrollment. Countries with more-developed institutions are expected to have higher enrollment: In such countries corruption and cronyism play a lesser role in deciding who will and who will not be accepted to school. More-developed countries with developed democracies might also vote for greater spending on schools. There is likely greater return on education in more-developed countries, encouraging greater enrollment.

When estimating relationships like the one described above, it is important to consider whether the estimates could be biased due to simultaneity: Greater inequality can limit access to education, while higher educational attainment (which is likely to be correlated with enrollment) can affect inequality. However, if education does increase incomes and lower inequality, it can do so only *after* it has been obtained. To illustrate this point further, if there is an increase in primary enrollment in the current year, that increase cannot result in an immediate increase in productivity and higher incomes because it will be several years before these pupils will join the labor force. Therefore, if we use data for the same period for enrollment and inequality, we would expect enrollment to be affected by inequality but not the other way around. This logic, however, can be employed only if we use time-series or panel data to analyze changes in inequality and enrollment over time. Cross-section estimates might be affected by simultaneity bias.

The same argument probably would not hold when considering the relationship between enrollment and public spending on education: When government officials are faced with low enrollment levels, they might increase public spending on education, so the two variables would be endogenous[1]. However, because the main goal of this paper is to study the effect of inequality, the public expenditure variable will be used mainly as a control variable. So, even though the estimates of this variable's effect on enrollment might be biased, it should not affect the estimates of other variables included in the model.

The empirical results presented in this paper are based on two data sets: *World Bank Development Indicators* (2007, henceforth WDI) and the data set on income inequality measures compiled by Deininger and Squire (1996) and updated in May 2007. Tables 12-17 in the appendix list the countries and years for which all of the data were available by different levels of education.

This paper explores determinants of three levels of school enrollment: primary, secondary and tertiary. For primary and secondary schooling, WDI data contain estimates of gross and net enrollment. According to the UNESCO Institute of Statistics, which collects the data on school enrollment, net enrollment is defined as the "enrollment of the official age-group for a given level of education expressed as a percentage of the corresponding population." Gross enrollment is the "total enrollment in a specific level of education, regardless of age, expressed as a percentage of the eligible official school-age population corresponding to the same level of education in a given school-year." There are data only on gross enrollment at the tertiary level because UNESCO does not specify an age-relevant group for that level of schooling.

Inequality is measured by the Gini coefficient, which takes on values from 1 to 100, where 100 is perfect inequality. The data set of income inequality by Deininger and Squire (1996) contains several subsets of data ranked according to the quality of the data collected. Here, only "high-quality" data were used.

Instead of per capita GDP, per capita consumption expenditure is used to measure the average incomes of individuals. Recent research on measures of inequality[2] suggests that consumption rather than total output per capita might be the measure that more accurately represents individual well-being. The data are in purchasing-power parity adjusted, constant 2000 U.S. dollars and were transformed into logarithms.

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[1]For example, United Nations stated that one of its *Millennium Development Goals* is to "*achieve universal primary education.*" To create incentives for countries to increase primary enrollment rates, a Fast Track Initiative has been put in place to expedite donor assistance to the countries that succeed in raising their enrollment rates and eradicating the male/female gap. Such economic incentives are certainly welcome, but, econometrically, they make it difficult to identify the effect off public expenditure due to simultaneity bias.
[2]See Blundell et al. (1998) for references on the literature studying income and consumption inequality.

Government expenditure on schools is measured as spending per student for a particular level of education relative to per capita GDP in that country.

Institutional differences among countries are captured by using two variables: the Organization for Cooperation and Economic Development (OECD) dummy and degree of urbanization. The latter is measured as the proportion of population residing in urban areas of a country.

Assuming a linear relationship[3] between enrollment and the variables that might affect it, we can write the model of determinants of school enrollment as

$$Enrollment_{i,t} = \alpha + \delta_i + \beta_1 PerCapita\ Consumption_{i,t} + \beta_2 Gini_{i,t} + \beta_3 Ed.Spending_{i,t} + \beta_4 Urbanization_{i,t} + \varepsilon_{i,t,}, \quad (2)$$

where δ_i are time-invariant, country-specific effects, and the explanatory variables are as described above. Subscripts denote data for country i at time t. The equation is estimated using both Fixed-Effects and Random-Effects models. In addition to panel date regressions, between regressions are estimated to study determinants of cross-country variation in enrollment. That regression equation is

$$Enrollment_i = \alpha + \beta_1 PerCapita\ Consumption_i + \beta_2 Gini_i + \beta_3 Ed.Spending_i + \beta_4 Urbanization_i + \varepsilon_{i.} \quad (3)$$

In this model, the data are averages of each variable for each country over all of the years for which the data were available.

In addition to the base models in equations (2) and (3), modified versions of these equations are estimated and they include slope dummy variables to allow for different effects of inequality and school expenditure in developing and developed countries. Another version of these equations includes an interaction variable to test whether public spending on education diminishes the effect of income inequality. These models will be discussed in greater detail in the next section, which reports estimation results.

IV. Estimation Results

Estimation results are organized by educational level and type of enrollment (net and gross). The tables with regression estimates are included in the appendix.

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[3]Although linearity assumption is simplistic, it is fairly reasonable. Cecchi (2003), for example, provides a theoretical justification for using a linear model: He developed a model in which income inequality measures such as Gini coefficient are linearly related to enrollment (which are measured as the proportion of children in a school-age population that attend schools). Empirically, including squared terms on the left hand side of the regression equation (2) to capture possible nonlinear relationship did not alter the signs of coefficients. In the vast majority of the cases, square terms were not significant. Therefore, linear regression appears to be a reasonable model for determinants of enrollment.

Net Primary School Enrollment

Cross-section estimates from the between regression shown in table 2 demonstrate that the only variable that is significant across the board is the logarithm of per capita consumption expenditure. The estimate is positive, indicating that in higher income countries, pupils are more likely to attend primary schools at the appropriate age. The Gini coefficient is not significant, implying that for the countries in the sample, inequality does not limit primary-school participation. The only exception is that the *OECD*Gini* slope dummy in Model 3 is statistically significant at the 5% level. The null hypothesis $\beta_1=\beta_6$ can be rejected at the 5% level in favor of the alternative $\beta_1\neq \beta_6$. Therefore, there is evidence that, in OECD countries, higher inequality leads to lower Net Primary Enrollment, but that is not the case in non-OECD countries.

The estimates show no evidence that greater spending per pupil increases net enrollment. Degree of urbanization is also insignificant: Countries with greater rural population seem to have the same access to primary schooling as more urbanized countries do. Other than the *Gini*OECD* slope dummy, all other dummies are insignificant. There is no evidence that institutional differences between developed and developing countries lead to differences in Net Primary Enrollment.

The panel estimates shown in table 3 largely resemble the between estimates: The proportion of urban population, public expenditure on primary education and income inequality have no effect on net enrollment rates. There is some evidence of interaction between expenditure on primary schools and income inequality: When the interaction between the two variables is included in the equation (Model 4), the Gini coefficient becomes negative and significant and the interaction variable is close to being significant at the 10% level. These estimates suggest that inequality does have a negative effect on Net Primary Enrollment, but this effect can be countered by greater spending on primary schooling. This is an important finding because it suggests that public spending on primary schools might equalize access to schools for different income groups.

Per capita consumption is also statistically significant and positive but only in Random-Effects models and not in the Fixed-Effects model. Because Fixed-Effects models measure only time series variation by factoring out cross-section differences, these results imply that when per capita incomes increase, there is no immediate effect on net enrollment. Why is there such a disparity between cross-section and time series effects of income? One reason could be that the difference in per capita incomes across countries might reflect more than just the differences in incomes themselves. Higher per capita incomes may be a sign of more-deeply developed institutions that take years to build. In other words, the between regression might reflect a long-run relationship between Net Primary Enrollment and income, while within regression (which measures year-to-year effects) measures a short-run relationship, which usually tends to be more inelastic.

Specification tests indicate that there is no evidence of heteroskedasticity in the data used to estimate the between regression. In the panel estimates, the Hausman test shows that country-specific effects are not correlated with right-hand side variables in the Random-

Effects model. Therefore, variables that might affect school enrollment but were omitted from the equations are not correlated with the variables included in the regression, so the estimates of the Random-Effects model are unbiased.

Gross Primary Enrollment

The cross-country estimates shown in table 4 indicate that the only statistically significant variable is the Gini coefficient, which, surprisingly, is positive. This finding implies that countries with higher income inequality have *higher* gross enrollment. To understand this result, it is important to keep in mind how Gross Enrollment is defined – the ratio of *all* students attending primary schools relative to the number of people in the appropriate age group. Quoting the UNESCO Institute of Statistics description of the data, the key limitation of this variable is that "(Gross Enrollment Ratio) can be more than 100% due to the inclusion of over-aged and under-aged pupils/students because of early or late entrants and grade repetition." Even though both under- and over-age students can increase Gross Enrollment, the most likely cause for it to be greater than 100% is delayed enrollment[4]. Simple descriptive statistics shown in table 1 further illustrate this issue: Average Gross Primary Enrollment is 103.46% in OECD countries (the highest value in the sample is 124.72% for Portugal, in 2000) and 105.78% in non-OECD countries (the highest value is 154.68% in Brazil, in 1999). On the other hand, average net enrollment is 97.60% in OECD countries and 92.23% in non-OECD countries.

The lack of statistical significance for other variables included in the equation is also an important finding. Per capita consumption and public expenditure on primary education don't seem to play a big role in Gross Primary Enrollment. Institutional differences that might exist between OECD and non-OECD countries don't seem to matter either. Therefore, it appears that delayed enrollment is not so much a function of the well-being of a country as it is of income distribution. Judging by the estimates of Gross Primary Enrollment, most of which are in excess of 100%, most countries are equipped to educate their primary-school-age students, but income inequality prevents the students from enrolling in schools at the appropriate age.

Panel estimates shown in table 5 are mostly consistent with the between regression estimates: Urbanization, per capita income, and school expenditure appear to have little to no effect on gross enrollment. The only substantial difference between cross-section and panel estimates is that most estimates for the Gini coefficient are negative, although a few are

[4]There are, potentially, other problems with the way these data are collected or reported by governments. For example, Ukraine was excluded from the primary gross enrollment sample because its observations were, likely, outliers. For years 2000-2003, primary gross enrollment was recorded as 104.85%, 110.39%, 90.59% and 93.4%. No other country in the sample has experienced such a large drop in enrollment in such a short period of time. This drop occurred because of the change in the system of primary education: Around the same time, a new, four-year primary curriculum was introduced in addition to the existing three-year curriculum in the primary schools. However, not all areas of the country had the four-year curriculum available. While it was mandatory for pupils to start school at the age of 7 to fulfil the three-year curriculum, parents had an *option* to start their children in school at the age of 6 on the four-year curriculum. This change led to a change in the official school age from 7 to 6, but because it was not accompanied by a corresponding change in the enrollment, leading to a sudden drop in the reported gross enrollment.

statistically significant. Coefficients that do turn out to be significant are in the models that include variables measuring interaction between inequality and public expenditure on primary schools. These estimates show results similar to Net Primary Enrollment: An increase in income inequality within a country leads to a reduction in Gross Primary Enrollment, but that reduction is mitigated by public expenditure on schools. The estimates of model 4, which includes the interaction variable *Gini*Ed.Spending,* show that the slope coefficient on Gini is statistically significant and the interaction variable is borderline significant.

<u>Net Secondary Enrollment</u>

The results for secondary-school enrollment appear to be more in line with the predictions of theoretical models: Estimates of the between regression (shown in table 6) indicate that there is a negative and significant correlation between Net Secondary Enrollment and inequality. Using the estimates of this regression, we can see that a 1 percentage point increase in the Gini coefficient results in close to a 1 percentage point decrease in enrollment. It also appears that a greater degree of urbanization is positively associated with secondary- school enrollment. The latter finding is not surprising: If a greater proportion of the population resides in urban areas where there are more schools, then more children will have an opportunity to attend schools. Hazarika (2001) examines the various costs of attending schools and finds that distance to schools is one of the key (and sometimes the only) determinants of school attendance. It might also be the case that families living in urban areas are more likely to have both parents employed full-time, in which case secondary schools are playing the role of a daycare provider.

One surprising finding in almost all the models is that the coefficient for per capita consumption expenditure is negative. Taken at face value, this would indicate that countries with higher per capita incomes have lower net enrollment than the poorer countries. That is unlikely to be the case, contrary to predictions of the between regression. For example, OECD countries included in the sample have an average per capita consumption expenditure of $13,420.73 and a Net Secondary Enrollment of 89.79%, while non-OECD countries have an average per capita consumption expenditure of $2,249.03 and a net enrollment of 76.82%. Graph 1 might explain the disagreement between these simple descriptive statistics and regression estimates: The results might be driven by outliers. Eliminating the two observations for which net enrollment was below 60% (El Salvador and Colombia) made the coefficient estimates on per capita consumption insignificant in all versions of the model.

When the *OECD*Ed.Spending* slope dummy is included in the equation (Model 4), public expenditure on schooling becomes significant (although only at 10% level). Therefore, there is some evidence that non-OECD countries that spend more on secondary schools do have higher net enrollment. In OECD countries additional funding does not seem to increase net enrollment: The null hypothesis that the sum of coefficients β_3 and β_7 (which measures the marginal effect of government spending on education on enrollment in OECD countries) is equal to zero cannot be rejected.

Panel estimates for Net Secondary Enrollment included in table 7 show that the only variable that seems to have any effect on net enrollment is the Proportion of Urban Population, which is positively correlated with net enrollment. Fixed-Effects estimates show that a 1% increase in Proportion of Urban Population leads to about 1.35% to 1.45% increase in Net Secondary Enrollment. The Hausman test indicates that country-specific effects are correlated with explanatory variables, so the Random-Effects estimates are biased. When we compare the estimates of Fixed-Effects and Random-Effects models, we can see that, for all versions of the model, Fixed-Effects estimates of the slope coefficient on the Proportion of Urban Population are about three times the size of the Random-Effects estimates. From this we can conclude that the cross-country effects capture some unobserved variable that is positively (negatively) correlated with urbanization and negatively (positively) correlated with Net Secondary Enrollment[5].

Although the results of the Random-Effects model are likely to be biased, one particular estimate is worth mentioning. When the *OECD*Gini* slope dummy is included in the equation (Model 2), the slope coefficient on the *Gini* variable is negative and statistically significant. The sum of the coefficients on *Gini* and *OECD*Gini* is close to zero (the p-value for the null hypothesis $\beta_1+\beta_6=0$ versus the two-sided alternative is 0.2541). Therefore, there is some evidence that inequality might have an adverse affect on Net Secondary Enrollment, but only in non-OECD countries.

Gross Secondary Enrollment

Cross-section estimates for Gross Secondary Enrollment shown in table 8 are similar to the estimates for net enrollment: Variables that are found to be statistically significant are the Gini coefficient (negative effect on enrollment) and Proportion of Urban Population (positive effect on enrollment).

Government expenditure on secondary schooling does not appear to be correlated with gross enrollment. The only other variable that appears statistically significant is the OECD dummy in Model 2, whose estimate is 14.2%. This dummy indicates that even after we control for income, inequality, urbanization and public expenditure on schools, OECD countries still have higher enrollment than non-OECD countries. Once again, we could argue that this difference might reflect more-established institutions that foster education in developed countries.

Table 9 contains panel estimates that appear quite different from estimates of the between regression: the Gini coefficient is found to be insignificant in all of the versions of

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[5]If the true model is $Y=\alpha + \beta X + \delta Z + \varepsilon$, but we estimate $Y=\alpha^* + \beta^* X + \varepsilon^*$ instead, then b, the estimator of β^* *is* $b=Cov(X,Y)/Var(X)$. The expected value of b is $E(b) = \beta + Cov(X,\delta Z)/Var(X)$. Thus b is biased if there is correlation between X and Z. The Random-Effects models assume that cross-section effects are randomly distributed and include them in the error term of the regression. Thus, using Random-Effects model when Fixed-Effects should be used is equivalent to omitting an important variable. Because the estimates of Fixed-Effects model are unbiased due to the fact that the model includes cross-section effects, by comparing Random- and Fixed-Effects models we can determine the direction of the bias and how cross-section effects are correlated with the variables included in the regression.

the model. From the estimates of Model 3, we can see that government expenditure on schooling does not appear to affect enrollment in non-OECD countries but has a negative effect in OECD countries (the p-value for the null hypothesis $\beta_3+\beta_7=0$ versus the alternative hypothesis $\beta_3+\beta_7<0$ is 0.093). Even though there is weak evidence that public expenditure on schools has a negative effect on enrollment, it is worthwhile to explore why there could be a negative relationship between the two variables. There are two possible explanations: The first possible reason is that in OECD countries public expenditure reduces *gross* enrollment so spending more money on secondary education does not lower the number of students attending school but, instead, ensures they do attend school at the appropriate age (i.e by increasing net enrollment). The second reason could be a potential data flaw resulting from the way public expenditures are measured in the WDI data. Expenditures on schools are calculated as the amount spent per student relative to the per capita GDP. If total spending on schools remains flat but enrollment increases, the amount spent per student will diminish as well. So, increases in enrollment could coincide with decreases in per capita expenditures.

One interesting finding for secondary enrollment comes from simple descriptive statistics. When we look at the averages for different types (gross versus net) and levels of school enrollment, we can see that Gross Primary Enrollment was 13.55% higher than net enrollment in non-OECD countries. In OECD countries that difference is only 5.86%. When we look at these differences for secondary enrollment, we find that the difference between net and Gross Secondary Enrollment is 11.14 percentage points (87.96% - 76.82%) in non-OECD countries and 21.29 percentage points (111.8% - 89.79%) in OECD countries. These statistics indicate that, even though Net Primary Enrollment in non-OECD countries is below 100%, these countries catch up by educating their pupils later (as indicated by greater than 100% Gross Primary Enrollment).

The same phenomenon is observed in secondary-school enrollment of OECD countries. Even though Net Secondary Enrollment is not far from 90%, gross enrollment exceeds 100%. So, even though students might not attend secondary schools at the appropriate age in OECD countries, they return to school later in life. In non-OECD countries, gross enrollment is below the *net* enrollment of OECD countries. Therefore, in non-OECD countries, a large proportion of the population not only delays entry into secondary education but it also never attends school. Because the data cover the period from late 1990s to early 2000s, these secondary-school age students either have already entered or are entering the labor force now. Therefore, these estimates imply that the educational gap between developed and developing countries is likely going to persist for years to come.

Gross Tertiary Enrollment

The between regression estimates for Gross Tertiary Enrollment (shown in table 10) show that the Gini coefficient is negatively and significantly correlated with enrollment. It is also the only variable that is significantly correlated with gross enrollment.

There is no evidence that greater spending on higher education increases tertiary enrollment. This finding confirms the results reported in Bergh and Fink (2008), which were discussed earlier. More urbanized countries don't appear to have higher enrollment. OECD dummies are also insignificant. If it is indeed true that these dummies capture institutional differences between developed and developing countries, than this lack of significance means that the discrepancy in tertiary enrollment between non-OECD and OECD countries can be largely explained by their inequality. In other words, after controlling for effects of inequality, both non-OECD and OECD (at least the ones included in the sample) provide the same access to tertiary schooling (but, certainly, not the same amount).

The within estimates (table 11) are almost the complete opposite of the between regression estimates: Urbanization and per capita consumption expenditure are positive and significant but the Gini coefficient is (almost) not. Therefore, when a greater proportion of the population moves to urban areas, tertiary enrollment increases. This finding is not surprising: Higher-education institutions are likely to be found in cities, not in rural areas. If a greater proportion of the population migrates to urban areas, those people will have greater access to higher education[6].

An increase in per capita income should make college more affordable for a greater number of individuals, so it is not surprising that per capita consumption expenditure has a positive and significant effect on tertiary enrollment. The estimates show that the slope coefficients on this variable are much higher than the corresponding estimates for primary or secondary enrollment. The reason for that is much greater variation in tertiary enrollment: The within standard deviation is 2.8% and the range is -5.49% to 9.94%. For comparison, the within standard deviation of the Gross Secondary Enrollment is 1.81% and the range is -3.55% to 6.22%. Correlation between tertiary enrollment and log consumption expenditure is 0.55. Correlation between secondary gross enrollment and log consumption expenditure is 0.28. Therefore, higher correlation and greater variance in tertiary enrollment leads to a much higher estimate of the slope coefficient for the log of per capita expenditure.

The sign on the Gini coefficient depends on whether a country is a developed country. In all models, Estimates of coefficient β_1 are not significantly different from zero, implying that in non-OECD countries, inequality has no effect on enrollment. In model 2, however, the sum of coefficients β_1 and β_6 is equal to -0.32 and is significantly different from zero (the p-value for the test of null hypothesis $\beta_1+\beta_6=0$ versus the alternative $\beta_1+\beta_6\neq0$ is 0.044). Therefore, a decrease in income inequality by 1% leads to an increase in tertiary enrollment by 0.32%, but only in OECD countries.

The only counterintuitive finding is that the public expenditure on higher education is negatively associated with enrollment. However, as it was already discussed above, this might be a result of how the public expenditure variable is constructed.

[6]There is probably a reverse effect of tertiary enrollment on urbanization – when more students enroll in higher education institutions, they might be moving from rural to urban areas. Thus, greater enrollment could lead to greater urbanization. However, rising enrollment is unlikely to be a dominant factor in greater urbanization – total number of student in higher education is relatively compared to the overall population.

V. Conclusions

The results presented in the previous section show that income inequality does not appear to limit access to primary education. Neither cross-section nor panel estimates show much support for the hypothesis that income inequality has an adverse effect on primary-school enrollment. Moreover, when panel estimates do show a negative correlation between inequality and primary-school enrollment, they also indicate that greater spending on primary education reduces the effect of inequality. This finding carries an important policy implication, especially in the light of the Millennium Development Goals set by United Nations and the World Bank in conjunction with the governments of developing and developed countries. One of these goals was to achieve universal primary education. The estimates presented in this paper indicate that primary enrollment do not seem to depend greatly on income distribution: The poor appear to be able to send their children to primary school along with the rich. The effect that inequality does have on primary-school enrollment can be offset by greater spending on primary education.

The same, unfortunately, cannot be said for secondary and tertiary education. Income inequality appears to play a substantial role in determining school enrollment. Because greater public spending on schooling does not seem to have much of an effect on enrollment, it is not immediately obvious how one could counter the effect of inequality. Increasing the supply of schools with the hope of lowering the private costs of attending them seems to have very little effect[7]. However, the findings presented in the paper do suggest that urbanization is positively correlated with both secondary- and tertiary-school enrollment. It would be naïve to think that countries should solve the problem of low secondary- and tertiary-school enrollment by encouraging their citizens to move to urban areas. Nevertheless, this particular finding indicates that greater access to schools in urban environments coupled with greater returns on schooling in urban areas (due to greater chances of finding a job requiring advanced skills) might lead to greater investment in human capital. Therefore, the problem of low enrollment in secondary and tertiary schools is probably not going to be solved solely by lowering the cost of education by increasing public spending; it will require fostering an economy where the benefits of education can be realized.

[7]Filmer (2004) finds that lowering the distance to schools in rural areas of Sub-Saharan Africa had a positive but a very small effect on school participation.

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APPENDIX

Table 1:
Descriptive Statistics for the Data in the Sample

Entire Sample

Variable	N	Min	Max	Mean	St.Dev
Primary Gross Enrollment (%)	182	90.41	154.68	104.70	8.73
Primary Net Enrollment (%)	136	80.63	99.99	95.11	4.94
Secondary Gross Enrollment (%)	179	49.82	159.85	98.94	19.55
Secondary Net Enrollment (%)	127	39.32	98.33	84.07	11.74
Tertiary Gross Enrollment (%)	173	14.00	86.90	48.54	17.60
Public Expenditure on Primary Ed. (%)	177	5.27	46.17	16.38	5.90
Public Expenditure on Secondary Ed. (%)	165	7.19	38.62	20.85	6.82
Public Expenditure on Tertiary Ed. (%)	162	9.36	76.29	30.93	12.81
Proportion of Urban Population (%)	182	30.78	91.72	69.15	13.33
Gini (%)	182	19.69	61.20	37.00	10.96
Per Capita Consumption Expenditure ($)	182	183.03	25125.00	7466.58	6338.9

Non-OECD

Variable	N	Min	Max	Mean	St.Dev
Primary Gross Enrollment (%)	97	90.41	154.68	105.78	10.79
Primary Net Enrollment (%)	63	80.63	99.54	92.23	5.41
Secondary Gross Enrollment (%)	94	49.82	111.80	87.96	13.71
Secondary Net Enrollment (%)	56	39.32	95.31	76.82	13.71
Tertiary Gross Enrollment (%)	93	14.00	70.98	39.46	15.74
Public Expenditure on Primary Ed. (%)	92	5.27	30.03	13.73	5.39
Public Expenditure on Secondary Ed. (%)	86	7.19	27.86	16.77	5.50
Public Expenditure on Tertiary Ed. (%)	90	9.36	62.36	26.53	10.44
Proportion of Urban Population (%)	97	30.78	91.72	65.00	14.41
Gini (%)	97	19.69	61.20	42.12	11.88
Per Capita Consumption Expenditure ($)	97	183.03	5957.40	2249.03	1446.9

OECD

Variable	N	Min	Max	Mean	St.Dev
Primary Gross Enrollment (%)	85	93.25	124.72	103.46	5.31
Primary Net Enrollment (%)	73	91.55	99.99	97.60	2.65
Secondary Gross Enrollment (%)	85	83.21	159.85	111.08	17.83
Secondary Net Enrollment (%)	71	81.33	98.33	89.79	5.03
Tertiary Gross Enrollment (%)	80	31.04	86.90	59.11	13.25
Public Expenditure on Primary Ed. (%)	85	11.17	46.17	19.25	5.04
Public Expenditure on Secondary Ed. (%)	79	14.16	38.61	25.28	5.15
Public Expenditure on Tertiary Ed. (%)	72	19.64	76.29	36.43	13.41
Proportion of Urban Population (%)	85	53.74	89.58	73.89	10.15
Gini (%)	85	22.60	46.40	31.15	5.72
Per Capita Consumption Expenditure ($)	85	6819.68	25125.00	13420.73	4108.5

Table 2:
Cross-Section (Between) Estimates for Net Primary Education

Number of Observations: 30

Variable (regression coeff.)	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept (α)	72.66***	68.94***	62.25***	71.14***	59.83***	62.21***
	(7.95)	(8.67)	(8.65)	(9)	(12.03)	(16.98)
Gini coefficient (%) (β_1)	-0.011	-0.043	0.057	-0.091	0.287	0.145
	(0.103)	(0.107)	(0.110)	(0.119)	(0.235)	(0.398)
Log Per Capita Consumption Expenditure (β_2)	2.114**	3.055**	3.367***	3.573**	2.195**	3.051*
	(0.925)	(1.277)	(1.199)	(1.395)	(0.909)	(1.643)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	0.068	0.011	0.080	-0.196	0.800	0.436
	(0.194)	(0.201)	(0.190)	(0.299)	(0.556)	(1.057)
Percentage of Urban Population (β_4)	0.054	0.046	0.032	0.032	0.067	0.051
	(0.081)	(0.081)	(0.075)	(0.082)	(0.080)	(0.089)
OECD (β_5)		-2.883	11.193	-10.036		-4.738
		(2.709)	(7.007)	(8.096)		(11.812)
OECD*Gini (β_6)			-0.434**			
			(0.202)			
OECD*Ed.Spending (β_7)				0.345		0.126
				(0.368)		(0.512)
Ed.Spending*Gini (β_8)					-0.021	-0.014
					(0.015)	(0.022)
R-squared	53.3%	55.4%	62.6%	57.2%	57.2%	58.1%
White Heteroskedasticity test (p-value)	0.75	0.59	0.35	0.36	0.79	0.53

Numbers in parenthesis are standard errors.

***, ** and * indicate significance at 1%, 5% and 10% levels, respectively.

Estimates were obtained using OLS

Table 3:
Panel Estimates for Net Primary Education

Number of Observations: 126. Number of Countries:30

Variable	Model 1		Model 2		Model 3		Model 4		Model 5	
	Fixed	Random	Fixed	Random	Fixed	Random	Fixed	Random	Fixed	Random
Intercept (α)	81.2***	75.9***	82.4***	73.7***	87.0***	76.3***	88.9***	78.9***	92.9***	79.6***
	(11.7)	(4.0)	(11.8)	(5.0)	(13.3)	(4.3)	(12.6)	(4.9)	(13.9)	(5.2)
Gini coefficient (%) (β_1)	-0.025	-0.034	0.001	-0.021	-0.029	-0.034	-0.118*	-0.091	-0.115*	-0.096
	(0.031)	(0.028)	(0.043)	(0.032)	(0.031)	(0.028)	(0.067)	(0.061)	(0.068)	(0.062)
Log. Per Capita Consumption Expenditure (β_2)	0.38	1.72***	0.39	2.02***	-0.28	1.63***	0.26	1.56***	-0.25	1.48**
	(1.07)	(0.54)	(1.07)	(0.69)	(1.29)	(0.58)	(1.07)	(0.57)	(1.29)	(0.59)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	0.017	0.012	0.014	0.009	0.073	0.015	-0.129	-0.082	-0.076	-0.094
	(0.027)	(0.027)	(0.028)	(0.027)	(0.067)	(0.046)	(0.099)	(0.092)	(0.124)	(0.105)
Percentage of Urban Population (β_4)	0.131	0.081	0.128	0.077	0.147	0.086	0.076	0.080	0.092	0.082
	(0.161)	(0.057)	(0.159)	(0.061)	(0.161)	(0.063)	(0.163)	(0.059)	(0.165)	(0.064)
OECD*Gini (β_6)			-0.051	-0.033						
			(0.061)	(0.039)						
OECD*Ed.Spending (β_7)					-0.069	-0.004			-0.054	0.005
					(0.076)	(0.049)			(0.076)	(0.051)
Ed.Spending*Gini (β_8)							0.006	0.004	0.005	0.004
							(0.004)	(0.003)	(0.004)	(0.003)
R-squared	98.1%	23.7%	98.1%	22.1%	98.1%	20.1%	77.9%	23.1%	98.1%	20.8%
p-value for F-test of Null Hypothesis of No Fixed Effects	<0.01	-	<0.01	-	<0.01	-	<0.01	-	<0.01	-
p-value for Hausman Test of Random Effects	-	0.68	-	0.79	-	0.62	-	0.31	-	0.43

Numbers in parenthesis are standard errors. ***, ** and * indicate significance at 1%, 5% and 10% levels, respectively. There is no OECD intercept dummy included in the estimates because that variable is absorbed in country-specific effects. The intercept in Fixed-Effects model is identified by setting the fixed effect for the last country in the sample to zero.

Table 4:
Cross-Section (Between) Estimates for Gross Primary Education

Number of Observations: 36

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept (α)	76.67***	77.72***	73.18***	80.38***	63.76***	72.14**
	(12.21)	(16.05)	(16.8)	(16.26)	(19.08)	(26.74)
Gini coefficient (%) (β_1)	0.289**	0.289**	0.346**	0.258*	0.619	0.443
	(0.133)	(0.135)	(0.149)	(0.139)	(0.397)	(0.494)
Log Per Capita Consumption Expenditure (β_2)	1.305	1.099	1.430	1.506	1.292	1.523
	(1.531)	(2.527)	(2.556)	(2.558)	(1.537)	(2.597)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	-0.064	-0.065	-0.033	-0.307	0.811	0.225
	(0.279)	(0.284)	(0.287)	(0.372)	(1.03)	(1.41)
Percentage of Urban Population (β_4)	0.105	0.112	0.099	0.096	0.104	0.095
	(0.107)	(0.125)	(0.126)	(0.126)	(0.107)	(0.128)
OECD (β_5)		0.505	11.173	-10.347		-8.014
		(4.885)	(12.384)	(11.834)		(13.408)
OECD*Gini (β_6)			-0.346			
			(0.368)			
OECD*Ed.Spending (β_7)				0.576		0.436
				(0.572)		(0.681)
Ed.Spending*Gini (β_8)					-0.023	-0.013
					(0.026)	(0.032)
R-squared	23.9%	24.0%	26.2%	26.5%	25.9%	26.9%
White Heteroskedasticity test (p-value)	0.81	0.60	0.81	0.78	0.62	0.62

Numbers in parenthesis are standard errors.
***, ** and * indicate significance at 1%, 5% and 10% levels, respectively.
Estimates were obtained using OLS

Table 5: Panel Estimates for Gross Primary Education

Number of Observations: 153. Number of Countries:36

Variable	Model 1		Model 2		Model 3		Model 4		Model 5	
	Fixed	Random	Fixed	Random	Fixed	Random	Fixed	Random	Fixed	Random
Intercept (α)	141.6***	100.6***	141.2***	98.5***	141.4***	99.1***	145.1***	105.5***	145.4***	105.2***
	(30.8)	(9.7)	(31.1)	(10.9)	(31.0)	(10.3)	(30.7)	(10.2)	(30.8)	(11.2)
Gini coefficient (%) (β_1)	-0.076	-0.003	-0.082	0.007	-0.075	-0.001	-0.232**	-0.142	-0.237*	-0.139
	(0.068)	(0.057)	(0.082)	(0.061)	(0.068)	(0.057)	(0.117)	(0.106)	(0.122)	(0.107)
Log. Per Capita Consumption Expenditure (β_2)	-0.88	-0.32	-0.89	-0.06	-0.93	-0.15	-0.33	-0.03	-0.29	-0.04
	(2.09)	(1.23)	(2.10)	(1.50)	(2.10)	(1.36)	(2.10)	(1.26)	(2.13)	(1.37)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	0.018	-0.091	0.018	-0.096	0.038	-0.081	-0.459	-0.507*	-0.482	-0.506
	(0.118)	(0.095)	(0.118)	(0.096)	(0.135)	(0.105)	(0.316)	(0.288)	(0.354)	(0.305)
Percentage of Urban Population (β_4)	-0.367	0.121	-0.367	0.121	-0.347	0.124	-0.405	0.103	-0.415	0.106
	(0.304)	(0.113)	(0.305)	(0.112)	(0.311)	(0.111)	(0.302)	(0.115)	(0.312)	(0.114)
OECD*Gini (β_6)			0.001	0.001						
			(0.136)	(0.088)						
OECD*Ed.Spending (β_7)					-0.066	-0.040			0.032	0.001
					(0.207)	(0.144)			(0.214)	(0.147)
Ed.Spending*Gini (β_8)							0.012	0.010	0.012	0.010
							(0.007)	(0.007)	(0.008)	(0.007)
R-squared	96.8%	1.4%	96.8%	1.6%	96.8%	1.6%	96.8%	2.9%	96.8%	2.9%
p-value for F-test of Null Hypothesis of No Fixed Effects	<0.01	-	<0.01	-	<0.01	-	<0.01	-	<0.01	-
p-value for Hausman Test of Random Effects	-	0.30	-	0.40	-	0.39	-	0.41	-	0.51

Numbers in parenthesis are standard errors. ***, ** and * indicate significance at 1%, 5% and 10% levels, respectively. There is no OECD intercept dummy included in the estimates because that variable is absorbed in country-specific effects. The intercept in Fixed-Effects model is identified by setting the fixed effect for the last country in the sample to zero.

Table 6:
Cross-Section (Between) Estimates for Net Secondary Education

Number of Observations: 26

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept (α)	95.87*** (15.17)	114.06*** (17.71)	124.78*** (18.73)	97.02*** (20.13)	120.78*** (22.39)	116.21** (42.23)
Gini coefficient (%) (β_1)	-0.871*** (0.179)	-0.820*** (0.173)	-0.979*** (0.2)	-0.681*** (0.188)	-1.561*** (0.497)	-1.089 (0.806)
Log Per Capita Consumption Expenditure (β_2)	-1.928 (2.549)	-5.589** (2.549)	-6.15** (2.509)	-5.815** (2.46)	-1.561 (1.552)	-5.523** (2.571)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	0.428 (0.279)	0.271 (0.280)	0.146 (0.286)	0.815* (0.435)	-1.125 (1.084)	-0.189 (1.981)
Percentage of Urban Population (β_4)	0.379** (0.141)	0.491*** (0.149)	0.532*** (0.148)	0.541*** (0.147)	0.410*** (0.139)	0.538*** (0.150)
OECD (β_5)		11.477* (6.462)	-5.981 (13.508)	32.094** (14.354)		24.197 (21.095)
OECD*Gini (β_6)			0.549 (0.376)			
OECD*Ed.Spending (β_7)				-0.904 (0.567)		-0.560 (0.878)
Ed.Spending*Gini (β_8)					0.039 (0.026)	0.020 (0.038)
R-squared	79.0%	81.9%	83.7%	84.0%	81.1%	84.2%
White Heteroskedasticity test (p-value)	0.12	0.59	0.37	0.42	0.39	.66

Numbers in parenthesis are standard errors.
***, ** and * indicate significance at 1%, 5% and 10% levels, respectively.
Estimates were obtained using OLS

Table 7: Panel Estimates for Net Secondary Education

Number of Observations: 104. Number of Countries:26

Variable	Model 1		Model 2		Model 3		Model 4		Model 5	
	Fixed	Random	Fixed	Random	Fixed	Random	Fixed	Random	Fixed	Random
Intercept (α)	-36.1 (48.7)	26.6 (23.4)	-35.4 (49.4)	63.2** (26.2)	-37.1 (49.1)	32.5 (25.3)	-40.6 (49.6)	24.9 (24.9)	-53.2 (50.9)	26.5 (31.6)
Gini coefficient (%) (β_1)	0.222 (0.161)	-0.014 (0.139)	0.198 (0.254)	-0.026 (0.166)	0.231 (0.164)	-0.019 (0.141)	0.353 (0.287)	0.025 (0.26)	0.641 (0.394)	-0.002 (0.313)
Log. Per Capita Consumption Expenditure (β_2)	1.25 (4.14)	2.61 (2.55)	1.04 (4.47)	-2.68 (3.32)	1.19 (4.17)	1.85 (2.78)	1.15 (4.17)	2.59 (2.57)	0.73 (4.18)	1.91 (2.95)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	-0.166 (0.146)	-0.062 (0.132)	-0.166 (0.147)	-0.052 (0.130)	-0.109 (0.214)	-0.149 (0.179)	0.094 (0.494)	0.012 (0.464)	0.825 (0.846)	-0.204 (0.656)
Percentage of Urban Population (β_4)	1.325*** (0.453)	0.502* (0.264)	1.332*** (0.459)	0.637** (0.259)	1.353*** (0.462)	0.507* (0.266)	1.331*** (0.455)	0.504* (0.266)	1.457*** (0.470)	0.583** (0.286)
OECD*Gini (β_6)			0.042 (0.332)	0.455** (0.195)						
OECD*Ed.Spending (β_7)					-0.101 (0.277)	0.156 (0.221)			-0.415 (0.391)	0.139 (0.277)
Ed.Spending*Gini (β_8)							-0.007 (0.012)	-0.002 (0.011)	-0.019 (0.017)	0.001 (0.014)
R-squared	98.2%	7.8%	98.2%	12.9%	98.2%	8.2%	98.2%	7.8%	98.2%	11.9%
p-value for F-test of Null Hypothesis of No Fixed Effects	<0.01	-	<0.01	-	<0.01	-	<0.01	-	<0.01	-
p-value for Hausman Test of Random Effects	-	0.03	-	0.09	-	0.06	-	0.05	-	0.12

Numbers in parenthesis are standard errors. ***, ** and * indicate significance at 1%, 5% and 10% levels, respectively. There is no OECD intercept dummy included in the estimates because that variable is absorbed in country-specific effects. The intercept in Fixed-Effects model is identified by setting the fixed effect for the last country in the sample to zero.

Table 8:
Cross-Section (Between) Estimates for Gross Secondary Education

Number of Observations: 36

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept (α)	77.98***	108.94***	106.48***	118.07***	53.01	83.51*
	(20.82)	(25.75)	(27.84)	(28.13)	(34.18)	(46.63)
Gini coefficient (%) (β_1)	-0.669**	-0.747***	-0.717**	-0.838***	-0.021	0.007
	(0.251)	(0.251)	(0.275)	(0.27)	(0.747)	(0.947)
Log Per Capita Consumption Expenditure (β_2)	0.795	-4.042	-3.893	-3.63	0.269	-5.003
	(2.519)	(3.504)	(3.607)	(3.557)	(2.589)	(3.859)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	0.459	0.212	0.240	-0.151	1.937	1.891
	(0.426)	(0.429)	(0.45)	(0.615)	(1.659)	(2.279)
Percentage of Urban Population (β_4)	0.398*	0.558**	0.551**	0.526**	0.417**	0.588**
	(0.201)	(0.211)	(0.215)	(0.215)	(0.203)	(0.226)
OECD (β_5)		14.204*	19.043	-3.124		11.793
		(7.446)	(20.272)	(22.222)		(27.441)
OECD*Gini (β_6)			-0.159			
			(0.619)			
OECD*Ed.Spending (β_7)				0.725		0.152
				(0.875)		(1.072)
Ed.Spending*Gini (β_8)					-0.037	-0.045
					(0.04)	(0.048)
R-squared	57.4%	62.0%	62.1%	62.9%	58.6%	64.0%
White Heteroskedasticity test (p-value)	0.42	0.61	0.68	0.30	0.32	0.69

Numbers in parenthesis are standard errors.
***, ** and * indicate significance at 1%, 5% and 10% levels, respectively.
Estimates were obtained using OLS

Table 9: Panel Estimates for Gross Secondary Education
Number of Observations: 148. Number of Countries: 36

Variable	Model 1		Model 2		Model 3		Model 4		Model 5	
	Fixed	Random	Fixed	Random	Fixed	Random	Fixed	Random	Fixed	Random
Intercept (α)	-138.1**	0.84	-147.4***	8.82	-131.1**	-7.9	-132.0**	8.7	-130.5**	-1.3
	(53.7)	(23.0)	(54.0)	(25.2)	(53.3)	(23.9)	(54.3)	(23.9)	(54.0)	(25.7)
Gini coefficient (%) (β_1)	0.181	0.051	0.080	0.002	0.197	0.052	0.017	-0.135	0.178	-0.071
	(0.132)	(0.115)	(0.154)	(0.123)	(0.131)	(0.115)	(0.245)	(0.219)	(0.264)	(0.227)
Log. Per Capita Consumption Expenditure (β_2)	9.91**	7.55***	9.58**	5.82*	8.99**	8.56***	10.17**	7.70***	9.04**	8.54***
	(4.04)	(2.64)	(4.04)	(3.05)	(4.04)	(2.74)	(4.06)	(2.63)	(4.10)	(2.74)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	-0.025	0.123	-0.023	0.129	0.221	0.253	-0.364	-0.264	0.178	-0.024
	(0.167)	(0.147)	(0.166)	(0.147)	(0.217)	(0.177)	(0.456)	(0.421)	(0.573)	(0.479)
Percentage of Urban Population (β_4)	1.579***	0.407	1.585***	0.477*	1.684***	0.423	1.539***	0.383	1.677***	0.403
	(0.547)	(0.278)	(0.545)	(0.288)	(0.545)	(0.279)	(0.550)	(0.276)	(0.554)	(0.278)
OECD*Gini (β_6)			0.365	0.248						
			(0.286)	(0.199)						
OECD*Ed.Spending (β_7)					-0.528*	-0.301			-0.517	-0.252
					(0.302)	(0.229)			(0.334)	(0.24)
Ed.Spending*Gini (β_8)							0.009	0.011	0.001	0.007
							(0.011)	(0.011)	(0.013)	(0.011)
R-Squared	97.9%	13.4%	98.0%	14.2%	98.0%	14.4%	98.0%	14.0%	98.0%	14.7%
p-value for F-test of Null Hypothesis of No Fixed Effects	<0.01	-	<0.01	-	<0.01	-	<0.01	-	<0.01	-
p-value for Hausman Test of Random Effects	-	0.08	-	0.11	-	0.07	-	0.14	-	0.13

Numbers in parenthesis are standard errors. ***, ** and * indicate significance at 1%, 5% and 10% levels, respectively. There is no OECD intercept dummy included in the estimates because that variable is absorbed in country-specific effects. The intercept in Fixed-Effects model is identified by setting the fixed effect for the last country in the sample to zero.

Table 10:
Cross-Section (Between) Estimates for Gross Tertiary Education

Number of Observations: 37

Variable	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Intercept (α)	24.24 (23.79)	38.64 (29.51)	42.81 (30.88)	38.12 (30.28)	25.75 (31.05)	46.80 (62.56)
Gini coefficient (%) (β_1)	-0.547** (0.254)	-0.555** (0.256)	-0.614** (0.282)	-0.559** (0.262)	-0.588 (0.593)	-0.731 (1.108)
Log Per Capita Consumption Expenditurec	5.194 (2.832)	2.554 (4.261)	1.987 (4.442)	2.577 (4.335)	5.236 (2.929)	2.565 (4.407)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	-0.086 (0.211)	-0.125 (0.217)	-0.086 (0.232)	-0.103 (0.282)	-0.141 (0.752)	-0.403 (1.903)
Percentage of Urban Population (β_4)	0.056 (0.233)	0.142 (0.256)	0.166 (0.263)	0.142 (0.26)	0.055 (0.236)	0.144 (0.265)
OECD (β_5)		7.216 (8.67)	-5.856 (26.167)	8.912 (16.354)		5.239 (28.417)
OECD*Gini (β_6)			0.422 (0.796)			
OECD*Ed.Spending (β_7)				-0.056 (0.454)		0.078 (0.959)
Ed.Spending*Gini (β_8)					0.001 (0.017)	0.006 (0.036)
R-squared	44.6%	45.9%	46.4%	45.9%	44.7%	45.5%
White Heteroskedasticity test (p-value)	0.61	0.44	0.36	0.50	0.39	0.36

Numbers in parenthesis are standard errors.
***, ** and * indicate significance at 1%, 5% and 10% levels, respectively.
Estimates were obtained using OLS

Table 11: Panel Estimates for Gross Tertiary Education

Number of Observations: 147. Number of Countries:37

Variable	Model 1 Fixed	Model 1 Random	Model 2 Fixed	Model 2 Random	Model 3 Fixed	Model 3 Random	Model 4 Fixed	Model 4 Random	Model 5 Fixed	Model 5 Random
Intercept (α)	-456.2*** (64.3)	-232.2*** (40.2)	-447.6*** (63.7)	-248.7*** (39.5)	-453.5*** (64.9)	-233.1*** (41.2)	-457.6*** (64.6)	-234.4*** (40.6)	-456.2*** (65.7)	-243.4*** (43.6)
Gini coefficient (%) (β_1)	0.135 (0.186)	0.042 (0.171)	0.383 (0.267)	0.324 (0.222)	0.121 (0.189)	0.043 (0.173)	0.238 (0.273)	0.172 (0.258)	0.212 (0.332)	0.276 (0.312)
Log. Per Capita Consumption Expenditure (β_2)	37.81*** (4.74)	29.63*** (3.92)	38.84*** (4.72)	33.12*** (4.05)	37.72*** (4.76)	29.69*** (3.99)	38.52*** (4.95)	30.13*** (4.03)	38.35*** (5.13)	31.09*** (4.36)
Ed. Spending per Student (as a % of per capita GDP) (β_3)	-0.315*** (0.090)	-0.405*** (0.083)	-0.289*** (0.089)	-0.366*** (0.083)	-0.339*** (0.106)	-0.402*** (0.099)	-0.161 (0.311)	-0.219 (0.287)	-0.201 (0.427)	-0.054 (0.399)
Percentage of Urban Population (β_4)	1.934*** (0.646)	0.699 (0.471)	1.947*** (0.639)	0.502 (0.444)	1.896*** (0.655)	0.704 (0.474)	1.805** (0.695)	0.589 (0.491)	1.815** (0.702)	0.549 (0.495)
OECD*Gini (β_6)			-0.703* (0.378)	-0.907*** (0.296)						
OECD*Ed.Spending (β_7)					0.083 (0.198)	-0.009 (0.179)			0.034 (0.247)	-0.135 (0.225)
Ed.Spending*Gini (β_8)							-0.004 (0.007)	-0.004 (0.006)	-0.003 (0.009)	-0.007 (0.008)
R-Squared	97.7%	46.9%	97.8%	49.1%	97.7%	46.9%	97.7%	46.9%	97.7%	47.0%
p-value for F-test of Null Hypothesis of No Fixed Effects	<0.01	-	<0.01	-	<0.01	-	<0.01	-	<0.01	-
p-value for Hausman Test of Random Effects	-	<0.01	-	<0.01	-	<0.01	-	<0.01	-	<0.01

Numbers in parenthesis are standard errors. ***, ** and * indicate significance at 1%, 5% and 10% levels, respectively. There is no OECD intercept dummy included in the estimates because that variable is absorbed in country-specific effects. The intercept in Fixed-Effects model is identified by setting the fixed effect for the last country in the sample to zero.

Table 12:
Data Availability for Net Primary Education Regressions

Country	1991	1998	1999	2000	2001	2002	2003	2004	Total
ARG		X	X						2
AUS		X		X	X	X			4
AZE				X	X	X			3
BGR					X	X	X		3
BOL			X	X		X			3
BRA			X		X	X			3
CHE				X	X	X			3
COL		X	X	X					3
DNK			X	X	X	X			4
ESP			X	X	X	X	X		5
EST			X		X	X	X		4
FIN	X		X	X	X	X	X		6
FRA			X	X	X	X	X		5
GBR	X	X	X	X	X	X	X		7
GRC		X	X	X	X				4
HUN	X	X	X	X		X	X		6
IRL		X	X	X	X		X		5
ITA	X		X	X	X	X			5
KGZ			X	X	X	X			4
MDA				X	X	X			3
NLD	X		X		X	X	X		5
NOR	X	X	X	X	X	X	X		7
PAN					X	X		X	3
PER		X			X	X	X		4
POL	X	X				X	X		4
SLV						X	X		2
SVN	X				X	X	X		4
SWE	X		X	X	X	X	X		6
UKR					X	X	X		3
USA		X	X	X	X	X	X		6
N=30	9	11	19	19	24	26	17	1	126

Table 13:
Data Availability for Gross Primary Education Regressions

Country	1998	1999	2000	2001	2002	2003	2004	Total
ARG	X	X	X	X	X	X		6
AUT		X	X	X		X		4
AZE	X		X	X	X			4
BGR				X	X	X		3
BOL		X	X		X			3
CHE			X	X	X			3
CHL	X		X			X		3
COL	X	X	X				X	4
CZE		X	X	X	X			4
DEU		X	X	X	X	X		5
DNK		X	X	X	X			4
ESP		X	X	X	X	X		5
EST		X		X	X	X		4
FIN		X	X	X	X	X		5
FRA		X	X	X	X	X		5
GBR	X	X	X	X	X	X		6
GRC	X	X	X	X		X		5
HUN	X	X	X		X	X		5
IRL	X	X	X	X		X		5
ITA		X	X	X	X			4
KGZ		X	X	X	X			4
LVA		X	X	X	X	X		5
MDA			X	X	X			3
NLD		X		X	X	X		4
NOR	X	X	X	X	X	X		6
PAN				X	X		X	3
PER	X			X	X	X		4
POL	X				X	X		3
PRT		X	X	X				3
PRY				X	X	X		3
SLV	X	X	X		X	X		5
SVK		X	X	X	X	X		5
SWE		X	X	X	X	X		5
THA	X		X	X				3
URY			X	X	X	X		4
USA	X	X	X	X	X	X		6
N=36	14	25	29	30	29	24	2	153

Table 14:
Data Availability for Net Secondary Education Regressions

Country	1998	1999	2000	2001	2002	2003	2004	Total
ARG	X	X	X	X	X	X		6
AZE			X	X	X			3
BGR				X	X	X		3
BRA		X		X	X			3
CHE			X	X	X			3
COL	X	X	X				X	4
DNK		X	X		X			3
ESP		X	X	X	X	X		5
EST				X	X	X		3
FIN		X	X	X	X	X		5
FRA		X	X	X	X	X		5
GBR	X	X	X	X	X	X		6
GRC	X	X	X	X		X		5
HUN	X		X		X			3
IRL	X	X	X	X		X		5
ITA		X	X		X			3
MDA			X	X	X			3
NLD		X		X	X	X		4
NOR	X	X	X	X	X	X		6
PAN				X	X		X	3
PER	X			X	X	X		4
PRT		X	X	X				3
SLV	X		X		X			3
SWE		X	X		X	X		4
UKR			X	X	X	X		4
USA	X	X		X	X	X		5
N=26	10	16	19	20	22	15	2	104

Table 15:
Data Availability for Gross Secondary Education Regressions

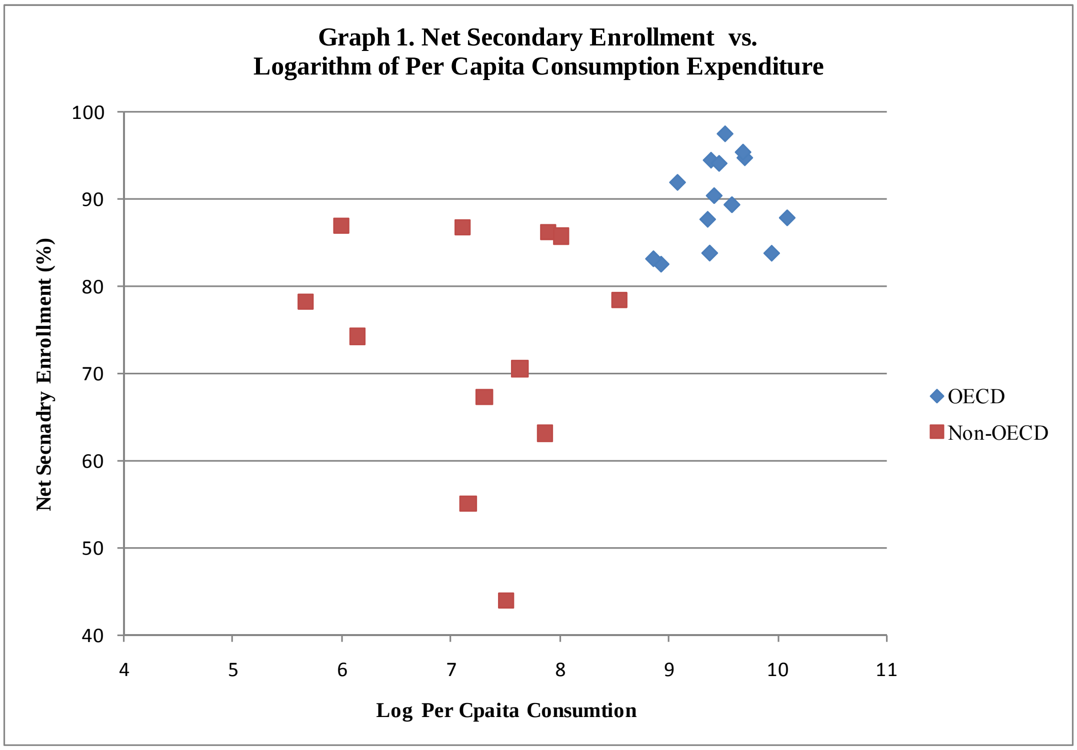
Country	1998	1999	2000	2001	2002	2003	2004	Total
ARG	X	X	X	X				4
AUT		X	X	X		X		4
AZE	X		X	X	X			4
BGR				X	X	X		3
BOL		X	X		X			3
BRA		X		X	X			3
CHE			X	X	X			3
CHL	X		X			X		3
COL	X	X	X				X	4
CZE		X	X	X	X			4
DEU		X	X	X	X	X		5
DNK		X	X	X	X			4
ESP		X	X	X	X	X		5
EST		X		X	X	X		4
FIN		X	X	X	X	X		5
FRA		X	X	X	X	X		5
GBR	X	X	X	X	X	X		6
GRC	X	X	X	X		X		5
HUN	X	X	X		X	X		5
IRL	X	X	X	X		X		5
ITA		X	X	X	X			4
KGZ		X	X	X	X			4
LVA		X	X	X	X	X		5
MDA			X	X	X			3
NLD		X		X	X	X		4
NOR	X	X	X	X	X	X		6
PAN				X	X		X	3
PER	X			X	X	X		4
PRT		X	X	X				3
PRY				X	X	X		3
SLV	X		X		X	X		4
SVK		X	X	X	X	X		5
SVN				X	X	X		3
UKR			X	X	X	X		4
URY			X	X	X	X		4
USA	X	X		X	X	X		5
N=36	12	24	27	31	29	23	2	148

Table 16:
Data Availability for Gross Tertiary Education Regressions

Country	1998	1999	2000	2001	2002	2003	2004	Total
ARG	X	X	X	X				4
AUT		X		X		X		3
AZE			X	X	X			3
BGR				X	X	X		3
BOL		X	X		X			3
BRA		X		X	X			3
CHE			X	X	X			3
CHL	X		X			X		3
COL	X	X	X				X	4
CZE		X	X	X	X			4
ESP		X	X	X	X	X		5
EST		X		X	X	X		4
FIN		X	X	X	X	X		5
FRA		X	X	X	X	X		5
GBR	X	X	X	X	X	X		6
GRC	X	X	X	X				4
HUN	X	X	X		X	X		5
IRL	X	X	X	X		X		5
ITA		X	X	X	X			4
KGZ			X	X	X			3
LTU				X	X	X		3
LVA		X	X	X	X	X		5
MDA			X	X	X			3
NLD		X		X	X	X		4
NOR	X	X	X	X	X	X		6
PAN				X	X		X	3
PER				X	X	X		3
POL		X	X	X	X	X		5
PRT		X	X	X				3
PRY				X	X	X		3
SLV	X		X		X	X		4
SVK		X	X	X	X	X		5
SVN				X	X	X		3
SWE		X	X	X	X	X		5
UKR			X	X	X	X		4
URY			X	X	X	X		4
USA	X	X		X	X	X		5
N=37	10	23	26	32	30	24	2	147

Table 17:
List of OECD and non-OECD Countries Used in the Sample

Non-OECD		OECD	
Country Code	Country Name	Country Code	Country Name
ALB	Albania	AUS	Australia
ARG	Argentina	AUT	Austria
ARM	Armenia	BEL	Belgium
AZE	Azerbaijan	CAN	Canada
BGR	Bulgaria	CHE	Switzerland
BOL	Bolivia	DNK	Denmark
BRA	Brazil	ESP	Spain
CHL	Chile	FIN	Finland
CHN	China	FRA	France
COL	Colombia	GBR	United Kingdom
CZE	Czech Republic	GRC	Greece
EST	Estonia	IRL	Ireland
HUN	Hungary	ITA	Italy
ISR	Israel	JPN	Japan
KGZ	Kyrgyz Republic	KOR	Korea, Republic of
LTU	Lithuania	NLD	Netherlands
LVA	Latvia	NOR	Norway
MDA	Moldova	PRT	Portugal
MEX	Mexico	SWE	Sweden
MKD	Macedonia, FYR	USA	United States
MUS	Mauritius		
PAN	Panama		
PER	Peru		
PHL	Philippines		
POL	Poland		
PRY	Paraguay		
SLV	El Salvador		
SVK	Slovak Republic		
SVN	Slovenia		
THA	Thailand		
UKR	Ukraine		
URY	Uruguay		


Graph 1. Net Secondary Enrollment vs.
Logarithm of Per Capita Consumption Expenditure
100
90
80
70
60
50
40
4
5
6
7
8
9
10
11
Net Secnadry Enrollment (%)
Log Per Cpaita Consumtion
OECD
Non-OECD