5-2008

Three Essays on Environmental Economics and International Trade

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THREE ESSAYS ON ENVIRONMENTAL ECONOMICS AND INTERNATIONAL TRADE

A Dissertation
Presented to
the Graduate School of
Clemson University

In Partial Fulfillment
of the Requirements for the Degree
Doctor of Philosophy
Applied Economics

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by
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May 2008

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ABSTRACT

This dissertation addresses the broad topic of appropriate metrics, proxies, and estimation methods in environmental economics and international trade research, presented as three separate studies. The first, entitled, "Something in the Water? Testing for Groundwater Quality Information in the Housing Market," examines how informed real estate markets are with respect to groundwater quality by using a couple of different proxies for groundwater quality in a hedonic framework. This research topic has potentially suffered from imperfect proxies and incomplete information, which I test. In the second, entitled, "Do Economic Integration Agreements Actually Work? Issues in Understanding the Causes and Consequences of the Growth in Regionalism," I address a topic in international trade that has consistently suffered from endogeneity biases in estimations: the effect of economic integration agreements on bilateral trade flows. The third study, called "Trade Flow Consequences of the European Union's Regionalization of Environmental Regulations," synthesizes the fields of environmental economics and international trade. I introduce a new proxy – survey data – that does not rely on environmental outcomes and thus hopefully avoids endogeneity. Controlling for any possible interaction effect between environmental regulation stringency and European Union membership, I estimate the effect of increasing environmental regulation stringency on trade flows to and from three groups of countries: high income countries, low income countries, and all countries.

ACKNOWLEDGMENTS

I wish to acknowledge all my committee members' assistance in my development as an economist in general and in their guidance in producing this manuscript. In particular, I thank: Dan Benjamin for tirelessly subjecting my work to sound scientific analysis; Scott Baier for giving me my first good paper idea; Bentley Coffey for his absolute genius in turning abstract ideas into structural and testable models; and Bob Tollison for keeping me focused on the fundamentals and encouraging me.

Chapter 2 of this manuscript was partially produced at the Property and Environment Research Center in Bozeman, Montana, whose assistance I gratefully acknowledge.

Chapter 3 stems from a paper coauthored with Scott Baier, Jeffrey Bergstrand, and Peter Egger, that has not yet been published. Their contributions are acknowledged, and for publication purposes, we are all coauthors on the paper.

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CHAPTER ONE

INTRODUCTION

This dissertation addresses the broad topic of appropriate metrics, proxies, and estimation methods in environmental economics and international trade research. Chapters two, three, and four present research into three seemingly diverse subjects. The first of the three research subjects is estimating the effect of groundwater contamination on real estate prices. The second subject is the estimation of the effect of bilateral economic integration agreements on trade flows. The third subject, at the intersection of environmental economics and international trade, is estimation of the effect of environmental regulations on trade flows inside and outside economic integration agreements.

Proxies and estimation issues in environmental economics

Chapter two, entitled, "Something in the Water? Testing for Groundwater Quality Information in the Housing Market," examines how informed real estate markets are with respect to groundwater quality by using a couple of different proxies for groundwater quality in a hedonic framework. Houses are usually sold bundled with property rights to groundwater access, and contamination of a house's groundwater source diminishes the value of that house. Researchers have often tried to assess the economic damages caused by environmental disamenities such as groundwater contamination, air quality degradation, and elevated ambient noise levels by inserting some variable measuring the disamenity in a hedonic model. The metrics of these disamenities, however, generally have been proxies, such as a house's distance from a contamination source, rather than some measurement that takes into account the likely migration path of contaminants. In the case of groundwater, contaminants typically do not migrate in all directions away from the source at the same speed; rather, the distribution of contaminants at each event depends on the hydrologic and geologic realities of the vicinity. If market participants are fully informed of the levels of contamination in the groundwater below each house, then using distance from contamination as a proxy likely biases downward estimates of economic damages from the contamination. Conversely, it is possible that market participants are not fully informed of where the contamination is; in such a case, distance from a contamination source or some other proxy might represent market participants' best guess as to where the contamination is, and, as a result, would serve well as a determinant of house value in a hedonic estimation.

I use data on which houses were chosen by governmental regulators to have their groundwater quality tested in an area with potentially contaminated groundwater near St. Paul, Minnesota; with these data, I create a new proxy for groundwater contamination that differs from distance as a proxy because it takes into account the groundwater migration path of the contaminants. Controlling for other features that likely contribute or detract from house value, I test whether this proxy – a binary variable indicating whether a house had its groundwater tested – is a significant determinant of house value. The estimate of the effect of this proxy on house value turns out to be negative and significant prior to the passage of legislation that requires sellers in the area to inform potential buyers of the potential contamination of groundwater in the vicinity. After the new legislation, however, the estimate of the effect of being tested becomes statistically insignificant; instead, the estimate on the value of house location in the geographic area created by the new legislation decreases relative to pre-legislation levels. Thus, prior to the existence of the legislation, the market appeared to be relatively well-informed about the location of the contamination; after the passage of the legislation, information levels changed, and all houses, regardless of the contaminant's migration path, in the vicinity were treated by the market as if they had contaminated water. I interpret this as a result of a change in the cost of information-gathering: the legislation provided a cheap, if imperfect, information source to market participants.

Proxies and estimation issues in international trade

In the second chapter, entitled, "Do Economic Integration Agreements Actually Work? Issues in Understanding the Causes and Consequences of the Growth in Regionalism," I address a topic in international trade that has consistently suffered from endogeneity biases in estimations: the effect of economic integration agreements on bilateral trade flows. I show that traditional gravity equations estimations of the effects of the European Union and other major European free trade agreements have been implausibly low. Because countries select into free trade agreements, it is not an exogenous treatment. Using bilateral fixed effects and first-differencing, I overcome the endogeneity bias and deliver consistent and plausible estimates of the effects of joining these trade agreements on bilateral trade flows, estimates that are much higher than those of "traditional" gravity estimates.

Environmental economics and international trade

The third chapter, called "Trade Flow Consequences of the European Union's Regionalization of Environmental Regulations," synthesizes the fields of environmental economics and international trade. Environmental regulations could both theoretically and empirically affect trade flow patterns between countries. Still, our understanding of their effect has been hindered for three main reasons. First, researchers investigating this effect have ignored the possibility of an interaction effect between economic integration agreements and environmental regulations. Second, many studies on this topic have suffered from endogeneity both in their measures of environmental regulation stringency and in their inclusion of economic integration agreement variables without bilateral fixed effects. Third, despite the fact that regional groups of countries do sometimes impose environmental regulations on themselves, researchers have not analyzed whether certain segments of the regional group – for example, high income countries – stand to gain from such impositions.

I introduce a new proxy – survey data – that does not rely on environmental outcomes and thus hopefully avoids endogeneity. Controlling for any possible interaction effect between environmental regulation stringency and European Union membership, I estimate the effect of increasing environmental regulation stringency on trade flows to and from three groups of countries: high income countries, low income countries, and all countries. I find that an increase in environmental regulation stringency in low income countries leads to a decrease in exports if that country is a European Union member; otherwise, there is no significant effect. Conversely, a similar change in high income countries actually increases exports, and this increase is larger if the country is a European Union member.

CHAPTER TWO

SOMETHING IN THE WATER? TESTING FOR GROUNDWATER QUALITY INFORMATION IN THE HOUSING MARKET

Introduction

In 1988, an extensive plume of trichloroethylene (TCE), which the US Environmental Protection Agency lists as a potential carcinogen, was discovered in the groundwater in the area of Baytown Township, in Washington County, Minnesota. Many houses and businesses in the area of the plume rely on groundwater as the primary source for water consumption, so the Minnesota Pollution Control Agency (MPCA) and the Minnesota Department of Health (MDH) subsequently took actions to limit human exposure to TCE and to prevent further spread of the contaminant plume. The contaminant plume is an environmental disamenity that might negatively affect real estate prices in the area. Well-water measurements of TCE levels by the MPCA, as well as other actions taken by MPCA and MDH, contain information that might affect real estate values in different ways, and these effects might not be limited to only those houses situated on the plume. If the real estate market is completely informed about the whereabouts of the plume, then any negative effect on real estate prices should occur only where houses have contaminated water or are likely to have contaminated water in the future. Conversely, if the market is incompletely informed about the whereabouts of the plume, then houses whose groundwater will likely never be affected by Baytown Township TCE plume could experience a loss in property value.

In this paper, I test for the presence of complete information, incomplete information, or no information regarding the quality of groundwater in the residential real estate market near the Baytown Township Groundwater Contamination Superfund Site (Baytown Site or the Site). I also test the effects of some state regulations regarding the Site on information levels and, correspondingly, prices in the real estate market. The hypotheses that I test are:

Hypothesis 1: There is no information regarding groundwater quality priced into the market (this is observationally equivalent to the hypothesis that market participants simply do not value groundwater quality).

Hypothesis 2: There is incomplete information regarding groundwater quality priced into the market. This hypothesis might hold if market participants rely on imperfect proxies such as distance from the contaminant source or state and local regulations for information about present and future groundwater quality.

Hypothesis 3: There is complete information regarding groundwater quality priced into the market. This hypothesis might hold if participants rely on groundwater tests at each house for information about present groundwater quality and participants are able to reliably predict future groundwater quality.

One focus of this paper is determining whether governmental regulations regarding the Baytown Site induce some market reaction. For example, one regulation established regarding the Baytown Site was special well construction area (SWCA) legislation, passed in 1988 and subsequently revised in 2002. The SWCA legislation and a later disclosure statute related to it could affect the real estate market in two ways. The first is the geographic delineation of the area that would be included in the SWCA; any work done on wells inside this area required special permits and inspections, increasing the cost of new well construction. The geographic boundaries of the SWCA do not match the edges of the groundwater contamination plume, and, furthermore, the boundaries of the SWCA changed once in accordance to changes in regulatory policies regarding the relative toxicity of TCE. Market participants might rely on the delineation of the SWCA as a proxy for the probability that a house has contaminated groundwater, negatively affecting real estate value inside the SWCA. The second way the SWCA might, indirectly, affect the real estate market is through a Minnesota statute passed in 2003 requiring sellers of property in Washington County to disclose to prospective buyers if the property is located within the SWCA. It is possible that market participants did not possess information about the SWCA prior to the disclosure law, or that market participants interpreted the creation of a SWCA disclosure law as a signal that all houses in the SWCA might possess contaminated water. If the disclosure law either added new information regarding groundwater quality into the housing market or lowered the cost of information gathering, house prices in the SWCA might respond accordingly.

The results indicate that during the period from 1995 – 2002, prior to the passage of the disclosure law, the market was well-informed about the location of the contaminant plume. Houses that are in the SWCA but not at risk of contamination do not suffer any loss in property value, while those that are at risk of contamination do. In the period from 2003 – 2006, after the passage of the disclosure law, houses in the SWCA that are not at risk of contamination lose property value, relative to the previous period. Houses that are at risk of contamination suffer no more loss in property value than other homes (assumed to not be at risk) in the SWCA after the passage of the disclosure law. These results imply that market participants used the lowest-cost information – namely, whether a house was located in the SWCA – available regarding groundwater quality in making buying and selling decisions, even though that information was an imperfect proxy for the real distribution of risk. Alternatively, these results could indicate that market participants interpreted the disclosure law as a signal that the contaminant plume might expand, even though the plume has been relatively stable for many years.

Background

In the residential real estate market, some information about the valued components of a house is readily observable and quantifiable: a prospective buyer can tour a house, count the number of bedrooms, and test the functionality of the bathrooms at a relatively low cost. Conversely, some components of the house are not so easily observable. For example, a prospective buyer would find it difficult to predict whether the neighborhood will offer the proper level of "peace and quiet" without spending a few weekends in the house listening for raucous neighbors, and prospective buyers regularly rely on expert house inspectors for information regarding structural integrity and whether termites have ever infested the house.

One implicitly-owned component of a house that is costly to observe is the quality of the groundwater beneath the property. Groundwater quality presumably would be an important aspect of houses with private wells that tap into a potentially contaminated aquifer for water consumption. Also, groundwater quality could be important even if the house had a municipal water supply, because the possibility of exposure to groundwater-borne contaminants through some other medium than consumption exists. While the present quality of groundwater at a given house can be tested at some expense, it is much more difficult to predict the future quality of groundwater at that house. One way to predict the future quality level of groundwater when there is a known contaminant source is to use a groundwater contaminant transport model (see McLaughlin and Coffey, 2007, for an example). Such a model requires detailed information about the hydrological and geological features in an area, as well significant scientific and mathematical expertise. As an alternative to testing the present quality of groundwater or predicting future quality levels with a groundwater contaminant transport model, market participants may rely on proxies and signals to inform them of quality levels. There are many possible sources of information about groundwater quality at a house; some, such as actual well sample tests and effective groundwater contaminant transport models, are more accurate sources than others, such as taste, smell, newspaper articles, word of mouth, or legislated zones like the special well construction area (SWCA). The less accurate sources might indeed create a misperception of health risk from consumption of groundwater if the information conveyed does not reflect the actual present and probable future groundwater contamination status. Conveyance of incomplete information or of incorrect information regarding groundwater contamination might induce market reactions where none would have occurred, had there been complete information.

In the case of a publicly-known groundwater contamination site, such as a Superfund site, market participants (and particularly prospective buyers) might use some proxy for the probability of groundwater contamination, such as distance from the contaminant source, if the source is known. Market participants might alternatively rely on other proxies where and when they exist, such as the aforementioned SWCA or other government legislation requiring prospective buyers to be informed of a house's proximity to a groundwater contamination source. Such proxies are usually imperfect; groundwater contaminants do not normally migrate away from their source at equal speeds in all directions, nor do they tend to follow county or township borders inside which legally created signals such as the SWCA exist. For instance, a house might be situated a very short distance from a contaminant source yet have almost zero probability of groundwater contamination from that source because contaminants are transported away from the house. In such a case, using distance from the contaminant source as a proxy for the probability of groundwater contamination from that source would result in an overestimation of that probability.

Baytown Site History

This paper focuses on the level of information regarding groundwater quality that is present in the real estate market surrounding the Baytown Site at different points in time and how that information is priced into the market for houses. Figure 1 shows the location of Washington County in Minnesota. Table 1 details a chronology of Baytown Site events, and this section provides a brief history of the Site.

In 1987, a Minnesota Department of Health (MDH) sampling of wells near a landfill at Stillwater Prison showed the presence of trichloroethylene (TCE) and carbon tetrachloride (CCl_4) in the groundwater. Subsequent testing showed that CCl_4 was not widespread or at high levels, and the primary contaminant of concern became TCE. People who drink water containing TCE in excess of five micrograms per liter over many years could experience liver problems and may have increased cancer risk (U.S. Environmental Protection Agency, 2006). The Minnesota Pollution Control Agency tracked the plume to the Lake Elmo Airport, and in May 1988, the Minnesota Department of Health (MDH) designated an area of Washington County including the known plume and its vicinity as a special well construction area (SWCA). One MDH document states, "The SWCA informs well owners and drillers about the potential for contaminated ground water [sic] in the area and serves to prevent further degradation of the aquifer by requiring proper construction of new wells" (MDH 2004, 10). The Baytown Site was added to Minnesota's State Superfund Permanent List of Priorities, while the federal Environmental Protection Agency (EPA) added the site to the Superfund National Priorities List in 1995.

In 1988, because the Minnesota Pollution Control Agency (MPCA) had tracked the plume to Lake Elmo Airport, MPCA issued a formal request for information to the Metropolitan Airports Commission (MAC). MAC voluntarily investigated groundwater beneath the airport from 1988 though 1991, and found significant quantities of TCE below the airport in two aquifers, the Prairie du Chien aquifer and the Jordan aquifer, both of which are used for drinking water. MAC was declared a responsible party in 1991, blamed for the TCE contamination, and together with MPCA conducted further investigations from 1992 to 1998. Finally, in 2000, based on a feasibility study finished in 1999, MPCA decided to install point-of-use granulated activated carbon (GAC) filter systems on certain private wells as the primary remedial action. Tests performed by the MPCA on post-filter samples indicate that the GAC filter systems effectively reduce TCE to below laboratory reporting limits, "indicating that [GAC filtered-]well users were not exposed to the contaminants" (MPCA, 2007, 9). At this point in time, although the source was suspected to be physically underneath the airport, no location had been pinpointed; thus remedial action could not include removing or treating the source. Houses that had water with TCE above 30 μg/L received GAC filters at this time; 30 μg/L was the Minnesota Department of Health's human risk limit, or maximum level allowable without treatment, until 2002. Six filter systems were installed under this policy prior to 2002. A change in the human risk limit in 2002 (addressed in detail in section 2.3) resulted in many more houses receiving GAC filters, at MPCA's expense. As of March 2007, a total of 162 GAC filters had been installed and paid for by MPCA. Houses that were built on parcels platted after April 9, 2002, and that had TCE measured above 5 μg/L in private wells were required to install GAC filter systems, but the MPCA did not pay for these.

From 2002 to 2004, MPCA conducted additional soil and groundwater tests west of the airport in an attempt to locate the primary contaminant source. As a result, the primary source was thought to lie below Hagberg's Country Market, about ¾ mile west of the airport. From 1940 to 1968, a metal-working facility occupied this property, and it is suspected that TCE was used as a degreaser at this facility and subsequently released into the groundwater. A remedial plan for treatment of the source had been investigated but not yet implemented as of August 2007. It is still not certain if the primary source has been found, nor is it known if that is the only source.

The TCE plume was characterized in March 2007 as approximately five miles long, covering seven square miles in central Washington County. Approximately 650 homes and several businesses rely upon private wells that tap into the contaminated aquifers. The MPCA monitors public sentry wells (monitoring wells) and many private wells in the area, and offers bottled water to residents whose wells exceed the human risk limit (HRL) until GAC filter systems can be installed. Policies regarding where and when GAC filter systems are installed and who pays for them were set by MPCA and MDH. Between 2000 and 2002, the policy was that any home with measured TCE above the HRL of 30 μg/L (micrograms per liter) had a whole house GAC filter system installed at MPCA's expense[1]. The HRL was changed to 5 μg/L in 2002; as a result, policy changed. The new policy was that any house with TCE measured above 5 μg/L and with a parcel platted prior to April 9, 2002, received a GAC filter system from MPCA. Houses platted after that date and that had TCE above 5 μg/L were required to purchase their own GAC filter systems.

Special Well Construction Area

Legislative and institutional controls might be a source of information for real estate market participants. One institutional control established regarding the Baytown

[1] The plume's source was originally thought to be the Lake Elmo Airport, and the Metropolitan Airport Commission (MAC) was the only potentially responsible party. As such, MAC voluntarily conducted various investigations and feasibility studies from 1988 to 2001 and helped pay for GAC systems on those houses with high TCE levels. Subsequently, the source was discovered to be farther west and merely migrating beneath the airport. MAC is investigating options to recover the money it "voluntarily" spent.

site was the special well construction area (SWCA) legislation, passed in 1988 and subsequently revised in 2002. There are two important facts related to the SWCA that could affect the real estate market. The first is the delineation of the area that would be included in the SWCA. The second is a Minnesota statute (Minn. Statute 103I.236) passed in 2003 that required sellers of property in Washington County to disclose to prospective buyers whether the property is located in the SWCA, if the property is not served by a municipal water system or if the property contains an unsealed well.

The original SWCA legislation, created in 1988, identified a geographic area encompassing the known plume itself as well as extra buffer area around the plume. At that time, the EPA had determined that the maximum contaminant level (MCL) allowable in drinking water for TCE was 30 micrograms per liter (μg/L); Minnesota Department of Health (MDH) had adopted the same standard, setting its human risk level at 30 μg/L. Accordingly, the primary goal for MDH was to ensure that residents were not consuming water with TCE above the accepted human risk level, and created the original SWCA with this human risk level in mind. Later, however, regulators expanded the SWCA both as additional testing discovered the full extent of TCE migration and as EPA policy regarding the maximum contaminant level for TCE changed. The SWCA is a geographic area within which there exist substantial limitations on the construction, sealing, repair, and location of wells (Minn. Rule 4725.3650; US EPA 2007, 10; MPCA 2007, 3). Per conversations with well drillers licensed to drill in and out of the SWCA in Washington County (McCullough, phone conversation on 10/31/07; Sampson, phone conversation on 11/1/07), the costs of drilling a new residential well inside the SWCA range from $5,000 to $20,000 more than drilling a well just outside of the SWCA border. This cost increase arises primarily because of licensing restrictions and additional inspections; the equipment used and the depth of drilling are virtually identical inside and, for example, one-half a mile outside the SWCA.

Importantly, house location in the SWCA does not always imply any significant increased risk of groundwater contamination, compared to house location outside the SWCA. The original SWCA is shown in Figure 2, and the expanded SWCA is shown in Figure 3. Additionally, Figures 4 and 5 show the expanded SWCA in relation to the 5 μg/L plume contours in years 1999, 2001, 2002, and 2003. The SWCA is presently a 12.5 square-mile area surrounding the Baytown Site and does not perfectly match the known contaminant plumes at this site. According to a Minnesota Department of Health summary of the Baytown site published in April 2006, "The SWCA includes a generous border area outside the plume. Many wells within the SWCA are too far from the plume to be affected [by TCE contamination]" (MDH, 2006, 1). Furthermore, according to conversation with a Minnesota Pollution Control Agency (MPCA) hydrologist intimately familiar with the site, the SWCA includes some houses that have a "very, very low probability" of ever having their groundwater contaminated from the Baytown Site's contaminant source. As Figure 4 and Figure 5 show, the SWCA is drawn along county quadrant and half-quadrant borders, while the plume itself is not nearly so well-behaved.

The second component of the SWCA is the statutory requirement of disclosure to prospective buyers whether the property is located in the SWCA, if it is not served by a municipal water supply or has an unsealed well. Passed in 2003, this statute may have changed the amount or nature of information present in the market regarding groundwater quality. One goal of this paper is to determine whether there is a difference in the effect of actual groundwater contamination on house prices and the effect of location in the SWCA, because a house could be in the SWCA and not have any TCE contamination problems whatsoever. The question is: if the market reacts to possible water contamination, does it react only where houses have a reasonable possibility of water contamination, or does it react to the larger, legislatively-defined zone, the SWCA, in which some houses do not a reasonable possibility of water contamination? It is worth noting that, in this paper, having a “reasonable possibility of water contamination” is based off of MPCA investigations and conclusions. The market could, of course, have a different opinion about which houses have a “reasonable possibility of water contamination.”

Changing the Human Risk Limit

Other legislation might have effects as well. In January 2002, responding to a draft US EPA health risk assessment for TCE, Minnesota Department of Health changed the human risk limit (HRL) from 30 μg/L (micrograms per liter) to 5 μg/L. This resulted in an increase in the area of concern; the change in the HRL directly caused the expansion of the SWCA discussed in section 2.2. Many more residential wells suddenly were classified as having groundwater with TCE above the HRL, in accordance with the newly adopted limit. Anecdotal evidence gleaned from local newspaper articles and conversations with residents indicates that residents reacted with some trepidation. There are accounts of residents using bottled water for all domestic (including pet) consumption despite having brand new GAC filters installed (AP 2002b,1) and other residents worrying that the water they consumed while the HRL was at 30 μg/L would have long-term negative health consequences (AP 2002a,1). In fact, the change in HRL and the consequential expansion of the Baytown site's area of concern generated more newspaper articles than any other single event related to this site. If nothing else, the increased media coverage probably created a greater public awareness of the possibility of groundwater contamination in the area.

This change in the human risk limit might have consequences in the real estate market. For one, the change might induce people to mistrust any human risk limit for TCE determined by governmental agencies. As a result, even if Minnesota Pollution Control Agency (MPCA) declares that some houses have no reasonable probability of future contamination, potential buyers might still believe that those houses do face some risk. Also, as a result of this change, market participants might conclude that no human risk limit set by the MPCA is reliable, that homes that are outside the SWCA might eventually be inside the SWCA, or that the plume will spread in the future. Because the SWCA was expanded once before, it might be expanded again, and because the human risk limit was lowered once, it might be lowered again. Homebuyers considering moving into Washington County might conclude that the real price of property with a private well in the county is the nominal price plus the cost of filtered water.

Second, the lower HRL resulted in many more houses qualifying for MPCA-financed GAC filters; houses that existed on property platted before April 9, 2002, and had private wells using water above the HRL received MPCA-financed GAC filters. On average, each filter costs $1500 to install and $450 every two to six years for change-out and maintenance (MPCA, 2007b, 10; MPCA 2007b, 2). Any wells installed on property platted after April 9, 2002, however, that tapped water above the HRL would be required to have GAC systems that would not be financed by MPCA.

Methods and Data

I follow Rosen's (1974) hedonic model, which assumes that consumers maximize utility by choosing the characteristics of the house they buy, given a competitive housing market with a continuous equilibrium price schedule for house characteristics. Consumers can affect the price they pay by choosing which house and bundled characteristics they buy, but they cannot affect the equilibrium price schedule (Palmquist, 2003, 3 – 4). Consumers are therefore price-schedule-takers.

Empirically, the hedonic model is estimated by using data on the prices of houses and their characteristics, such as bedrooms, bathrooms, square footage, etc. One innovation of this research is the inclusion of multiple variables which measure information regarding the probability that a particular house has or will eventually have contaminated groundwater. These variables, m_{it} and s_{it}, standing for "measured and not filtered" and "SWCA," indicate whether a house had a TCE reading of its well water done (as reported by the Minnesota Pollution Control Agency) at any time prior to the sale and whether a house is located inside the SWCA, respectively[2]. The hedonic model attempts to predict house prices by quantifying and estimating the marginal prices of all observable house characteristics, while assuming there is an unobserved stochastic

[2] I use "measured and not filtered" as a variable rather than the more obvious variable, "measured TCE level," because of the possibility of measurement error in the latter. See Section 3.3 for more on this.

element. The functional form of the hedonic estimation (linear, semi-log, log-linear, etc.) is not made obvious by the underlying hedonic model and must be determined by the data. Cropper et al (1988) found in simulation studies of the accuracy of various functional forms in predicting marginal component prices that the linear Box-Cox and simpler forms, such as linear or semi-log forms, performed best (Cropper et al, 1988). In this paper, a semi-log functional form was chosen. The estimation equation is:

$$p_{it} = \alpha + \beta x_{it} + \gamma y_i + \mu m_{it} + \sigma s_{it} + \tau T_{it} + \varepsilon_{it} \qquad \text{(Eq. 1)}$$

where:

p_{it} = natural log of adjusted price of house *i* at time *t* (nominal price was adjusted by a GDP deflator, base year 2000)

x_{it} = physical characteristics of the house (square footage, bathrooms, age, etc…)

y_i = locational attributes of the house

m_{it} = "measured and no filter" dummy, equal to 1 if house *i* had its well tested for TCE prior to time *t* and did not receive a GAC filter prior to time *t*

s_{it} = SWCA dummy, equal to 1 if location of house *i* is in the SWCA at time *t*, 0 otherwise

T_{it}= time dummy, equal to 1 if sale of house *i* at time *t* occurred in year T

ε = iid disturbance term capturing other factors determining housing price

The primary variables of interest in this baseline specification are s_{it} and m_{it}. The coefficients on these variables will provide tests of the three hypotheses: no information, incomplete information, and complete information. Table 2 below presents the hypotheses and the conditions for rejection.

Table 2: Null Hypotheses

Hypothesis Tested	Variable(s) of Interest and Their Coefficients (in parentheses)	Reject if:
Hypothesis 1: There is no information about groundwater in the market.	SWCA dummy: s_{it} (σ); measured and no filter dummy: m_{it} (μ)	$\sigma \neq 0$ or $\mu \neq 0$
Hypothesis 2: There is incomplete information about groundwater in the market.	s_{it}(σ), m_{it} (μ)	$\sigma \neq 0$ and $\mu = 0$
Hypothesis 3: There is complete information about groundwater in the market.	s_{it}(σ), m_{it} (μ)	$\mu = 0$

These hypothesis tests rely on two assumptions. The first assumption is that GAC filters effectively render water at houses free of TCE. The second assumption is that m_{it} is a fairly accurate proxy for the probability that a house might ever have contaminated groundwater.

Two important estimation issues must be addressed before we can rely on the estimates of the hedonic model: spatial extent of the housing market and stability over time of parameter estimates.

Spatial Extent of the Market

The hedonic model estimates an equilibrium price schedule for house components in a single market. Problems can occur when separate markets are treated as one single market, particularly when the variable of interest is observed only in one of the markets.

According to Palmquist, “if there are a reasonable number of consumers who would consider the alternative areas [as substitutes], then those areas can be treated as a single market, even if many people only consider one or the other [area]” (Palmquist, 2003, 26). Nevertheless, most researchers consider an urban area to be a single market, and urban areas often encompass multiple counties. I treat the entirety of Washington County as a single market in all specifications reported in this draft of the paper. Alternative specifications were investigated, such as using only the central portion of the county, which centers on the plume. The results do not differ substantially; all coefficient estimates are similar in sign and significance levels.

Stability Over Time

More important to this study is whether the values of the various characteristics of houses are relatively stable over the period studied, and, if they are not stable, whether this affects my estimates on the variables of interest. I test information levels regarding groundwater quality present in the real estate market by estimating changes in the coefficients on s_{it} and m_{it} after certain events which might alter the content and amount of information present in the market, such as the expansion of the SWCA and the enactment of the SWCA disclosure law. These estimates will be valid only if the contributions of the other house characteristics to the value of the house are stable over time (Palmquist 2003, 26) or if the contributions of other house characteristics are orthogonal to the coefficients of interest. The time period for which I collected data is from January 1, 1980, to Dec. 31, 2006. Any pooling of data over this entire time period would likely be inappropriate, because the housing market in Washington County changed drastically over the same period. In the 1980's, Washington County was largely agricultural. However, in the late 1980's and early 1990's, large farms were gradually split into suburban-type subdivisions due to the expansion of nearby St. Paul, MN[3].

In this draft, I have pooled data from 1995 through 2006; judging by, it appears that after 1995 the average parcel size in each year had stabilized, indicating that the housing market remains relatively consistent thereafter.

Data

House sales and house characteristic data were taken from the Washington County Tax Assessor's website. Government legislation variables, such as the delineation of the SWCA, were created using various sources including MPCA and MDH documents and their websites. I have included all houses that were sold between Jan. 1, 1995 and Dec. 31, 2006, except townhouses, condominiums, and apartments, because these types are typically sold bundled with unobservable (to the econometrician) home owners' association payments. Table 3 summarizes the house sales and characteristic variables, location variables, and water quality variables. The data are divided into two time periods, 1995 – 2002 and 2003 – 2006, because of the events that occur in 2002 related to changing the HRL, and because the disclosure law went into effect at the beginning of 2003. Summary statistics for each period are shown in Table 4 and Table 5. The Minnesota Pollution Control Agency provided its well sampling data for those houses in and around the SWCA that had their TCE levels measured.

[3] Despite this suburbanization process, there remain large amounts of unused parcels and agricultural lands in the county.

Information regarding groundwater quality can enter the market in multiple ways. If there existed complete information in the market and market participants valued uncontaminated water, then we should expect the actual measured contaminant level to consistently reflect this with a negative coefficient estimate when entered in the hedonic model. Furthermore, assuming that the GAC filter systems perfectly remove all TCE prior to water consumption, then we should expect to see the negative coefficient estimate on the measured contaminant level variable decrease after the year 2002, when most of the filter systems were installed on those houses with more than 5 μg/l TCE[4]. However, a data issue prevents reliable direct estimation of the effect of measured contamination on house prices.

The data issue preventing the direct estimate of the effect of measured contaminant levels is the possibility of measurement error in the measured contaminant level variable. Not all houses in the dataset were actually measured for TCE; in fact, not even all houses inside the SWCA were measured for TCE. The Minnesota Pollution Control Agency tests those houses that are most likely to have TCE contamination; the decision on which houses are most likely to have TCE contamination is presumably based on knowledge of where the plume actually is, which aquifer a residential well uses, and where the plume is most likely to spread. While this cost-minimizing water testing strategy might be effective in terms of preventing residents from consuming water with more than 5 μg/l TCE, it does not provide actual measurements at all houses in the dataset. It is probably the case that all houses with high levels of TCE were tested, but

[4] There were 162 GAC filter systems installed by the end of 2006. 120 of these were installed in the year 2002.

there is still the possibility that some houses with low (less than 1 μg/l) levels of TCE did not get tested. It would be dubious science to make the assumption that any house that did not get tested has zero TCE.

Despite this issue, tests of information levels in the housing market are still possible. A source of information about whether a house has contaminated water or is likely to have contaminated water in the future is whether a house had its water tested by the MPCA. The MPCA tests water at those houses most likely to have TCE contamination, and the dataset contains records of which houses were tested and when. Thus, m_{it}, a dummy equal to unity if a house had its water tested prior to the sale and did not have a GAC filter installed prior to the sale, contains information about the MPCA's opinion of the probability of TCE contamination without any measurement error. The information relayed to the market is that those houses that were tested were deemed most likely to have contaminated water by the MPCA. There are 214 sales of houses between 1995 and 2006 that occurred after the house had its TCE level measured in the dataset. Under the assumption that the GAC filter systems perfectly reduce the probability of TCE contamination to zero, observations of house sales where the water was tested and a filter was installed prior to the sale receive a value of zero for m_{it}. 20 of the 214 sales of measured houses occur after a GAC filter system has been installed in the house. As a result, there are 194 observations of sales of measured and unfiltered houses in the dataset. Houses that have their TCE tested are also likely to be situated near monitoring wells and other possible visible indicators of possible water contamination. It is thus reasonable to think that potential homebuyers could be more worried about possible TCE contamination at those homes that were measured than at those homes that were not. I am thus putting all houses into two categories: those with possible contamination issues, and those without. Houses that had their TCE measured and never got a filter installed are in the first category. Houses that never were measured and houses that were measured and had filters installed are in the second category.

A second source of information regarding possible water contamination is location of a house in the SWCA. This is particularly pertinent after the SWCA disclosure law went into effect at the beginning of 2003. Under the SWCA disclosure law, sellers of homes in the SWCA must disclose that the house is in the SWCA to homebuyers at the time of contract signing. Location in the SWCA does not necessarily mean that a house has contaminated water. Houses can be inside the SWCA and still have zero TCE in their water. Also, some houses inside the SWCA never had their water tested. A dummy variable, s_{it}, indicates whether a house is inside the SWCA at the time of the sale. By examining the effect of location in the SWCA, s_{it}, and the effect of whether a house's water was measured, m_{it}, before and after the disclosure law goes into effect, it is possible to determine whether the market discounted houses that the MPCA tested and did not install filters on, implying the possibility of present or future contamination (m_{it} = 1), regardless of the SWCA, or whether the market discounted houses that were located in the SWCA (s_{it} = 1), regardless of whether the MPCA decided the house needed its water measured.

Outliers and Erroneous Data

Tax assessors' sales data often include probable non-market transactions, suggested by the fact that 561 different observations of sales of three-bedroom houses have recorded prices of \$0.00. There are possibly other non-market transactions, such as sales of \$1.00, \$10.00, or even \$100.00, which likely represent intra-family trade, shifting of nominal ownership for tax purposes, or refinancing. These must be dropped or weighted in a logical and reproducible manner.

There is also the possibility of erroneous sale records at the upper end of the price range. Any attempt to normalize the distribution of price observations will have to address both tails of the price distribution. I addressed this in two steps. First, I dropped those observations that seemed, in my judgment, obviously either erroneous or non-market transactions at the bottom end of the price distribution. Accordingly, all observations of sales at nominal price of less than \$1001 were dropped (1087 observations). Secondly, all houses with an age (calculated as year sold less year built) of less than negative three were dropped on suspicion of erroneous entry (667 obs.). There was also a group of houses that had two sales recorded for the same house on the same day at vastly different prices. These were dropped (634 obs.). Finally, because 1087 observations were dropped somewhat arbitrarily from the bottom end of the price distribution, the 1087 observations with the highest adjusted prices were dropped.

The second step taken to deal with outliers is the implementation of quantile regressions, which "emphasize the middle of the distribution rather than the tails" (Evans, 2007, 18). All regressions reported in this draft are the results of quantile regressions at the median.

Results

The three hypotheses regarding information levels in the market were tested using quantile regressions at the median of adjusted house price. All results reported in this draft are from regressions done in the semi-log functional form[5].

Table 6 shows the results of quantile regressions at the adjusted house price median designed to test whether "measured and not filtered," m_{it}, or SWCA, s_{it}, inform the market about water quality, and whether this changes after the events of 2002 and the implementation of the disclosure law at the beginning of 2003. The events of 2002 are: the human risk limit (HRL) is lowered from 30 µg/l to 5 µg/l; the SWCA is expanded; 120 out of 162 GAC filters are installed; more newspaper articles about the Baytown Site are written than any other year; and multiple town meetings occur due to residents' concerns about health risk and property values. I have divided all sales into two time periods: 1995 – 2002 and 2003 – 2006. Columns 1 and 2 of Table 6 show the coefficient estimates from a quantile regression including township fixed effects. Column 1 is for period 1, 1995 – 2002, and Column 2 is for period 2, 2003 – 2006. All housing and location variables have the expected signs and most are significant in both periods, and for brevity I will focus the following discourse on only the variables of interest.

[5] For robustness, ordinary least squares regressions in the semi-log form were also performed. Results of OLS had the same signs and significance levels, but the magnitudes of some coefficient estimates of the variables of interest were greater than in quantile regressions. Quantile regression results are reported because of possible outlier influence.

The variables *read_nofilt* and *swca* represent m_{it} and s_{it}, the variables of interest. The coefficient estimate of *read_nofilt* in the first period, 1995 – 2002, is approximately -0.135, or -13.5%, while that of *swca* is approximately 0.07, or 7%; both are significant at the 5% level. As these are both binary variables, houses could be in one of four categories: measured, unfiltered, and in the SWCA; measured, unfiltered, and not in the SWCA; not measured (or measured and filtered) and in the SWCA; and not measured (or measured and filtered) and not in the SWCA. The net effects, corresponding to the summed appropriate coefficient estimates from Table 6, for the four groups are summed in Table 7. Wald tests of joint significance of *read_nofilt* and *swca* show that the negative, net effect in Period 1 on Group 1 is significant; in Period 2, it is not.

The positive and significant coefficient on *swca* indicates that there is likely some omitted variable, some characteristic of the neighborhoods in SWCA that makes them valuable relative to those that are not in SWCA, while the negative coefficient on *read_nofilt* indicates that houses with the possibility of contaminated water sold at a discount in this period. One possible explanation for this positive and significant coefficient on *swca* (the "SWCA premium") could arise from the additional well-drilling costs that the SWCA legislation creates. According to local drillers (McCullough, 2007; Sampson, 2007), the cost of drilling a new residential well inside the SWCA could be from $5000 to $20000 more than drilling a similar well outside the SWCA. The "SWCA premium" does not offset the "tainted water" discount for those houses that get both; on net, as shown in Table 7, houses inside the SWCA that were measured and not filtered sold for 6.5% less than houses outside the SWCA that were not measured, at the median. That implies a $10,000 "tainted water" discount at the median in Period 1.

Regardless of whatever omitted variable is causing the "SWCA premium" in the first period, it appears that the housing market reacts to the information captured by the *read_nofilt* variable. There is no SWCA disclosure law in place in this period, so market participants could feasibly have little information about the legislation delineating a certain area as a special well construction area. Market participants react to the information produced by the selection of houses for measurement: those houses that are tested are also those most likely to have TCE contamination and therefore suffer the "tainted water" discount. This is consistent with the hypothesis that there is complete information in the market in Period 1.

Column 2 shows the results of the same regression run for houses sold in the second period, 2003 – 2006. During this entire period, the SWCA disclosure law was in effect. The coefficient estimate on *read_nofilt* in the second period is not statistically different from zero. Compared to the first period, the coefficient estimate on *read_nofilt* increased by about 11%. The coefficient estimate on *swca* is also not statistically different from zero. Compared to the first period, the coefficient estimate on *swca* decreased by about 6.8%.

One possible explanation for the *read_nofilt* and *swca* coefficients converging to zero in the second period is the SWCA disclosure law. In the first period, the market reacted to the MPCA's choice of which houses to measure, but after the disclosure law goes into effect, market participants change their view on which houses might have "tainted water." In the second period, the market discounts all homes that are in the SWCA, thereby reducing the "SWCA premium" witnessed in the first period to zero in the second period. It no longer mattered whether a house has been measured or not; the disclosure law indicated to market participants that location in the SWCA meant a house might have "tainted water." All houses in the SWCA in the second period still possess whatever characteristic created the "SWCA premium" in the first period, but in the second period all houses in the SWCA also suffer from the "tainted water" discount. On net, the "SWCA premium" and "tainted water" discount nullify each other for all houses in the SWCA, making them no different from houses outside the SWCA.

Columns 3 and 4 show the results of quantile regressions with township fixed effects as well as property tax group fixed effects, both for robustness and to attempt to solve the omitted variable issue causing the "SWCA premium" in the first period. Property taxes in Washington County are a function of three location variables: watershed, school district, and township. I thus added in a tax group fixed effect for every unique combination of the three variables, for a total of 82 different tax groups in the dataset. The results from these regressions are quite similar to those in Columns 1 and 2.

These results point to a rejection of the first hypothesis, that there is no information regarding groundwater quality in the housing market. In the first period, houses that were measured for TCE sold at a substantial discount relative to those that were not measured, indicating both that there was information about the TCE contamination in the market and that market participants valued uncontaminated water.

Also, in the first period, houses that were located inside the SWCA sold at a premium compared to those outside the SWCA. In the second period, while the houses that were measured no longer sold at a discount, the premium for location inside the SWCA also disappears. The disappearance of the premium indicates that the source of information relied upon by market participants shifted from MPCA measurements to location in the SWCA. Because sales of houses located in the SWCA had to be accompanied by formal disclosure that the house is in the SWCA beginning in 2003, the cost to a potential buyer of learning whether a house was in the SWCA effectively fell to zero, while the cost of learning whether a house had ever been tested for TCE remained constant. This reduction in the cost of information for one indicator of possibly contaminated water induced market participants to use that indicator in their decisions.

In the first period, the results are consistent with the third hypothesis, that there is complete information in the market. The "tainted water" discount is concentrated at those homes that were deemed the most probable to have contaminated water by the MPCA. The less accurate, in terms of indicating which homes have a possibility of contamination, proxy *swca* does not appear to capture the "tainted water" discount in the first period.

In the second period, however, the reduction in the cost of learning whether a house is in the SWCA resulted in a proxy containing incomplete information being utilized in the market. The "tainted water" discount is no longer captured by the more accurate "measured and not filtered" variable; instead, the *swca* variable drops decreases from a statistically significant +7% in the first period to zero in the second period. These results indicate, perhaps unsurprisingly, that the cost of gathering information is important in determining what information is incorporated into market prices. The lowering of the cost of learning whether a house is in the SWCA results in the market actually using a less accurate proxy for the probability of contamination than in the first period. The market, in second period, incorporated incomplete information regarding groundwater quality because it was less costly to do so.

An alternative interpretation of these results is that there is complete information in the market in both periods. Market participants interpreted the decision by legislators to force disclosure of location in the SWCA as a signal that the water contamination might be wider spread than they thought in the first period. If this is the case, the market is forecasting that the plume will spread farther yet; in fact, there has been some minor spreading of the plume to the east, but overall it has remained relatively stable. Only future mappings of the plume will allow a test of whether the impact of the disclosure law was a result of a change from complete information to incomplete information or a result of the addition of new, correct information to the market. That is, if the plume does expand in the future, then the market was indeed forecasting correctly when those homes assumed here to be "at no risk of contamination" but still located in the SWCA lost some value.

Net Cost of Regulation

Prior to the passage of the disclosure law, the net effect of both the SWCA and house water tests by the MPCA was in fact positive. This occurs because of the SWCA premium – houses in the SWCA that did not have their water tested sold for $12,483.66 more than the average house during period 1 (1995 – 2002).

The affected houses fall into three different categories: in the SWCA and tested; outside the SWCA and tested; and in the SWCA and not tested. Of the three categories, the largest group is the “in the SWCA and not tested” group (see Table 7, which shows the number of observations in each group in each time period). To determine the net effect of regulation, I calculated the realized net effect of the SWCA and water testing in each period for all the groups. The realized net effect is the actual number of house sales in each group in the time period multiplied by the premium or discount for each group in each time period, evaluated at the mean price for the period. A calculation of realized costs or benefits from the net effect of the SWCA and water testing before and after the disclosure law passed at the beginning of 2003 yields the following numbers: prior to the disclosure law’s passage, the net effect was positive at $2,263,822.79. This does not imply that groundwater contamination imparted value to the neighborhood. Instead, it shows that some missing variable gives houses in the SWCA the SWCA premium, and that effect dominates the tainted water discount. After the new regulation passes, the net effect was negative at -$779,717.62. This is likely an underestimation: time period 1 includes eight years of sale observations (1995 – 2002) while time period 2 includes only four years (2003 – 2006). An equal number of years in period 2 as period 1 would decrease the net effect even more, assuming the discounts in each group remain the same.

Conclusions

In this paper, I have tested information levels regarding groundwater quality and their effects on house prices. Using tax assessor real estate data, GIS data, and data on which houses were chosen for testing for contaminants by the Minnesota Pollution Control Agency and when they were tested, the results indicate that, prior to the passage of a disclosure law mandating that homesellers inform homebuyers about a house's location in a special well construction area (SWCA), the real estate market incorporates complete information about where trichloroethylene (TCE) contamination is likely to occur. Only those houses that the MPCA tested because they are the most likely to have contaminated water suffered a "tainted water" discount during this time period. After the disclosure law passes, the market incorporates incomplete information about where TCE is likely to occur. This might occur because the disclosure law offers a very low-cost way of gathering information about groundwater quality, and, as a result, all houses in the legislatively-created special well construction area (SWCA) suffer a "tainted water" discount despite many of those house having uncontaminated water and little to no possibility of future contamination.

An alternative interpretation of the shift of the "tainted water" discount from those houses that were tested by the MPCA to all houses in the SWCA is that the disclosure law informed the market that the plume might spread to other houses in the SWCA. Thus, if the plume spreads to those houses in the SWCA that presently have no contamination and are viewed by the MPCA as having no risk of contamination, then the market would have predicted this. To date, however, the plume has spread little, while the disclosure law was passed in 2003.

These results emphasize the importance of information in markets. Many researchers have studied market reactions to potentially harmful environmental disamenities; often, these market reactions are cited as evidence of non-economic behavior on the part of market participants. Few studies, however, have questioned the assumption that market participants possess complete information about the environmental disamenity (a notable exception is Pope, 2007). Information acquisition can be costly. When markets react in ways that researchers find odd or "irrational," it could simply be the case that markets react to incomplete information because gathering complete information is too costly.

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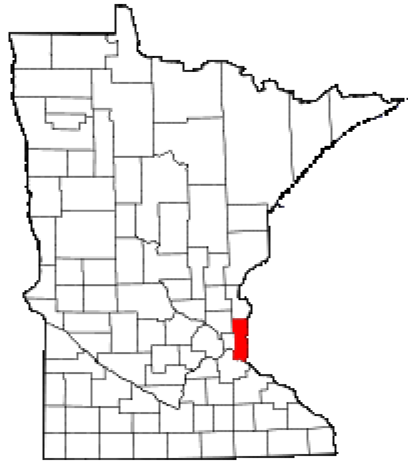
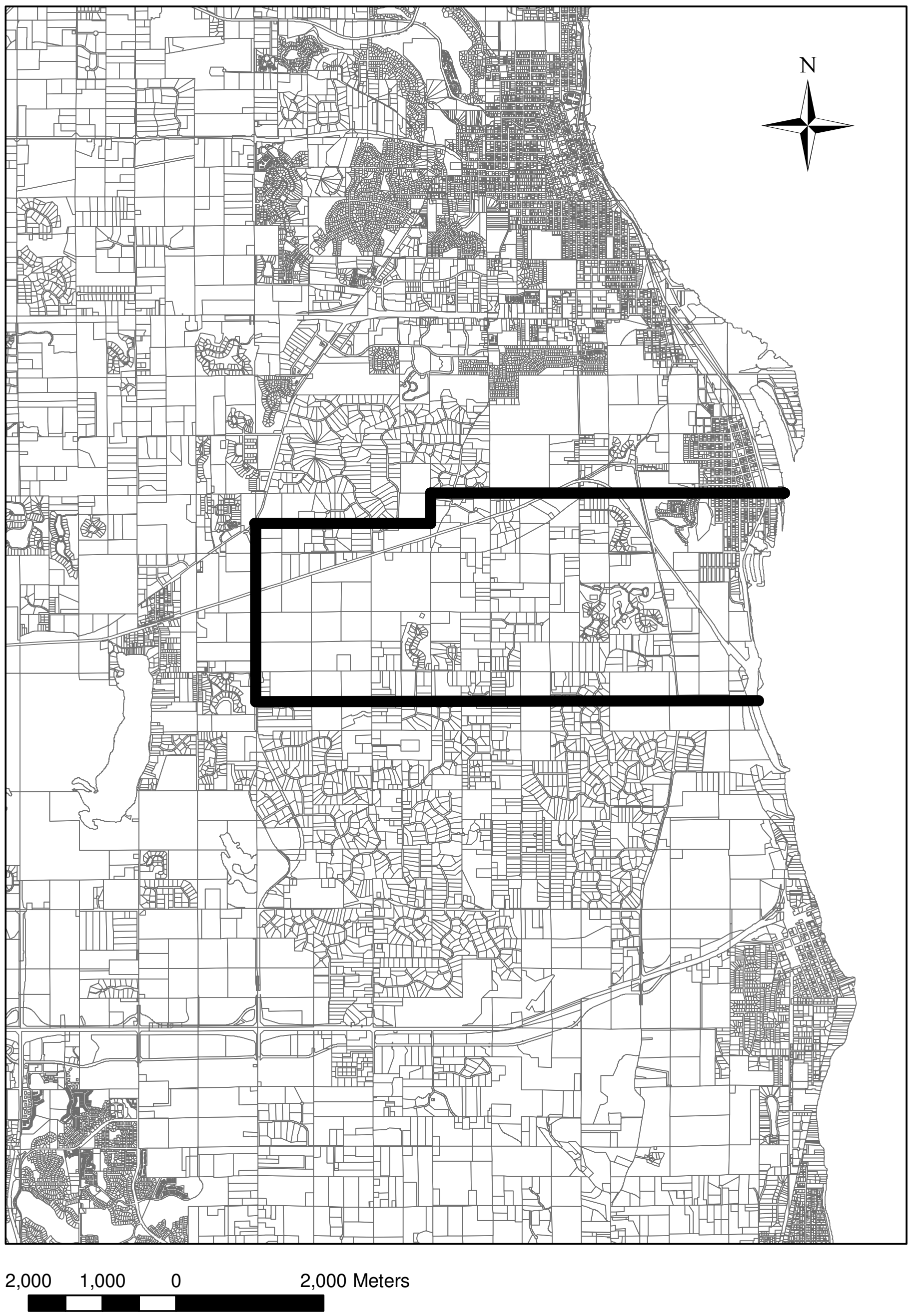
Figure 2.1: Location of Washington County in Minnesota



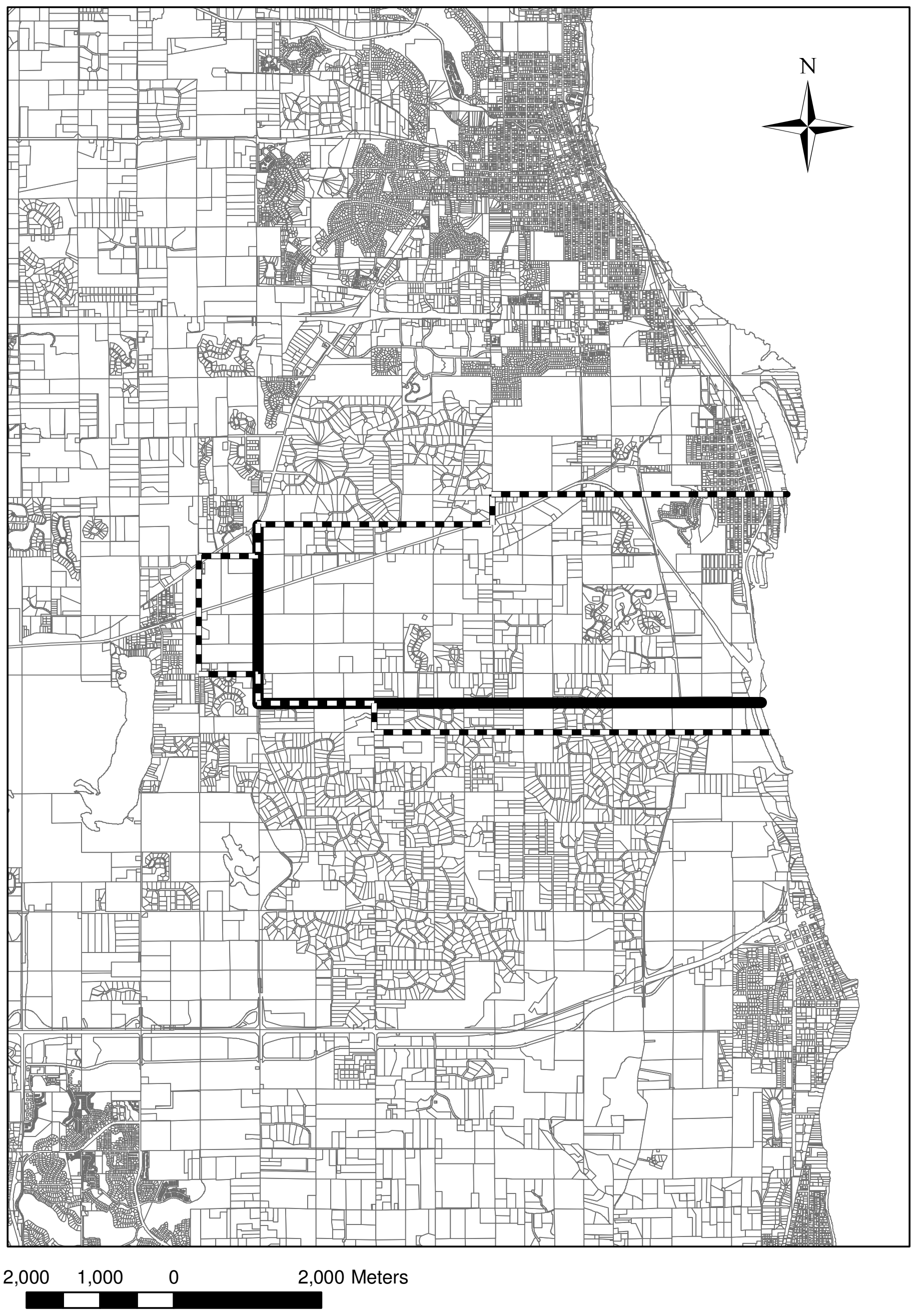
Notes: Washington County abuts the St. Croix river on the east, and St. Paul on the west. It is shaded in the figure above.

Figure 2.2: Original SWCA



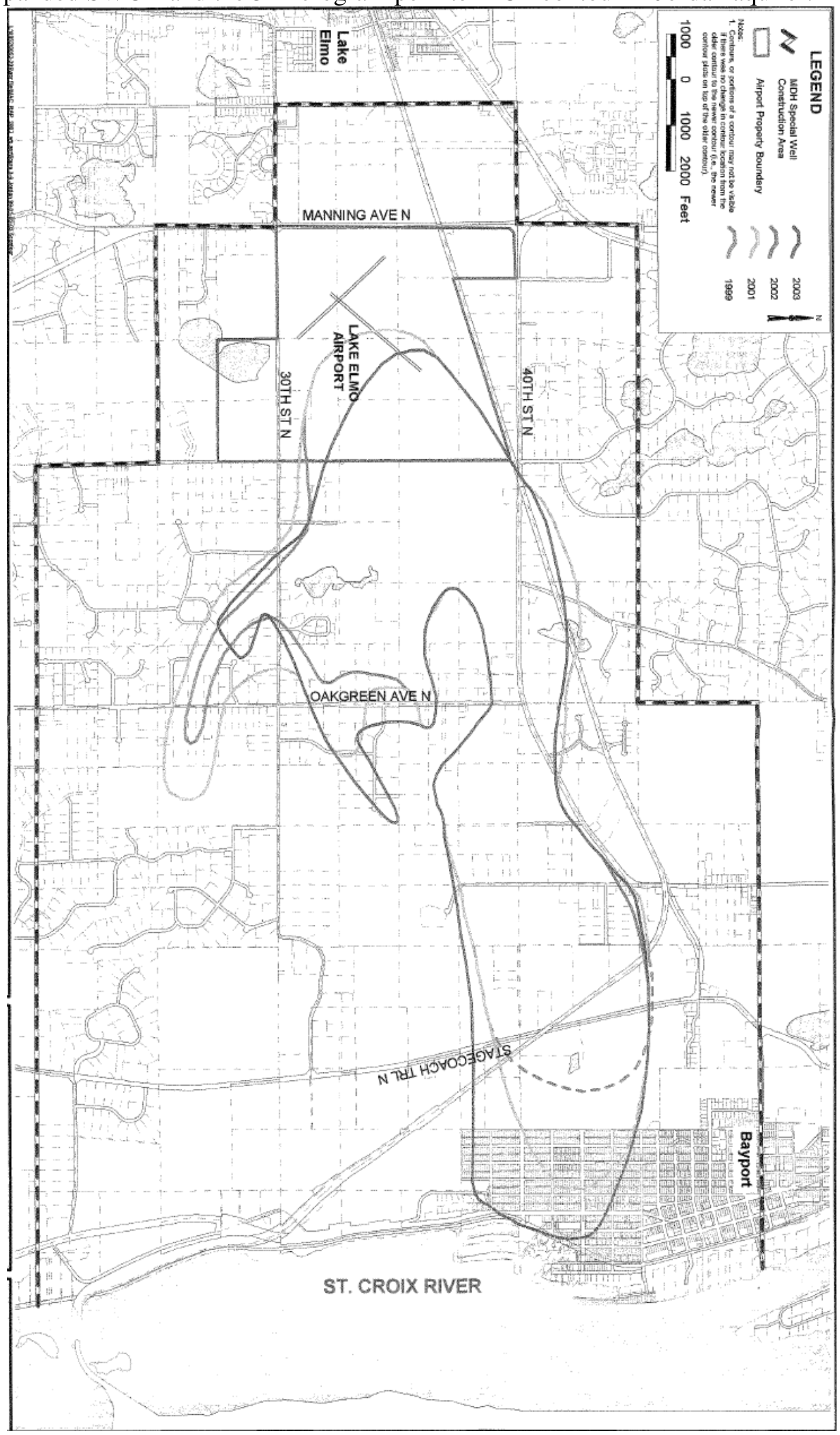
Notes: The solid black line shows the border of the SWCA prior to its expansion in 2002.

Figure 2.3: Expanded SWCA



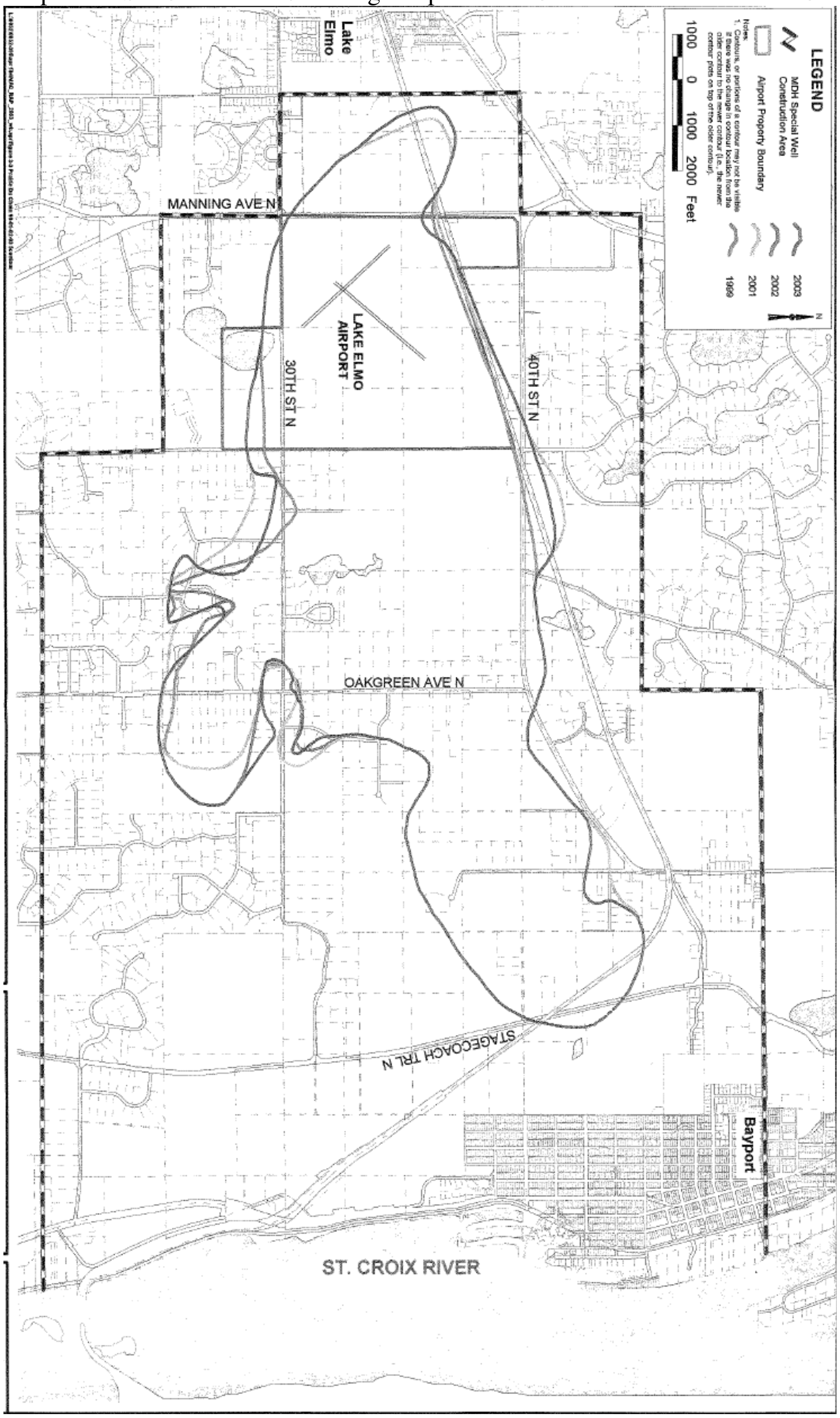
Notes: The dotted line shows the border of the SWCA after its expansion in 2002. The solid black line shows the old border of the SWCA on the west and southern ends, where the expansion took place. The expanded SWCA is 12.5 square miles.

Figure 2.4: Expanded SWCA and the 5 microgram per liter TCE contour in Jordan aquifer.



Notes: This shows the 5 µg/l TCE contour in the Jordan aquifer in years 1999, 2001, 2002, and 2003. The edges of the contour change somewhat across years, particularly in the east near the St. Croix River and Bayport. The dotted line around the perimeter is the expanded SWCA. Source: Wenck Associates, Inc.

Figure 2.5: Expanded SWCA and the 5 microgram per liter TCE contour in Prairie Du Chien aquifer.



Notes: This shows the 5 µg/l TCE contour in the Prairie Du Chien contour in years 1999, 2001, 2002, and 2003. The edges of the contour are relatively stable across time. The dotted line around the perimeter is the expanded SWCA. Source: Wenck Associates, Inc.

Table 2.1: Chronology of Site Events.

Date	Event
6/1987	MDH sampling of private water wells surrounding the Baytown Dump detects several volatile organic compounds (VOCs), including TCE and CCl4.
5/1988	MDH establishes the SWCA.
1988	Site listed on the Minnesota Permanent List of Priorities (PLP).
12/16/1994	Site listed on the National Priorities List (NPL).
6/1995	MPCA assumed full responsibility for oversight of Site under the Enforcement Deferral Pilot Project.
1999	Consent Order
4/1999	Feasibility Study (FS) completed for the site.
5/1999	Proposed Plan published.
5/2000	EPA and MPCA executed a Record of Decision (ROD).
1999 to present	Site wide water sampling and GAC installation initiated.
1988 to 2001	Metropolitan Airports Commission (MAC) conducted investigations at and near Lake Elmo Airport.
2002	MDH changes human risk limit for TCE from 30 micrograms/liter to 5 micrograms/liter.
2002	MPCA expands SWCA in accordance with new human risk limit.
2002 to 2004	MPCA conducts investigations designed to identify TCE source area.
2004	MPCA investigations succeed in locating primary TCE source area.
2004 to present	MPCA conducts investigation designed to further delineate the nature and extent of the TCE source area and to characterize the site.
2/2005	Feasibility study completed for TCE plume containment near source.
2005 to present	MPCA pursuing design, approval, and implementation of remedial actions addressing the groundwater contamination plume and source area.

Source: MPCA *Five-Year Review Report*: *First Five-Year Review Report*, 2007, p.2.

Table 2.3: Definitions of Variables

Variable	**Definition**
realprice	GDP deflator-adjusted sale price of the house, in dollars
lnprice	natural log of realprice
date	date of sale
age	age of house at time of sale = year sold less year built; negative values of age were changed to zero.
agesq	newage squared
T.L.A.	total living area, in square feet
Lot Area	size of parcel, in square feet
beds	number of bedrooms
full	number of full bathrooms
half	number of half bathrooms
threequart	number of ¾ bathrooms
deluxe	number of deluxe bathrooms
fireplace	number of fireplaces
add1	area of 1^{st} addition in square feet, if it existed at time of sale
addn	area of n^{th} addition in square feet, if it existed at time of sale
add_n	dummy indicating presence of n^{th} addition on the house at time of sale
gar1	area of 1^{st} garage in square feet, if it existed at time of sale
garn	area of n^{th} garage in square feet, if it existed at time of sale
ac	dummy indicating presence of central air-conditioning system
porcharea	area of porch(es) in square feet
deckarea	area of deck or patio in square feet
river_dist	convex index between 0 and 1 indicating proximity of a house to a river. river_dist = $\max[1-(d/d_{max})^{1/2},0]$ where d is the distance to the nearest river in meters and d_{max} is set to 500 meters. If d>500, river_dist=0.
lake_dist	convex index between 0 and 1 indicating proximity of a house to a river. lake_dist = $\max[1-(d/d_{max})^{1/2},0]$ where d is the distance to the nearest lake in meters and d_{max} is set to 500 meters. If d>500, lake_dist=0.
read_nofilt	dummy variable equal to one if a house had its TCE level measured by the MPCA prior to sale and had not received a GAC filter system, zero otherwise.
swca	dummy variable indicating location inside the SWCA at time of sale

Table 2.4: Summary Statistics, Period 1, Years 1995 - 2002

Variable	Obs.	Mean	Median	Std. Dev.	Min	Max
realprice	18608	178338	156251.2	90850.73	2096.143	647870.1
lnreal	18608	11.96498	11.95922	0.524272	7.647854	13.38145
full	18608	1.256286	1	0.57005	0	6
threequart	18608	0.481407	0	0.558144	0	4
half	18608	0.336993	0	0.481599	0	3
deluxe	18608	0.205092	0	0.40543	0	2
beds	18608	3.123087	3	0.88708	1	12
tla	18608	1570.115	1362	642.9056	112	8800
lotarea	18608	40698.53	11744	112030.2	0	2792196
age	18608	18.39724	8	27.02399	0	154
agesq	18608	1068.725	64	2864.931	0	23716
fireplace	18608	0.694763	1	0.687561	0	5
ac	18608	0.872073	1	0.334015	0	1
porcharea	18608	55.24657	0	97.38714	0	2011
deckarea	18608	103.0443	0	133.2517	0	1000
district1	18608	0.006335	0	0.079344	0	1
district2	18608	0.209657	0	0.407071	0	1
district3	18608	0.054657	0	0.227314	0	1
district5	18608	0.111046	0	0.314195	0	1
district6	18608	0.069531	0	0.254361	0	1
district7	18608	0.289852	0	0.453702	0	1
district8	18608	0.253374	0	0.434952	0	1
district9	18608	0.0024	0	0.048936	0	1
watershed2	18608	0.015223	0	0.122443	0	1
watershed3	18608	0.072352	0	0.259076	0	1
watershed4	18608	0.078982	0	0.269716	0	1
watershed5	18608	0.145891	0	0.353004	0	1
watershed6	18608	0.32313	0	0.467681	0	1
river_dist	18608	0.026398	0	0.131944	0	0.980238
lake_dist	18608	0.02149	0	0.10969	0	0.949659
add1	18608	128.2661	35	204.3049	0	2368
add2	18608	24.2806	0	78.89728	0	1484
add3	18608	4.07382	0	33.31759	0	960
add4	18608	0.021446	0	1.274798	0	128
gar1	18608	507.612	505	247.7878	0	2800
gar2	18608	17.30528	0	118.2749	0	2480
gar3	18608	0.104199	0	6.195348	0	624
swca	18608	0.015347	0	0.122929	0	1
read_nofilt	18608	0.004289	0	0.065352	0	1

Table 2.5: Summary Statistics, Period 2, Years 2003 – 2006

Variable	Obs.	Mean	Median	Std. Dev.	Min	Max
realprice	11861	273248.2	245513.5	115992.2	1882.931	744213.6
lnreal	11861	12.43498	12.41111	0.408489	7.540585	13.52008
full	11861	1.173101	1	0.499099	0	6
threequart	11861	0.483571	0	0.578756	0	3
half	11861	0.447964	0	0.511421	0	3
deluxe	11861	0.347451	0	0.478009	0	2
beds	11861	3.221077	3	0.903566	1	12
tla	11861	1691.737	1518	682.6022	140	7560
lotarea	11861	26485.17	10152	83698.71	0	2586379
age	11861	19.95127	11	26.20608	0	158
agesq	11861	1084.769	121	2867.64	0	24964
fireplace	11861	0.755497	1	0.664921	0	5
ac	11861	0.899175	1	0.301106	0	1
porcharea	11861	58.87419	0	89.45057	0	1240
deckarea	11861	89.33646	0	128.4225	0	772
district1	11861	0.006747	0	0.081863	0	1
district2	11861	0.158358	0	0.365088	0	1
district3	11861	0.049475	0	0.216865	0	1
district5	11861	0.115505	0	0.31964	0	1
district6	11861	0.044041	0	0.205192	0	1
district7	11861	0.412606	0	0.492319	0	1
district8	11861	0.210145	0	0.407424	0	1
district9	11861	0.001874	0	0.043251	0	1
watershed2	11861	0.011712	0	0.107589	0	1
watershed3	11861	0.057714	0	0.233209	0	1
watershed4	11861	0.115687	0	0.31986	0	1
watershed5	11861	0.143601	0	0.350696	0	1
watershed6	11861	0.420262	0	0.493617	0	1
river_dist	11861	0.019978	0	0.115476	0	0.98288
lake_dist	11861	0.015331	0	0.092069	0	0.961194
add1	11861	128.1496	40	195.1575	0	2112
add2	11861	33.82003	0	94.60141	0	1484
add3	11861	6.868066	0	39.02542	0	810
add4	11861	0.006184	0	0.372778	0	38
gar1	11861	540.6867	528	226.4092	0	2800
gar2	11861	12.99713	0	99.69175	0	2925
gar3	11861	0.287669	0	14.4616	0	1040
swca	11861	0.013618	0	0.115903	0	1
read_nofilt	11861	0.002499	0	0.049927	0	1

Table 2.6: Quantile Regressions at Median

	Township Fixed Effects				Tax Group & Township Fixed Effects			
Dep. Var.: lnreal	(1) Years: 1995 - 2002		(2) Years: 2003 - 2006		(3) Years: 1995 - 2002		(4) Years: 2003 - 2006	
full	0.045801	(14.35)**	0.049414	(11.46)**	0.045725	(15.00)**	0.048639	(11.16)**
threequart	0.036883	(12.30)**	0.043968	(11.81)**	0.036158	(12.60)**	0.043213	(11.46)**
half	-0.009487	(2.73)**	-0.011902	(2.73)**	-0.009837	(2.96)**	-0.012117	(2.74)**
deluxe	0.083112	(16.45)**	0.119267	(19.46)**	0.078936	(16.19)**	0.117478	(18.92)**
beds	0.022251	(12.38)**	0.027280	(11.78)**	0.023031	(13.40)**	0.027345	(11.64)**
T.L.A.	0.000174	(50.27)**	0.000184	(41.36)**	0.000173	(52.12)**	0.000183	(40.56)**
Lot Area	3.65e-07	(25.24)**	7.18e-07	(27.38)**	3.63e-07	(25.91)**	7.03e-07	(26.12)**
age	-0.003772	(23.50)**	-0.004100	(18.31)**	-0.003851	(25.04)**	-0.004212	(18.56)**
agesq	0.000015	(10.66)**	0.000019	(10.09)**	0.000016	(11.56)**	0.000020	(10.44)**
fireplace	0.047970	(20.40)**	0.052023	(16.96)**	0.046284	(20.61)**	0.051622	(16.65)**
ac	0.030746	(6.84)**	0.024225	(3.73)**	0.032823	(7.65)**	0.023071	(3.52)**
porcharea	0.000266	(16.02)**	0.000380	(16.68)**	0.000264	(16.69)**	0.000368	(15.94)**
deckarea	0.000165	(14.76)**	0.000159	(10.65)**	0.000159	(14.87)**	0.000153	(10.15)**
river_dist	0.175309	(11.66)**	0.179480	(8.17)**	0.173309	(11.97)**	0.178966	(7.98)**
lake_dist	0.557180	(20.82)**	0.722736	(18.32)**	0.510531	(15.39)**	0.748829	(14.63)**
add1	0.000046	(6.63)**	-0.000019	(2.07)*	0.000044	(6.68)**	-0.000018	(2.02)*
add2	0.000046	(2.69)**	0.000007	(0.36)	0.000040	(2.46)*	0.000014	(0.74)
add3	0.000102	(2.48)*	0.000177	(4.11)**	0.000073	(1.88)	0.000203	(4.68)**
add4	0.000987	(1.43)	0.006091	(2.80)**	0.001066	(1.62)	0.006090	(2.77)**
gar1	0.000372	(63.12)**	0.000302	(34.61)**	0.000372	(65.89)**	0.000303	(34.30)**
gar2	0.000106	(9.07)**	0.000107	(6.30)**	0.000115	(10.43)**	0.000108	(6.36)**
gar3	-0.000331	(1.56)	0.000130	(1.14)	-0.000161	(0.79)	-0.000003	(0.03)
read_nofilt	-0.134391	(5.08)**	-0.027570	(0.72)	-0.142021	(5.43)**	-0.001597	(0.04)
swca	0.070558	(3.10)**	-0.008068	(0.24)	0.090822	(4.17)**	-0.022143	(0.62)
Constant	11.470592	(283.42)**	11.983834	(216.60)**	10.622572	(176.74)**	11.566936	(150.19)**
Observations	18608		11861		18608		11861	
Pseudo-R^2	0.4326		0.5640		0.4359		0.5665	

Notes: Absolute value of t statistics in parentheses. * significant at 5%; ** significant at 1%. Time dummy variables, watershed variables, and school district variables are all included in all above regressions; their coefficient estimates are suppressed for brevity and are available upon request.

Table 2.7: Net effects of *read_nofilt* and *swca*

	Group 1: measured, unfiltered, in the SWCA,	Group 2: measured, unfiltered, outside the SWCA	Group 3: not measured (or measured and filtered), in the SWCA	Group 4: not measured (or measured and filtered), not in the SWCA
Period 1	-13.4% + 7% = -6.4%	-13.4%	+7%	baseline (0)
Observations	94	15	296	18308
Period 2	-2.8% -0.8% = -3.6%	-2.8%	-0.8%	baseline (0)
Observations	38	2	180	11641

CHAPTER THREE

DO ECONOMIC INTEGRATION AGREEMENTS ACTUALLY WORK? ISSUES IN UNDERSTANDING THE CAUSES AND CONSEQUENCES OF THE GROWTH OF REGIONALISM

Introduction

One of the most notable events of the world economy over the past twenty years has been the phenomenal growth in the number of international economic integration agreements (EIAs). EIAs are treaties between economic units – in the case of international EIAs, between nations – to reduce policy-controlled barriers to the flow of goods, services, capital, labor, etc. Most – though not all – EIAs tend to be “regional” (or continental) in scope and most tend to be free (or preferential) *trade* agreements (henceforth, FTAs). According to the World Trade Organization (WTO) website, in 2006 there are approximately 300 regional trade agreements that are either planned, have concluded negotiations, or are in force. Interestingly, of the 250 agreements notified to the General Agreement on Tariffs and Trade (GATT) and WTO between 1947-2002, about half were notified *since 1995*. Thus, there has been a virtual explosion in the number of EIAs in the past decade. This is the “latest wave” of regional trade and cooperation agreements that comes on the heels of the 50th anniversary of the most noted economic integration agreement of modern times, the1957 Treaty of Rome.

This wave has culminated in – what Jagdish Bhagwati and Arvind Panagariya (1999) have famously termed – a seeming “spaghetti bowl” of EIAs. Figure 1 from Estevadeordal (2006) illustrates vividly this “spaghetti bowl,” with each line representing an EIA between one country and another (or with a group of countries). However, one aim of this paper is to convince the reader that – instead of looking at this web of agreements as a spaghetti bowl – economists and policy makers should see this as a "market for regionalism."

This paper synthesizes and develops further a line of research pursued by the authors on the causes and consequences of the growth of regionalism. In this paper, we hope to accomplish four goals. First, we address conceptually why it is useful to consider this web of agreements as a type of "market." In a world with approximately 200 countries and national governments, there exist approximately 20,000 potential bilateral EIAs (200x199/2 = 19,900). To the extent that national governments promote the welfare of their nations' firms and consumers, the rules-of-engagement in bilateral trade are likely determined in a highly competitive political environment. We discuss the notion of "competitive liberalization," coined by Fred Bergsten more than a decade ago, and suggest a systematic conceptual framework for analyzing determinants of EIAs, initially in a static context. While bilateral trade agreements are ultimately negotiated by national governments, the rules are negotiated in the context of a type of "market" of 20,000 potential bilateral agreements, which can provide potentially quite a competitive setting for the beneficiaries of such agreements – various nations' firms and consumers – to influence their national policy makers to negotiate in a competitive manner. To a large extent, one might interpret our approach in the context of the "new institutionalism." We discuss empirical evidence consistent with the notion that EIAs are determined in a competitive economic environment.

Our second goal is to argue that the market for (bilateral) EIAs exists contemporaneously with the market for (bilateral) trade flows, obscuring *ex post* evaluation of the effects of EIAs on trade. Casual examination of trade flows and of EIAs suggests that the two markets are contemporaneous. Country pairs that are physically close and are large economically tend to have very large trade flows, e.g., U.S.- Canada and France-Germany. Moreover, countries that choose to form EIAs are physically close and are large economically, e.g., U.S.-Canada and France-Germany. However, if trade flows and EIAs are determined simultaneously, this raises problems for evaluating *ex post* the effects of EIAs on trade.

Our third goal is then to address issues concerned with providing better estimates of the *ex post* effects of EIAs on trade. While computable general equilibrium (CGE) models have long dominated policy makers analyses of the potential economic benefits from changing trade policies (including formation of EIAs), such models can only provide *ex ante* forecasts of the effects of eliminating (or reducing) measurable government-imposed trade barriers on trade, production, consumption and welfare of a nation. These models cannot address what actually *did* happen as a result of forming a specific EIA. Moreover, many have argued that CGE models have tended to underestimate the effects of EIAs on trade, cf., DeRosa and Gilbert (2005). Policy makers should be interested – and, we conjecture, *are* interested – in *ex post* quantitative estimates of the effects of an EIA on trade flows (and, subsequently, on production, incomes, etc.). As John Whalley puts it in his article in this same collection of symposium papers: “A recent World Bank (*Global Economic Prospects*, 2005) estimate is that perhaps around 43 percent of world trade was covered by agreements in force in 2003 and was projected to increase to 55 percent by 2005 (OECD, 2003). But such calculations only raise more questions: *How large are the impacts of these agreements on covered trade*?" (italics added).

Surprisingly, estimates to date using the workhorse for *ex post* empirical analysis of the effect of EIAs on trade flows (the "gravity equation") often find economically and statistically *insignificant* effects of EIAs on trade, cf., Frankel (1997). Moreover, recent empirical evidence shows that such estimates are quite *fragile*, cf., Ghosh and Yamarik (2004). We address estimation techniques that suggest that previous estimates are likely biased downward. Moreover, we provide empirical evidence of much more "sturdy" (*ex post*) estimates of the trade effects of EIAs.

Our fourth goal is then to address how the previous three issues help us to better understand the "latest wave" of regional trade agreements. We argue that policy makers have tended to expect larger trade effects from EIAs than *ex ante* CGE models have suggested. Because policy makers have selected endogenously into EIAs due the larger expected effects, previous *ex post* estimates of the trade effects of EIAs have been biased downward. Using our "sturdier" estimates of EIA "treatment effects," we then confirm this conjecture, demonstrating much stronger EIA effects on trade than witnessed previously.

<u>Determinants of Bilateral Trade Flows and Bilateral Economic Integration Agreements</u>

International economists such as Richard Baldwin (1995) and C. Fred Bergsten (1996) noted more than a decade ago that there were seemingly strong competitive pressures in the world economy – sensed by nations' governments – that induced such governments to liberalize trade both bilaterally and regionally. The large numbers of nations party to the GATT/WTO has grown over the past 50 years to approximately 150 countries. This large number of parties has likely made the ability of negotiators to liberalize trade in agriculture, goods, services, capital, and labor under one agreement much more difficult[6]. Nevertheless, governments are pressured by individual voters and firms' lobbies to provide a framework of policies (or "institutions") well-suited to both constituencies' interests (maximizing economic welfare and economic profits, respectively). In the face of these pressures and an impasse in multilateral trade and investment liberalization at the WTO level, governments have sought alternative policy changes to improve economic welfare and firms' profits. One alternative – potentially a "building block" for further multilateral liberalization – is economic integration agreements (which include bilateral agreements). As shown in Figure 1, the proliferation of EIAs over the past fifty years has created the so called "spaghetti bowl" of EIAs.

However, Baldwin's "domino theory" of regionalism and Bergsten's "competitive liberalization hypothesis" are implicitly dynamic stories. In our view, before one can conceptualize about the "latest wave" of regionalism (which is also implicitly dynamic), we consider it imperative to address first "Regionalism." That is, we start with a *static* long-run view of the determinants of regionalism (and bilateralism). The notion of "competitive liberalization" can be consistent with a static concept of regionalism as well

[6] See Mansfield and Reinhardt (2003) and Moravcsik (2005). Also, Moravcsik argues that competitive liberalization pressures have been the dominant force behind much of European economic integration, with the likely exception of Germany's motivation in the 1950s.

as a dynamic one. As is traditional in economics, one should probably examine the *long-run* economic factors influencing the equilibrium outcome *before* modeling explicitly the short- and medium-run factors influencing EIA formation, where the latter are often more easily observed and often discussed less technically. We intentionally have used the term "Economic Integration Agreements" initially to be inclusive. The term "Economic Integration" spans integration of goods, services, capital, and labor markets; in even broader views, it encompasses integration in economic activity that goes beyond economists' traditional categorizations of "goods" and "factors." We also used "Economic Integration" – not "Regional Economic Integration" – to be inclusive in geographic scope of coverage. Many recent economic integration agreements – the recently-signed Australian-U.S. FTA, for example – involve countries on different continents; economists have occasionally referred to these as "unnatural" EIAs, in the sense that they are not in the same geographic region or on the same continent.[7] However, the vast bulk of EIAs are regional free trade agreements, limited in scope to countries sharing common continents and to goods (and, in many cases, services) sectors. In the remainder of this paper, we will continue to use the acronym EIA to be inclusive of FTAs, customs unions, common markets, and economic unions, although most of the focus will be on the trade implications of EIAs. One reason for this is that, in the empirical analysis later, our EIAs will include some deeper integration agreements, such as the European Union.

[7] See, for example, Krugman (1991a,b), Frankel, Stein and Wei (1995, 1996), and Frankel (1997).

Determinants of Bilateral Trade

Before addressing directly static determinants of EIAs, it will be useful first to discuss the underlying economic context of world trade *in the absence* of policy-oriented barriers to trade. After we establish the fundamental determinants of trade and economic welfare in the presence of only "natural" barriers to trade (e.g., distance between economic agents), we then introduce (exogenously) policy oriented – or "artificial" – trade barriers. This will provide the background to then discuss *endogenous* regionalism behavior by governments.[8]

Because regionalism typically entails bilateralism,[9] we address briefly determinants of bilateral trade flows in an N-country world (N>2) in the absence (presence) of policy-based (natural) trade barriers. The modern theory of international trade – largely developed in the context of two countries with production of goods in two industries using two factors of production – usually emphasizes that the economic rationales for international trade are traditional comparative advantage (or inter-industry trade, driven by Heckscher-Ohlin relative factor endowment differences or Ricardian relative productivity differences) and by "acquired" comparative advantage (or intra-industry trade, due to increasing returns to scale in production of slightly differentiated products), but historically ignoring transport costs and economic geography.

[8] Our analysis initially will take as given exogenously the prevailing level of policy-oriented trade barriers, such as tariff rates. In reality, the ideal approach would be to consider the endogenously-determined Nash equilibrium tariff rates pre- and post-integration, as the pre-integration Nash equilibrium tariffs are likely to differ from the post-integration ones. Addressing this limitation, however, is beyond the scope of this paper.

[9] In the remainder of the paper, we often use the terms "bilateralism" and "regionalism" interchangeably.

However, motivated by the robust empirical regularity that bilateral trade flows between pairs of countries are explained well by the product of their gross domestic products (GDPs) and their bilateral distance, trade economists have formulated multi-country (or N-country) theoretical foundations for a “gravity equation” of bilateral international trade over the past 25 years, and in a manner consistent with established theories of intra- and inter-industry international trade. For instance, the first formal theoretical foundation for the gravity equation with a one-sector endowment economy, but many countries, was Anderson (1979). Anderson showed that a simple (conditional) general equilibrium Armington model with products differentiated by country of origin and constant-elasticity-of-substitution preferences yields a basic gravity equation:[10]

$$PX_{ij} = \beta_0 (GDP_i)^{\beta 1} (GDP_j)^{\beta 2} (DIST_{ij})^{\beta 3} \varepsilon_{ij} \tag{1}$$

where PX_{ij} is the value of the merchandise trade flow from exporter i to importer j, GDP_i (GDP_j) is the level of nominal gross domestic product in country i (j), $DIST_{ij}$ is the distance between the economic centers of countries i and j, and ε_{ij} is assumed to be a log-normally distributed error term. The theory suggested that $\beta 1 = \beta 2 = 1$and $\beta 3 < 0$.

Other papers extended these theoretical foundations in various important directions. Helpman and Krugman (1985) introduced monopolistic competition and increasing returns to scale, motivating a gravity equation with trade flows to explain intra-industry trade between countries with similar relative factor endowments and labor

[10] As noted in Anderson and van Wincoop (2004), Anderson (1979) and Anderson and van Wincoop (2003) are “conditional” general equilibrium models, employing a “trade separability” assumption where the allocation of bilateral flows across N countries is separable from production and consumption allocations within countries.

productivities. Bergstrand (1985) raised the issue of including multilateral price terms for importers and exporters as important for determining bilateral trade flows; for instance, the trade flow from i to j is influenced by the prices, transport costs, and other trade costs that the consumer in j faces from its N–2 other trade partners as well as domestic firms. Bergstrand (1989, 1990) showed formally that a gravity equation evolved from a traditional Heckscher-Ohlin model with two industries, two factors and N countries with both inter- and intra-industry trade. Evenett and Keller (2002) provided empirical evidence that a model with both Heckscher-Ohlin inter-industry trade and Helpman-Krugman intra-industry trade with imperfect specialization fit the data best. Most recently, Anderson and van Wincoop (2003) have shown formally that proper estimation of the gravity equation (to avoid omitted variables bias) must recognize *endogenous* multilateral price terms for both the exporter and importer countries, and likely requires estimation of a system of nonlinear equations using custom nonlinear least squares programming to account properly for endogeneity of prices:

$$PX_{ij}= \beta_0(GDP_i)^1(GDP_j)^1(t_{ij})^{1-\sigma}\, P_i^{\sigma-1}\, P_j^{\sigma-1}\varepsilon_{ij} \tag{2}$$

where $\sigma > 1$, t_{ij} denotes bilateral trade costs (which potentially can be explained by various observable variables) and P_i and P_j are “endogenous” multilateral price terms that account for trade costs that agents in countries i and j face from all N countries (including at home), where

$$P_i = \left[\sum_{j=1}^{N}\theta_j (t_{ij} / P_j)^{1-\sigma}\right]^{1/(1-\sigma)} \tag{3}$$

$$P_j = \left[\sum_{i=1}^{N} \theta_i (t_{ij} / P_i)^{1-\sigma} \right]^{1/(1-\sigma)} \quad (4)$$

under an assumption that bilateral trade barriers t_{ij} and t_{ji} are symmetric for all pairs. Letting GDP^T denote total income of all regions, which is constant across region pairs, then θ_i (θ_j) denotes GDP_i / GDP^T (GDP_j / GDP^T). Details of estimating (2) for aggregate trade flows using either nonlinear least squares or fixed effects for P_i and P_j are addressed in Anderson and van Wincoop (2003), Feenstra (2004), and Baier and Bergstrand (2002, 2006, 2007).[11] Baier and Bergstrand (2002) extend the Anderson-van Wincoop one-sector, N-country endowment economy to a world with two sectors, two factors, and N countries with Heckscher-Ohlin-Samuelson inter-industry trade and Chamberlin-Helpman-Krugman intra-industry trade, cf., Carrere (2006).

Baier and Bergstrand (2006) show a method for estimating coefficient estimates in equations (2)-(4) using ordinary least squares (OLS) that are virtually identical to those estimated using Anderson and van Wincoop's nonlinear least squares program or fixed effects, based upon a first-order Taylor series expansion of the theory:

$$\frac{PX_{ij}}{GDP_i GDP_j / GDP^T} = \left(\frac{t_{ij}}{t_i(\theta) t_j(\theta) / t^T(\theta)} \right)^{-(\sigma-1)} \quad (5)$$

[11] See Anderson and van Wincoop (2004) for an excellent survey of the literature on theoretical foundations
for the gravity model. In Anderson (1979), all prices were normalized to unity. In Bergstrand (1985, 1989, 1990), a "small-country" assumption was employed to treat the other N-1 countries' price levels as exogenous to the country pair ij. In Anderson and van Wincoop (2003) all countries' price levels are endogenous. Also, see Evenett and Hutchinson (2002) for a volume of papers on gravity equation methodology.

where $t_i(\theta) = \prod_{j=1}^{N} t_{ij}^{\theta_j}, t_j(\theta) = \prod_{i=1}^{N} t_{ji}^{\theta_i}, t^T(\theta) = \prod_{i=1}^{N} \prod_{j=1}^{N} t_{ij}^{\theta_i \theta_j}$, and recall $\theta_i = GDP_i / GDP^T$ and $t_{ij} = t_{ji}$.

The gravity equation in specification (1) has been used traditionally for about 40 years to explain the variation in bilateral trade flows among pairs of countries for a particular year and more recently for panel variation (especially, within variation using fixed effects, cf., Egger, 2000, 2002). Typically, several other binary variables are included to capture variation in various trade costs, such as an adjacency dummy and a language dummy. More relevant here, most researchers have included a dummy variable for the presence or absence of an EIA. As mentioned earlier, quantitative estimates of the coefficients of these EIA dummies have varied dramatically, cf., Frankel (1997), with some estimated average "treatment" effects seemingly small and others even negative. Estimates of gravity equation (2) for EIAs are scarce, since equation (2) surfaced in the past five years. Baier and Bergstrand (2002, 2007) provide some early estimates.

Determinants of EIAs

A key notion in this paper is that is that bilateral EIAs are – like bilateral trade flows – endogenous and – under certain assumptions – may be considered to be determined in a competitive setting as well. In considering what factors might explain whether or not certain country pairs are likely or unlikely to have an EIA, one needs to distinguish along two dimensions. First, we address static versus dynamic determinants of EIAs. In the static view taken in this section, we consider a world in "long-run equilibrium." We ask the question: what are some economic factors that explain theoretically whether or not a pair of countries is likely to have an EIA (in equilibrium)? We then examine empirically using a cross-section qualitative-choice econometric model whether or not the pairs of countries that have EIAs are the most likely ones to have such agreements, conditioned upon a set of economic determinants suggested by theory (relative economic sizes, relative factor endowments, trade costs, etc.) and that full *multilateral* free trade liberalization under the WTO is prohibitively expensive.[12]

Second, we must distinguish between the "economics" of EIAs versus the "politics" (or political economy) of EIAs.[13] In reality, of course, *national governments* are empowered to sign treaties regarding international commerce and factor mobility. In the international trade literature, it is common to assume that a representative (national) government's objective is to maximize a weighted average of the welfare of individuals (in economic terms, voters' utilities) and the influence of firms (in economic terms, firms' economic "rents" or profits), which likely operate through lobbies.[14] While both factors play a role in reality, we follow the intuitive suggestion by Bergsten (1996) that – in a long-run view – economic welfare is likely to be the dominant force, and that

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[12] In our theory, we assume that the decision to have or not have an FTA takes as exogenous the current WTO structure that impedes achieving "free" trade. We assume, as Bergsten (1996) states, "It simply turns out to be less time-consuming and less complicated to work out mutually agreeable arrangements with a few neighbors than with the full membership of well over 100 countries in the WTO," p. 4). This is also consistent with the approach taken in Grossman and Helpman (1995b) that, "As in Grossman and Helpman (1994a, 1995a), we suppose the incumbent government is in a position to set trade policy, which means here that it can either work toward a free trade agreement or terminate the discussions" (p. 670). A multilateral trade-policy alternative is ruled out by assumption. Also, since Bergsten wrote, there are now 150 parties to the WTO. Zissimos (2006) demonstrates in a game-theoretic setting the relevance of geography (i.e., trade costs) for the formation of FTAs.

[13] We borrow this useful distinction from Krugman (1991a).

[14] See, for example, Grossman and Helpman (1995b) or Gawande, Sanguinetti, and Bohara (2005). and Carrere (2006)

political factors (lobbies, special interest groups, etc.) are likely to be relatively more important in the short- to medium-run. Bergsten states:

> There are of course different national circumstances which explain the detailed strategies and timing of the individual initiatives. The overarching force, however, has been the process of competitive liberalization. The rapid increase of global interdependence has forced all countries, whatever their prior policies or philosophies, to liberalize their trade (and usually investment) regimes. ***Economic success in today's world requires countries to compete aggressively*** for the footloose international investment that goes far to determine the distribution of global production and thus jobs, profits and technology. (p. 2)

In our initial static analysis of selection into EIAs, we assume that the economic welfare of two nations' representative consumers determines whether or not the governments of that pair choose to have an EIA or not. To avoid the role of economic rents (or excessive profits), we assume monopolistically competitive markets for the production of goods, with large numbers of profit-maximizing firms that find political coordination prohibitively costly; this simplifies the model.[15] In a dynamic analysis that

[15] Even in a monopolistically competitive framework, countries might optimally choose higher tariffs in equilibrium. We assume they do not for three reasons: (1) the spirit of the GATT/WTO, where EIA members are precluded from raising their average external tariffs; (2) the Nash equilibrium may even yield a lowering of external tariffs (see work by Yi, 2000, and Ornelas, 2001); and (3) we have not observed increases in external tariffs (see empirical work by Estevadeordal, Freund and Ornelas, 2005).

addresses more the "timing" of formations of EIAs, political economy considerations and economic rents could surface.

Following in the spirit of Krugman (1991a,b), Frankel, Stein and Wei (1996), and Frankel (1997), Baier and Bergstrand (2004) created a model of a world economy with asymmetric countries recognizing explicitly inter- and intra-continental trade costs. Krugman (1991a) used a simple model of three symmetric (or identical) economies where firms produced slightly differentiated goods under increasing returns to scale in production to show that – in a world with *no* trade costs – regional EIAs decreased economic welfare of households unambiguously. However, Krugman (1991b) showed that in the same model – but with prohibitive inter-continental trade costs – regional EIAs increased economic welfare unambiguously. Frankel, Stein and Wei (1996) cleverly labeled this the "Krugman vs. Krugman" debate. Frankel, Stein, and Wei's extension of Krugman's model usefully allowed for a continuum of intercontinental trade costs, distinguishing "natural" EIAs (within continents) from "unnatural" EIAs (across continents). Frankel, Stein, and Wei could then show the cross-over point – in terms of inter-continental trade costs – at which on net welfare changed from positive to negative. Using some empirical estimates of the costs of inter-continental trade based upon a gravity model of trade, one conclusion from Frankel's (1997) book was that – if all continents followed the European example – the regionalization of the world economy would be "*excessive*."

In order to establish a quantitative model to predict which pairs of countries should or should not have an EIA, Baier and Bergstrand (2004) extended the Frankel-

Stein-Wei model to allow for asymmetric economies – both in terms of economic size and in relative factor endowments – and for asymmetric inter- and intra-continental transport costs. The model has six countries on three continents with countries on the same continent facing (Samuelson) iceberg-type intra-continental trade costs and countries on different continents facing additional iceberg-type inter-continental trade costs. Each country is endowed with two factors of production, capital (K) and labor (L). There are two industries, goods and services, with preferences for the two sectors' outputs of the Cobb-Douglas type. Preferences for each sector's output are of the constant-elasticity-of-substitution (CES) type, common to the trade literature. Each sector's products are slightly differentiated, with each product produced under increasing returns to scale; consumers value variety. The production of goods and of services uses capital and labor in different relative factor intensities. Standard demand functions are generated, the details of which are discussed in Baier and Bergstrand (2004).

If governments are welfare maximizers, then – in the context of this model – certain *economic characteristics* are likely to favor EIAs' formation in some pairs of countries relative to others.[16] For example, two important economic factors influencing trade and utility are intracontinental and intercontinental trade costs. First, countries that are closer together (on the same continent) benefit more from an EIA because, with lower intra-continental trade costs, they are already large traders. Second, the net benefits of a

[16] Moreover, in the context of 20,000 potential bilateral interactions, each government is assumed to operate competitively taking as given the behavior of other governments (and welfare of their consumers).

natural EIA increase (and the net costs of an unnatural EIA decrease) as intercontinental trade costs rise, because more remote countries trade little with distant countries.

Baier and Bergstrand (2004) demonstrate also that pairs of larger GDP economies tend to benefit more from EIAs than pairs of smaller countries, due to economies of scale in production and increased varieties of products available. As two countries' GDPs become more different, the likelihood of an EIA decreases. A larger economy's benefit from an EIA diminishes as the two countries become more dissimilar in size (for a given total economic size) because the breadth of variety in imports from a small EIA partner contracts for the larger economy.

Due to the presence of two industries and two factors, the wider the relative factor endowments of a country pair, the more likely an EIA (if inter-continental transports are sufficiently high) due to the gains of exchange relative comparative advantages, i.e., inter-industry trade. However, the wider the difference in two partners' relative factor endowments relative to the rest-of-the-world, the less likely an EIA. It is important to note – as perhaps surmised already – that most (if not all) of these economic factors are also well established as economic determinants of *bilateral trade flows.*

Based upon the qualitative-choice econometric model of McFadden, Baier and Bergstrand (2004) used a probit model to try to establish empirically the relative importance of these factors for explaining –and potentially predicting – the likelihood of an EIA between country pairs. We employed a sample of bilateral pairings among 54 countries, or 1431 observations for EIAs observed in 1996 [(54x53)/2 =1431]. These probabilities are predicted using bilateral distances, GDP sizes, GDP similarities, relative

K/L ratios, and indexes of remoteness (or multilateral resistance) as explanatory variables, cf., Baier and Bergstrand (2004).

We draw attention to three empirical outcomes. First, the empirical probit model actually works quite well. As a measure of overall fit, the pseudo-R2 value of the full specification is 73 percent for 1431 country pairs. We note that for a (more recently constructed) wider sample of 96 countries in 1995, the pseudo-R2 remains high at 67 percent. Of the 286 EIAs in 1996 in our original sample, the model predicted 85 percent (or 243) correctly. Of the remaining 1145 pairs with no EIAs, the model predicted correctly 97 percent (1114=1145-31). Details are available in Baier and Bergstrand (2004). We note that the most likely EIAs in 1996 (using exogenous geographic variables and GDPs and K/L ratios from 1960) were the earliest EIAs.

Second, of the top 200 pairs (of 1431) that were the most likely to have an EIA in 1996, only six pairs did not have one: Iran-Iraq, Iran-Turkey, Chile-Peru (EIA being negotiated), Japan-South Korea (EIA being negotiated), Hong Kong-South Korea, and Panama-Venezuela.

Third, of the 1000 pairs (of 1431) that were the least likely to have an EIA in 1996, only four pairs actually had an EIA: Portugal-Turkey, Egypt-Iraq, Mexico-Chile, and Mexico-Bolivia.

Simultaneous Markets for Trade Flows and EIAs

Why does the model work so well? We believe the model is consistent with the notion of "competitive liberalization." National governments realize countries are unique in economic characteristics. In the interest of liberalizing markets to improve productivity levels and levels of living standards, national governments select into arrangements with other countries for which they share certain economic characteristics, such as similar economic size or close in distance. Empirically, most pairs of countries with EIAs tend to have the key economic characteristics that the theoretical model suggests should be present for an EIA to enhance (on net) the welfare of pairs' representative consumers. In many (if not most) cases, these are pairings where countries already trade extensively with one another. This is consistent with Bergsten's "competitive liberalization" notion that economic welfare may be the dominant long-run "overarching" force in driving regionalism, despite political factors influencing timing, etc. Hence, the same *observable variables* that explain trade patterns – gravity-equation variables – *also explain* the likelihood of an EIA because of likely net benefits for producers and consumers from creating such an EIA. Hence, one can argue that *ex post* country pairs that have chosen to have EIAs have "chosen well."

The reader might ask a seemingly obvious question: If national governments are simply maximizing consumers' welfare, why not simply predict bilateral EIAs with bilateral trade flows? First, there is an "endogeneity" issue. Predicting the likelihood of an EIA based upon a probit regression using trade flows on the RHS will likely yield biased coefficient estimates. The reason is that "unobservable" variables – such as institutional and political factors – that likely influence the decision by governments to form EIAs also tend to influence trade flows. In cross-sectional data, these unobservable – to the econometrician – variables likely influence both EIA and trade variables. The coefficient estimates in the probit regression would be biased. Second, the probit specification we use helps identify the “(exogenous) economic characteristics” that influence the decision to form an EIA: economic geography variables, factors influencing intra-industry trade, and factors influencing inter-industry trade.

The approach and results just discussed have some potentially important implications for the forty-five years of empirical research using the gravity equation with cross-sectional data discussed in section I.A. Since Nobel Laureate Jan Tinbergen (1962) first used the gravity equation, the equation has been used increasingly to estimate the impact of EIAs on members’ trade flows. Tinbergen (1962) studied bilateral international trade flows among several countries in a cross-section from the 1950s including dummy variables for the BENELUX FTA and the British Commonwealth members; he found that membership in either of these agreements increased trade by only 5 percent. However, the previous discussion suggests that cross-section estimates of EIAs’ effects on trade over these forty years suffer from potential selection bias. If country pairs select into EIAs for unobservable reasons correlated with potential trade flows, OLS estimates will likely be biased.[17]

To support our claim that estimates of the impact of EIAs may be biased, we provide in Table 1 coefficient estimates from a typical cross-section gravity equation for multiple years: 1960, 1970, 1980, 1990, 2000. These coefficient estimates come from a typical log-linear version of equation (1) amended to include dummy variables for common land border (adjacency), common language, and common membership in

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[17] A case where this is least likely to occur is the original EEC6 countries, formed based upon strong political and national security considerations. Consequently, plausible estimates of the trade effects of the EEC6 in Aitken (1973) may well be unbiased.

various EIAs, estimated using the (non-zero) nominal trade flows among the 96 countries identified in the Data Appendix. These estimates are derived including separate EIA dummy variables for the European Union (EU), the European Free Trade Association (EFTA), the European Economic Area (EEA), and all "other" EIAs (OEIAs). *EUijt* is defined to equal 1 if a country pair ij in year t were members of the European Economic Community (1960 to 1970), the European Community (1975 to 1990), or the European Union (1995 and 2000), and 0 otherwise. *EFTAijt* is defined to equal 1 if a country pair ij in year t were members of EFTA, and 0 otherwise. *EEAijt* is defined to equal 1 if one country was in *EU* and the other was in *EFTA* in year t; members of the EC (EU) formed (maintained) FTAs with remaining EFTA members in 1973 (1994). *OEIAijt* is defined as 1 if country pair ij in year t had any other EIA agreement.

We describe briefly the data used for the gravity equations. Nominal bilateral trade flows are from the International Monetary Fund's *Direction of Trade Statistics* for the years 1960, 1965, . . ., 2000 for 96 potential trading partners (zero trade flows are excluded); these data are scaled by exporter GDP deflators to generate real trade flows for the panel analysis. Nominal GDPs are from the World Bank's *World Development Indicators* (2003); these are scaled by GDP deflators to create real GDPs for the panel analysis. Bilateral distances were compiled using the *CIA Factbook* for longitudes and latitudes of economic centers to calculate the great circle distances. The language and adjacency dummy variables were compiled also from the *CIA Factbook.* The EIA dummy variables were calculated using appendices in Lawrence (1996) and Frankel

(1997), various websites, and EIAs notified to the GATT/WTO under GATT Articles XXIV or the Enabling Clause for developing economies; we included only full (no 14 partial) EIAs. Table 3 lists the trade agreements used and sources.[18]

As Table 1 shows, common membership in *EU* had an economically significant effect in 1960 and 1970 only, with the sole statistically significant positive effect in 1960 – only three years into the original EEC agreement. These results are surprising. Second, common membership in *EFTA* had an economically and statistically significant effect on trade in 1960 (the year the agreement came into effect!) and in 1970 only. In fact, common membership in EFTA had more than *twice* the effect on members' trade than common membership in *EU*. These results are surprising. Third, common membership in any other EIA (*OEIA*) had a positive and economically significant effect in all five years examined, although the coefficient estimate is statistically different from zero in only three years of the sample (1960, 1980, 2000). Moreover, in 1970 the effect of other FTAs was to increase trade by *1900 percent.* Consequently, the results for *OEIA* are quite fragile. All in all, the empirical results using a typical gravity equation specification – assuming the EIA variables are exogenous – are not very supportive that EIAs actually work.

As discussed earlier, typical gravity equation (1) is likely misspecified owing to ignoring theoretical foundations that have developed over the past several decades. Table 2 provides estimates of theoretically motivated gravity equation (2) using (as is now common) country-specific fixed effects to account for the variation of multilateral price

[18] The data set is available at the authors' websites (http://www.nd.edu/~jbergstr and http://people.clemson.edu/~sbaier).

terms P_i and P_j in equation (2) and restricting the coefficient estimates for GDPs to be unity (as suggested by theory). As Table 2 reports, accounting for the theoretically-motivated multilateral price terms does improve the results for EIA effects relative to Table 1. If anything, estimates from the theoretically-motivated gravity equation (2) using country fixed effects lend *even less* support to the notion that *ex post* EIAs actually work.[19]

The reason why the EIA variables' coefficient estimates may be biased is perhaps due to the *endogenous* determination of EIAs in a competitive environment. For instance, in equations (1) or (2), the error term ε may be representing unobservable (to the empirical researcher) policy-related barriers tending to reduce trade between countries i and j that are not accounted for by standard gravity equation RHS variables, but may be correlated with the decision to form an EIA. Suppose two countries have extensive immeasurable domestic regulations (say, internal shipping regulations) that inhibit trade (causing ε to be negative). The likelihood of the two countries' governments selecting into an EIA may be high if there is a large expected welfare gain from potential bilateral trade creation if the EIA deepens liberalization beyond tariff barriers into domestic regulations (and other non-tariff barriers). Thus, EU_{ijt} and the intensity of domestic regulations may be positively correlated in a cross-section of data, but the gravity equation error term ε_{ijt} and the intensity of domestic regulations may be negatively

[19] It should be remembered throughout that the discussion of "effects" of an EIA are limited only to the primary "direct" effect associated with the dummy variable's coefficient estimates, and we are intentionally precluding from our discussion the full general-equilibrium comparative-static effects addressed in Anderson and van Wincoop (2003) and Baier and Bergstrand (2006).

correlated. This suggests that EU_{ijt} and ε_{ijt} are negatively correlated, and the *EU* coefficient estimate may be underestimated.

Numerous authors have noted that one of the major benefits of regionalism is the potential for "deeper integration." Lawrence (1996, p. xvii) distinguishes between "international policies" that deal with border barriers, such as tariffs, and "domestic policies" that are concerned with everything "behind the nation's borders, such as competition and antitrust rules, corporate governance, product standards, worker safety, regulation and supervision of financial institutions, environmental protection, tax codes ..." and other national issues. The GATT and WTO have been remarkably effective in the post-WWII era reducing border barriers such as tariffs. However, these institutions have been much less effective in liberalizing the domestic policies just named. As Lawrence states it, "Once tariffs are removed, complex problems remain because of differing regulatory policies among nations" (p. 7). He argues that in many cases, EIA "agreements are also meant to achieve deeper integration of international competition and investment" (p. 7). Gilpin (2000) echoes this argument: "Yet, the inability to agree on international rules or to increase international cooperation in this area has contributed to the development of both managed trade *and regional arrangements*" (p. 108; italics added).

We believe this omitted variable (selection) bias is the major source of endogeneity facing estimation of EIA effects in gravity equations using cross-section data. Moreover, the arguments above suggest that policymakers' decisions to select into an EIA are likely related to the *level* of trade (relative to its potential level), and not to recent changes in trade levels. Thus, the determinants of *EU, EFTA, EEA,* and *OEIA* are likely to be cross-sectional in nature.

Estimating the Effects of Various EIAs on Trade Flows using Panel Data

With cross-section data, standard econometric techniques to address omitted variables (and selection) bias include estimation using instrumental variables and Heckman control functions. Only a small handful of studies in the past three years have attempted to do this; Baier and Bergstrand (2002) was the first. Of the few studies that have attempted to solve this dilemma using instrumental variables and other cross-section techniques, there has been little success, cf., Baier and Bergstrand (2007). The reason basically is that – in cross-section – it is virtually impossible in a convincing way to identify variables that are correlated with the EIA dummy variable and are uncorrelated with trade flows. Baier and Bergstrand (2002) explored myriad possible trade-related instrumental variables (specifically, they tried capital-labor ratios, factor endowment differences relative to the rest of the world, and remoteness of continental FTA partners), concluding that none of the instruments can be shown to be sufficiently exogenous for two reasons. First, the multi-step estimation procedure detailed by Wooldridge (2002, ch. 18) and Baier and Bergstrand (2002) precludes a test of the over-identifying restrictions that establish, empirically, that the instruments are truly "exogenous." Second, the instruments that have been conceived of have also been used in gravity equation estimates, and estimates of their effects are often statistically significant; this indicates that they likely *are* correlated with the error term in our theoretically-motivated gravity equation. Baier and Bergstrand (2002) also report trying many political variables as instruments, but the same drawbacks exist for them as for the trade-related variables. Thus, there appear to be no observable variables to *identify* the respective equations.

However, some alternative techniques are available to address the problem. For example, if the decisions to form EIAs are "slow-moving" – as they are likely to be – but trade flows are not slow moving (also likely), then panel data offers an opportunity to better identify unbiased effects of EIAs on trade flows. Bayoumi and Eichengreen (1997) pursued this using first-differences and Cheng and Wall (2005) using fixed effects, but both in the context of atheoretical gravity specifications with small samples.

Baier and Bergstrand (2007) used both approaches in the context of a theoretically-motivated gravity equation for a broad sample of countries and panel data. Starting from the conditional general equilibrium of Anderson and van Wincoop (2003), Baier and Bergstrand (2007) motivated the panel version of the Anderson and van Wincoop gravity equation:

$$\begin{aligned} &\ln[X_{ijt}/(RGDP_{it}RGDP_{jt})] = \beta_0 + \beta_3(\ln DIST_{ij}) + \beta_4(\ln ADJ_{ij}) \\ &+ \beta_5(\ln LANG_{ij}) + \beta_6(\ln EIA_{ijt}) - \ln P_{it}^{1-\sigma} - \ln P_{jt}^{1-\sigma} + \varepsilon_{ijt} \end{aligned} \tag{6}$$

where X_{ijt} is the real (inflation-adjusted) trade flow from i to j in year t and $RGDP_{it}$ is real GDP of country i in year t and *EIA* is used generically to represent the set of *EU, EFTA, EEA,* and *OEIA*.

Using fixed effects, Baier and Bergstrand (2007) find that the cumulative average treatment effect of an EIA on trade after 10-15 years is 0.76. Given that *e*0.76 equals 2.14, this implies that an EIA on average increases two member's international trade by

114 percent after 10-15 years. This estimated effect is both considerably larger and more robust to sensitivity analyses than earlier estimates.

In this paper, we examine in particular the effects of EU membership, EFTA membership, EEA membership, and membership in *all other EIAs* using these techniques. Thus, in contrast to Baier and Bergstrand (2007) which treated the effects of all EIAs the same, this paper applies the *ex post* techniques of Baier and Bergstrand (2007) to examine some specific agreements, allowing here for changing membership over the forty year period from 1960-2000. We have two paper goals in mind for the remainder of this analysis. First, we want to try to estimate *with precision* (and robustness) the *ex post* effects of various Western European trade agreements on members' international trade, accounting for the endogeneity of trade agreements' formation. Second, we want to establish that the economic effects of trade agreements on members' trade were *much larger* than previous estimates have suggested, which will help to explain the proliferation of trade agreements in later years.

Alternative Panel Estimation Techniques: Fixed versus Random effects

Our panel estimation applies fixed effects rather than random effects for two reasons, the first on conceptual grounds and the second on empirical grounds. First, as addressed in section 2, we believe the source of endogeneity bias in the gravity equation is unobserved time-invariant heterogeneity. In economic terms, we believe there are unobserved time-invariant bilateral variables – termed *wij* – influencing simultaneously the presence of an EIA and the volume of trade. Because these variables are likely correlated with *EUij*, *EFTAij*, *EEAij*, and *OEIAij* they are best controlled for using bilateral "fixed effects," as this approach allows for arbitrary correlations of *wij* with these variables. By contrast, under "random effects" one assumes zero correlation between unobservables *wij* with *EUij*, *EFTAij*, *EEAij*, and *OEIAij*, which seems less plausible.

Second, recent econometric evaluations of the gravity equation with panel data have used the Hausman Test to test for fixed versus random effects. For example, Egger (2000) finds overwhelming evidence for the rejection of a random-effects gravity model relative to a fixed-effects gravity model, using either bilateral-pair or country-specific fixed effects.

Fixed Effects versus First Differencing

A standard discussion on the treatment of endogeneity bias using panel data focus on a choice between estimation using fixed effects versus using first-differenced data, cf., Wooldridge (2002, Ch. 10). Wooldridge notes that when the number of time periods (T) exceeds two, a fixed-effects estimator is more efficient under the assumption of serially uncorrelated error terms. When $T > 2$ and the error term ε_{ijt} follows a random walk (i.e., that the difference in the error terms, ε_{ijt}-$\varepsilon_{ij,t-1}$, is white noise), the first differencing estimator is more efficient.[20]

It is possible that first-differencing the panel data yields some potential advantages over fixed effects. First, it is quite plausible that the unobserved heterogeneity in trade flows, ε_{ijt}, is correlated over time. Following the points in section II, unobserved

[20] When the number of time periods is limited to two (T=2), estimation with fixed effects and first differencing produce *identical* estimates and inferences; moreover, first-differencing is easier. When T>2, the choice depends upon the assumption the researcher makes about the error term ε_{ijt}.

factors influencing the likelihood of an EIA (say, trade below its "natural" level) are likely slow moving and hence serially correlated. If the εijt are highly serially correlated, the inefficiency of fixed effects is exacerbated as *T* gets large. This suggests that differencing the data will increase estimation efficiency for our large-*T* panel. Second, aggregate trade flow data and real GDP data are likely "close to" unit-root processes. Using fixed effects is equivalent to differencing data around the *mean* (in our sample, 1980); this may create a problem since *T* is large in our panel. As Wooldridge (2000, p. 447) notes, if the data follow unit-root processes and *T* is large, the "spurious regression problem" can arise in a panel using fixed effects. First-differencing yields data that deviates from the previous period of our panel, and thus is closer to a unit-root process. In the following, we use fixed effects in sections 3.3 and 3.4, and for robustness we use differenced data in section 3.5.

Fixed-Effects Estimation of an Atheoretical Gravity Equation Ignoring Multilateral Price Terms

In a panel context, equation (1) can be expressed as:

$$\begin{aligned}\ln X_{ijt} &= \beta_0 + \beta_1(\ln RGDP_{it}) + \beta_2(\ln RGDP_{jt}) + \beta_3(\ln DIST_{ij}) + \beta_4(ADJ_{ij}) \\ &+ \beta_5(LANG_{ij}) + \beta_6(EIA_{ijt}) + \varepsilon_{ijt}\end{aligned} \quad (7)$$

Table 4 provides the empirical results of estimating gravity equation (7) using a panel of real trade flows (X_{ijt}), real GDPs ($RGDP_{it}$, $RGDP_{jt}$) and EIA dummies (EU_{ij}, $EFTA_{ij}$, EEA_{ij}, and $OEIA_{ij}$), and using alternative specifications with and without bilateral fixed effects and time dummies. Column (1) provides the baseline gravity equation without any fixed effects or time dummies for all nine years. Exporter and importer (real) GDPs have coefficients close to unity, distance has a traditional coefficient estimate of -1, and the adjacency and language dummies have typical coefficient estimates.

However, other than $OEIA_{ij}$ the coefficient estimates for the Western European EIAs are quite unstable across agreements, suggesting fragile estimates. Although *EFTAij* has an economically and statistically significant value of 0.33 (suggesting that EFTA increased trade by *e*0.33 = 39 percent), membership in various stages of the EEC/EC/EU had a statistically significant *negative* effect on members' trade, as did the EEA's EU-EFTA free trade agreements. Such results seem implausible.

Column (2) provides the empirical results including a time dummy, where (for brevity) we omit reporting the (statistically significant) coefficient estimates for these time dummies. Although the inclusion of the time dummies causes the RGDP elasticities to move closer to unity, the coefficient estimates for the time-invariant variables (distance, adjacency, and language) are unaffected. However, coefficient estimates for *EUij*, *EFTAij*, *EEAij*, and *OEIAij* are all affected. Now, even the coefficient estimate for *EFTAij* is surprisingly negative and statistically insignificant. Moreover, the *OEIAij* coefficient estimate becomes very large, 1.12, implying that non-Western European EIAs on average increase trade by 200 percent. This result also seems implausible. However, time dummies do not adjust for the endogeneity of EIAs.

Adjusting for unobserved time-invariant heterogeneity using bilateral fixed effects has a notable impact on the results. Column (3) provides results including bilateral fixed effects. The coefficient estimates for *EUij*, *EFTAij*, *EEAij*, and *OEIAij* now are *all* plausible and are statistically significant. It is worth noting now that the coefficient estimates for *EUij*, *EFTAij*, and *OEIAij* are also *all virtually identical* quantitatively (0.58, 0.55, and 0.57, respectively), each implying that the particular agreement increases trade about 75 percent. Membership in EEA_{ij} increases bilateral trade about 40 percent.[21]

Column (4) in Table 4 combines the inclusion of bilateral fixed effects and time dummies. One notable change occurred in the coefficient estimates for EU_{ij}, $EFTA_{ij}$, EEA_{ij}, and $OEIA_{ij}$ for this specification relative to the previous one. First, the coefficient estimate for EU_{ij} increases substantively, suggesting that membership in the EU increased trade of the typical country pair during the period by 144 percent. A second more minor difference is that the coefficient estimate for EEA_{ij} increased while those for $EFTA_{ij}$ and $OEIA_{ij}$ stayed approximately the same.

Column (5)'s specification differs from column (4)'s only by restricting the coefficient estimates for the (time-varying) real GDP variables to be unity. This reduces the overall explanatory power (Within R^2), but has only minor implications for the EU_{ij}, $EFTA_{ij}$, EEA_{ij}, and $OEIA_{ij}$ coefficient estimates.

Overall, the inclusion of bilateral fixed effects and time-varying dummies has made the coefficient estimates for EU_{ij}, $EFTA_{ij}$, EEA_{ij}, and $OEIA_{ij}$ more economically plausible and statistically significant. If (as we will argue shortly) the effects of an EIA on trade took 15 years to play themselves out, the coefficient estimates from column (5)

[21] The only other published studies that have estimated the ATE of an EIA using a panel of data spanning as many years and countries are Rose (2004) and Tomz, Goldstein and Rivers (2004). Using fixed effects, Rose found an ATE of *e*0.94 or 156 percent. However, using a classification of formal and informal GATT members, Tomz, Goldstein and Rivers (2004) estimate an ATE for EIAs (with fixed effects) of only *e*0.76 or 114 percent. Cheng and Wall (2002) used bilateral fixed effects in a four-year panel of trade among approximately only 30 high-income countries in the context of a traditional gravity equation ignoring multilateral price terms.

imply that common membership in the EU (beginning with the original six EEC countries) increased trade (in real terms) about 5.6 percent annually over 15 years. Common membership in EFTA (or the EC-EFTA trade pacts) increased trade about 3.5 percent annually and membership in any other EIA increased trade about 4.3 percent annually.

How do these results compare to previous ones? Bayoumi and Eichengreen (1997) examined the impacts of common membership in the original EEC6 and in the original EFTA7, but only over a much shorter period, 1956-1973. They found implied annualized impacts of only 3.2 and 2.3 percent, respectively, over the period. These are significantly lower than our estimates of 5.6 and 3.5 percent annually, respectively, over 1960-2000. By contrast, our estimate for *OEIAij* membership was 0.63, which is considerably lower than comparable estimates using similar specifications in Rose (2004) and Tomz, Goldstein, and Rivers (2007) of 0.94 and 0.76, respectively.

However, we emphasize that all these estimates used an “atheoretical” specification for the gravity equation. If we account for recent theoretical advances in foundations for the gravity equation, slightly different specifications from those above surface. The specifications above suffer *ex ante* from ignoring *time-varying* multilateral price terms, as suggested by recent theoretical developments. In the next section, we account for such terms, as well as the potential influence of “phasing-in” agreements.

Fixed-Effects Estimation of a Theoretically-Motivated Gravity Equation with Phased-In Agreements

In this section, we consider three modifications to the previous specification. In section 3.4.1, we include country-and-time effects to account for the theoretically-motivated multilateral price terms. In section 3.4.2, we account for the fact that all EIAs are “phased-in” over time, typically over five-to-ten years, and for the possibility that the change in two members’ terms of trade from formation of an EIA may have a lagged impact on their bilateral trade. In section 3.4.3, we address “strict exogeneity” issues; we test for the possibility of reverse causality by addressing the effect of *future* EIA dummies on current trade flows.

Accounting for Multilateral Price Terms

While the results in the previous section are encouraging, the gravity equation suggested by recent formal theoretical developments – summarized in the system of equations (2)-(4) in section 1 –suggests that one needs to account for the multilateral price variables. None of the four specifications in Table 4 accounts for these. First, accounting for the multilateral price variables in a panel context suggests estimating:

$$\begin{aligned} \ln X_{ijt} &= \beta_0 + \beta_1(\ln RGDP_{it}) + \beta_2(\ln RGDP_{jt}) + \beta_3(\ln DIST_{ij}) + \beta_4(\ln ADJ_{ij}) \\ &+ \beta_5(\ln LANG_{ij}) + \beta_6(\ln EIA_{ij}) - \ln P_{it}^{1-\sigma} - \ln P_{jt}^{1-\sigma} + \varepsilon_{ijt} \end{aligned} \tag{8}$$

As before, scaling the LHS variable by the product of real GDPs suggests estimating:

$$\begin{aligned} \ln[X_{ijt} /(RGDP_{it} / RGDP_{jt})] &= \beta_0 + \beta_3(\ln DIST_{ij}) + \beta_4(\ln ADJ_{ij}) + \beta_5(\ln LANG_{ij}) \\ &+ \beta_6(\ln EIA_{ij}) - \ln P_{it}^{1-\sigma} - \ln P_{jt}^{1-\sigma} + \varepsilon_{ijt} \end{aligned} \tag{9}$$

In a panel setting, the multilateral price variables would be *time varying*, and consequently the results in specifications (1)-(5) in Table 4 may suffer from an omitted variables bias as a result of ignoring these time-varying terms – a dilemma that cannot be resolved by the use of bilateral fixed effects and time dummies using the panel data in its current form.[22] Moreover, the theoretical model in equation (2) suggests that the coefficient estimates for the real GDP variables should be unity, as reported in specification (5) in Table 4.

We first estimate equation (8) using bilateral (*ij*) fixed effects to account for variation in *DIST, ADJ,* and *LANG* along with country-and-time (*it, jt*) effects to account for variation in real GDPs and the multilateral price terms. In the context of the theory (though ignoring the restriction of unitary income elasticities), this should generate an unbiased estimate of β_6.

Column (1) in Table 5 provides the results of estimating this equation using bilateral fixed effects and the country-and-time effects. We note two observations. First, all the coefficient estimates for the effects of EU_{ij}, $EFTA_{ij}$, EEA_{ij}, and $OEIA_{ij}$ on trade are diminished (relative to those in Table 4) by accounting for the theoretically-motivated multilateral price terms. Second, there is a notable change in the *relative effects* of the agreements. Common membership in the European Union (or, as appropriate in early years, EEC or EC) declines only slightly. Membership in the EU still increased trade by almost 100 percent. Membership in any other EIA increased trade by almost 60

[22] Random effects estimation would not be of any use either, as theory suggests that the multilateral price terms and the EIA variable would be correlated.

percent. However, membership in EFTA had no effect. The EC-EFTA free trade agreements that began in 1973, and continued in the 1994 EEA agreement, boosted trade by about 20 percent, considerably less than in Table 4's results.

Column (2) of Table 5 imposes explicitly unitary elasticities for real GDPs. However, in the presence of the *it* and *jt* dummies, this restriction is redundant, except for influencing the intercept estimate. Scaling or not scaling real trade flows by real GDPs will not matter for estimating the ATE in this specification. In log-linear form, the variation in the logs of real GDPs is captured by the country-and-time (*it, jt*) effects, and only the estimates of the intercept and the country-and-time effects' coefficients change; the *EIA* coefficient estimate is unaffected. In the remainder of the results, we use the real trade flow for the LHS variable; the EIA coefficient estimates are identical using trade shares instead (and are available on request).

Accounting for "Phased-In" Agreements and Lagged Terms-of-Trade Effects

In this section, we introduce lagged effects of EIAs on trade. The economic motivation for including lagged changes stems partly from the institutional nature of virtually all EIAs. The 0-1 EU_{ij}, $EFTA_{ij}$, EEA_{ij}, and $OEIA_{ij}$ variables were constructed using the "Date of Entry into Force" of the agreement, as best surmised by scrutinizing multiple data sources provided earlier. However, virtually every EIA is "phased-in," typically over 10 years. For instance, the original EEC agreement of 1958 had a 10-year phase-in period; NAFTA had a similar 10-year provision. Thus, the entire economic (treatment) effect cannot be captured fully in the concurrent year only. It is reasonable to expect an EIA entered into "legally" in 1990 to not come into economic effect fully until

2000. Thus, it is reasonable to include one or two lagged levels of the EIA dummy (e.g., $EU_{ij,t-1}$ and/or $EU_{ij,t-2}$).

Moreover, economic effects of an EIA include altering the terms of trade. However, as is well known from a large literature in international economics, terms-of-trade changes tend to have lagged effects on trade volumes. Thus, it is reasonable to assume that an EIA which enters into force in 1960, and which is even fully "phased-in" by 1965, might still have an effect on trade flows in 1970.

The results in columns (3) and (4) in Table 5 reveal that EU_{ij} has a statistically significant lagged effect on trade flows. Moreover, the coefficient estimates have economically plausible values, balanced across periods. In column (3), the sum of the two ATEs for EU_{ij} is 0.82 – identical in magnitude to the EU_{ij} coefficient estimate in column (5) of Table 4. With two lags, the coefficient estimate for one of the two lagged terms is statistically insignificant; however, summing the coefficient estimates yields a total ATE of 0.90. Since this ATE reflects the effect of EU membership over approximately 15 years, the implied average annual effect on members' trade across the 15-year transition period is 6.2 percent. This is only slightly larger than our earlier estimate (using the atheoretical gravity equation), and is roughly *twice* the average annual ATE found in Bayoumi and Eichengreen for the original EEC6 countries.

We will discuss the results and implications for *all other EIAs* (*OEIAijt*) in a later section.

Strict Exogeneity

The results of previous sections suggest that – after accounting for endogeneity using panel data – one can find economically significant ATEs for *EIA*. However, to confirm that there are no "feedback effects" from trade changes to *EIA* changes, we run one more specification using the fixed-effects approach.[23] Wooldridge (2002, p. 285) suggests that it is easy to test for the "strict exogeneity" of EIAs in our context. To do this, we add *future* levels of EU_{ij}, $EFTA_{ij}$, EEA_{ij}, and $OEIA_{ij}$ to the regression model. In the panel context here, if EU_{ij}, $EFTA_{ij}$, EEA_{ij}, and $OEIA_{ij}$ changes are strictly exogenous to trade flow changes, *EUij,t+1*, *EFTAij,t+1*, *EEAij,t+1*, and *OEIAij,t+1* should be uncorrelated with the concurrent trade flow. The results in column (5) of Table 5 confirm this. In only case did *EUij,t+1*, *EFTAij,t+1*, *EEAij,t+1*, and *OEIAij,t+1* affect the trade flow *Xijt* materially; except for *EEAij,t+1*, in all cases the coefficient estimate is not significantly different from zero. Moreover, the consistently negative coefficient estimates suggest – if anything – that firms delay trade temporarily in anticipation of an impending agreement.

First-Differenced Panel Gravity Equation Estimates

As discussed in section 3.2, for two econometric reasons we may expect first-differenced data to provide better estimates of the average treatment effect than using "fixed effects." At worst, differenced data provide an evaluation of the robustness of previous estimates. In the context of differenced panel data, the potential omitted

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[23] An empirical finding that trade leads an EIA need not even imply that trade "causes" an EIA. Trade may increase in anticipation of an EIA as infrastructure and delivery systems involving sunk costs are redirected, cf., McLaren (1997). Alternatively, trade may decrease – be delayed – in anticipation of the benefits of an EIA.

variables bias created by time-varying multilateral price terms for each country would require again country-and-time effects to obtain consistent estimates of the EIAs' ATEs. As before, with country-and-time effects the coefficient estimates of the EIA treatment effects are insensitive to the real bilateral trade flow being scaled or not scaled by real GDPs; for consistency to earlier results, we present those for the flows (the virtually identical results are available on request using trade flows scaled by the product of real GDPs). We start by first-differencing the natural logarithm of *Xijt*, creating *dlnXij,t-(t-1)*. Second, we regress *dlnXij,t-(t-1)* on 768 country-and-time effects (*Dumi,t-(t-1)*, where *i* denotes a country and *t-(t-1)* a 5-year period, e.g., 1995-2000) and retain the residuals. Third, we difference *EUijt*, creating *dEUij,t-(t-1)*, and regress *dEUij,t-(t-1)* on the same 768 country-and-time fixed effects and retain these residuals (and do the same for *EFTAij*, *EEAij*, and *OEIAij*). Fourth, a regression of the residuals from the first (*dlnX*) regression on the residuals from the other regressions will yield unbiased estimates of the ATE effect of an EIA holding constant time-varying multilateral price terms.

The procedure described above is equivalent to estimating:

$$d\ln X_{ij,t-(t-1)} = \beta_6 dEIA_{ij,t-(t-1)} + \beta_{i,t-(t-1)} Dum_{i,t-(t-1)} + \beta_{j,t-(t-1)} Dum_{j,t-(t-1)} + v_{ij,t-(t-1)} \quad (10)$$

where *dEIA* represents any of the four trade agreements we have been investigating and $v_{ij,t-(t-1)} = \varepsilon_{ij} - \varepsilon_{ij,t-1}$ is white noise. With nine years in the panel, we have 8 time periods *t-(t-1)*. Since there are 96 countries that can potentially trade, our procedure above effectively introduces 768 (= 8 x 96) country-and-time fixed effects ($Dum_{i,t-(t-1)}$ and $Dum_{j,t-(t-1)}$) to account for the changes in the unobservable theoretical multilateral resistance terms,

$d \ln P_{i,t-(t-1)}^{1-\sigma}$ and $d \ln P_{j,t-(t-1)}^{1-\sigma}$, to obtain an unbiased estimate of β_6. In the context of the theoretical model, the 768 estimates of $\beta_{i,t-(t-1)}$ and $\beta_{j,t-(t-1)}$ can be interpreted as changes in the countries' multilateral resistance terms.

Table 6 reports the coefficient estimates for the effects of concurrent, lagged and future changes in four agreements on trade flow changes. For the European Union, columns (1)-(4) all report slightly smaller coefficient estimates for the *EU* effect than the respective estimates in Table 5 using fixed effects. For EFTA, the results are more plausible. However, as in Table 5, the effects of EFTA are quite small. Using first differences, the effects of EC-EFTA free trade agreements are small as well, but largely similar to those in Table 5. As with the EU, the effects for all other EIAs are diminished using first differences relative to fixed effects.

The major point worth noting from an empirical standpoint is that the results using first differencing provide strong support for the robustness of the previous estimates in this section using fixed effects for the theoretically-motivated gravity equation. Membership in the EEC/EC/EU had an economically and statistically significant effect on trade among members between 1960 and 2000. This result is robust across many specifications. The small variation in results, say, between column (2) in Table 5 and Table 6 – total ATEs of 0.82 and 0.70, respectively (depending upon one's preferences over underlying assumptions about the error structure) – suggest that these results are fairly *precise* and robust. In average annual percent changes, the two effects are 5.6 and 4.8 percent, respectively, over a 15 year period. For all other EIAs, the results for the two approaches (using column (2) results again) are 0.77 and 0.59.

Implications for Understanding the "Latest Wave" of Regionalism

What do these empirical results mean for better understanding the "latest wave" of regional trade and cooperation agreements? National policy makers around the world operating in an increasingly competitive global environment face strong pressure from their national constituents (firms, households) to maximize their economic status (profits and consumer welfare, respectively). Such policy makers are likely making decisions about trade policies in a competitive environment. The proliferation of bilateral and regional EIAs in the world economy likely mirrors the proliferation of bilateral and regional trade in the world economy. There is a world market for goods and services is met efficiently by bilateral trade flows. Correspondingly, there has likely emerged a world "market" for bilateral and regional trade policies/institutions to *facilitate* the bilateral exchange of products, owing largely to the gains from specialization and the welfare benefits of product diversity for final goods producers (i.e., product differentiation in intermediates) and consumers (i.e., product differentiation in final goods).

The vast bulk of EIAs are among countries: (1) that are close in distance and consequently share low bilateral transaction costs; (2) that are large in economic size and consequently benefit from greater specialization in production and variety in terms of consumption; (3) that differ in relative factor endowments, benefitting from the exchange of traditional comparative advantages. Our probit estimates of the determinants of EIAs confirmed this. Hence, the vast bulk of EIAs are among countries that trade extensively; that is, countries that have formed EIAs have *chosen well*.

Traditional *ex ante* estimates of the trade and economic welfare gains from EIAs have often suggested relatively modest economic benefits. Much anecdotal evidence from policy makers suggests that the anticipated economic gains are much larger that traditional CGE models have implied. However, sufficient time has now passed – and econometric and theoretical developments advanced – such that policy makers can now examine with more precision the *ex post* effects of EIAs on trade patterns. The evidence in this paper suggests that the trade effects of membership in the EEC/EC/EU have been *much larger* than those suggested by *ex ante* considerations *and* much larger than even earlier empirical estimates using cross-sectional gravity equations suggested, cf., Frankel (1997). The results here suggest that EEC/EC/EU membership over the past forty years (1960-2000) is of an economically significant magnitude even larger than that postulated a decade ago in Bayoumi and Eichengreen's excellent analysis of early EEC6 effects between 1957-1972.

Policy makers around the world have likely drawn lessons from the apparent success of the major economic integration agreement experiment of 1957, the Treaty of Rome. They have likely pursued the seeming trade enhancements for bilateral and regional EIAs. And the evidence in this paper suggests that their "economic expectations" have largely been correct. Our results suggest that *Other EIAs* that have formed over the 1960-2000 period have also yield "average treatment effects" of nearly the same magnitudes as the trade effects of the EEC/EC/EU members. Naturally, the deeper integration of the EU has likely boosted the trade effects of that particular agreement relative to most other agreements, which have been FTAs.

Our overall message is twofold. First, *ex post* empirical evidence suggests that policy makers are likely operating in a competitive environment, pursuing economic integration agreements in “natural cases” where the members already trade extensively (based upon bilateral, multilateral, and world levels of GDPs and trade costs). Second, after accounting for the pitfalls associated with the “endogeneity of EIAs’ determination,” the vast bulk of EIAs have tended to augment members’ trade by about *100 percent* over a 15-year period. This is consistent with anecdotal evidence from policy makers that the economic benefits from EIAs are much larger than conventional *ex ante* economic analyses have previously suggested.

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Table 3.1: Typical cross-section gravity equation coefficient estimates.

Variable	(1) 1960	(2) 1970	(3) 1980	(4) 1990	(5) 2000
ln GDP_i	0.76 (46.57)	0.89 (57.77)	1.01 (69.37)	1.09 (85.00)	1.19 (103.97)
ln GDP_j	0.76 (49.65)	0.92 (64.17)	1.01 (73.56)	0.97 (77.96	0.98 (87.36
ln $DIST_{ij}$	-0.65 (-16.81)	-0.84 (-20.95)	-1.06 (-27.65)	-1.07 (-28.68)	-1.20 (-33.00)
ADJ_{ij}	0.14 (0.93)	0.13 (0.78)	0.35 (2.24)	0.58 (3.65)	0.67 (6.90)
$LANG_{ij}$	0.05 (0.54)	0.27 (2.75)	0.55 (5.83)	0.79 (8.07)	0.65 (6.90)
EU_{ij}	0.67 (2.00)	0.48 (1.16)	-0.36 (-1.32)	-0.25 (-1.15)	-0.29 (-1.76)
$EFTA_{ij}$	0.56 (2.41)	1.04 (4.25)	0.32 (0.91	-0.19 (0.41)	-0.98 (-0.71)
EEA_{ij}			-0.07 (-0.31)	-0.15 (-0.71)	-0.11 (-0.29)
$OEIA_{ij}$	0.72 (1.77)	3.01 (0.38)	0.86 (1.81)	0.61 (1.42)	0.61 (5.05)
Constant	-10.17 (-21.63)	-14.36 (-30.74)	-17.16 (-37.62)	-18.34 (-43.34)	-19.72 (-51.56)
RMSE	1.4144	1.7548	1.8935	1.9919	1.9616
R^2	0.6035	0.6364	0.6453	0.6651	0.7147
Obs.	2789	4030	5494	6474	7302

t-statistics are in parentheses. The dependent variable is the (natural log of the) nominal bilateral trade flow from *i* to *j*.

Table 3.2: Theory-motivated cross-section gravity equations with country fixed effects

Variable	(1) 1960	(2) 1970	(3) 1980	(4) 1990	(5) 2000
ln GDP_i	0.76 (46.57)	0.89 (57.77)	1.01 (69.37)	1.09 (85.00)	1.19 (103.97)
ln GDP_j	0.76 (49.65)	0.92 (64.17)	1.01 (73.56)	0.97 (77.96)	0.98 (87.36)
ln $DIST_{ij}$	-0.65 (-16.81)	-0.84 (-20.95)	-1.06 (-27.65)	-1.07 (-28.68)	-1.20 (-33.00)
ADJ_{ij}	0.14 (0.93)	0.13 (0.78)	0.35 (2.24)	0.58 (3.65)	0.67 (6.90)
$LANG_{ij}$	0.05 (0.54)	0.27 (2.75)	0.55 (5.83)	0.79 (8.07)	0.65 (6.90)
EU_{ij}	0.67 (2.00)	0.48 (1.16)	-0.36 (-1.32)	-0.25 (-1.15)	-0.29 (-1.76)
$EFTA_{ij}$	0.56 (2.41)	1.04 (4.25)	0.32 (0.91)	-0.19 (0.41)	-0.98 (-0.71)
EEA_{ij}			-0.07 (-0.31)	-0.15 (-0.71)	-0.11 (-0.29)
$OEIA_{ij}$	0.72 (1.77)	3.01 (0.38)	0.86 (1.81)	0.61 (1.42)	0.61 (5.05)
Constant	-10.17 (-21.63)	-14.36 (-30.74)	-17.16 (-37.62)	-18.34 (-43.34)	-19.72 (-51.56)
RMSE	1.4144	1.7548	1.8935	1.9919	1.9616
R^2	0.6035	0.6364	0.6453	0.6651	0.7147
Obs.	2789	4030	5494	6474	7302

t-statistics are in parentheses. The dependent variable is the (natural log of the) nominal bilateral trade flow from country *i* to country *j* divided by the product of their nominal GDPs. Coefficient estimates of country fixed effects are not reported for brevity.

Table 3.3: Economic integration agreements

European Union, or EU (1958): Belgium–Luxembourg, France, Italy, Germany, Netherlands, Denmark (1973), Ireland (1973), United Kingdom (1973), Greece (1981), Portugal (1986), Spain (1986), Austria (1995), Finland (1995), Sweden (1995)
The Customs Union of West African States (1959): Burkina Faso, Mali, Mauritania, Niger, Senegal
European Free Trade Association, or EFTA (1960): Austria (until 1995), Denmark (until 1973), Finland (1986–1995), Norway, Portugal (until 1986), Sweden (until 1995), Switzerland, United Kingdom (until 1973)
Latin American Free Trade Agreement/Latin American Integration Agreement, or LAFTA/LAIA (1961–1979,1993–): Argentina, Bolivia, Brazil, Chile, Ecuador, Mexico, Paraguay, Peru, Uruguay, Venezuela (became inoperative during 1980–1990, but reinitiated in 1993)
African Common Market (1963): Algeria, Egypt, Ghana, Morocco
Central American Common Market (1961–1975, 1993–present): El Salvador, Guatemala, Honduras, Nicaragua, Costa Rica (1965)
Economic Customs Union of the Central African States (1966): Cameroon, Congo, Gabon
Carribean Community, or CARICOM (1968): Jamaica, Trinidad and Tobago, Guyana (1995)
EU–EFTA Agreement/European Economic Area (1973/1994)
Australia–New Zealand Closer Economic Relations (1983)
US–Israel (1985)
US–Canada (1989)
EFTA–Israel (1993)
Central Europe Free Trade Agreement, or CEFTA (1993): Hungary, Poland, Romania (1997), Bulgaria (1998)
EFTA–Bulgaria (1993)
EFTA–Hungary (1993)
EFTA–Poland (1993)
EFTA–Romania (1993)
EU–Hungary (1994)
EU–Poland (1994)
North American Free Trade Agreement, or NAFTA (1994): Canada, Mexico, United States
Bolivia–Mexico (1995)
Costa Rica–Mexico (1995)

EU–Bulgaria (1995)

EU–Romania (1995)

Group of Three (1995): Columbia, Mexico, Venezuela

Mercado Comun del Sur, or Mercosur (1991): Argentina, Brazil, Paraguay, Uruguay (formed in 1991 and a free trade area in 1995)

Andean Community (1993): Bolivia, Columbia, Ecuador, Peru, Venezuela, Peru (1997)

Mercosur–Chile (1996)

Mercosur–Bolivia (1996)

Canada–Chile (1997)

Canada–Israel (1997)

Association of Southeast Asian Nations, or ASEAN (1998): Indonesia, Philippines, Singapore, Thailand (effective on 80% of merchandise trade in 1998)

CARICOM–Dominican Republic (1998)

Hungary–Turkey (1998)

Hungary–Israel (1998)

India–Sri Lanka (1998)

Israel–Turkey (1998)

Mexico–Nicaragua (1998)

Romania–Turkey (1998)

Poland–Israel (1998)

Romania–Turkey (1998)

Mexico–Chile (1999)

Common Market for Eastern and Southern Africa (2000): Egypt, Kenya, Madagascar, Malawi, Mauritius, Sudan, Zimbabwe, Zambia

EU–Israel Agreement (2000)

EU–Mexico (2000)

Poland–Turkey (2000)

Mexico–Guatemala (2000)

Mexico–Honduras (2000)

Mexico–Israel (2000)

Mexico–El Salvador (2000)

New Zealand–Singapore (2000)

Countries listed in agreements only include those in our sample of 96 countries listed in the Data Appendix. Agreements are listed in chronological order of date of entry into force. Years in parentheses denote date of entry, except where noted otherwise.

Sources:

http://www.wto.org/english/tratop_e/region_esummary_e.xls.

http://europa.eu.int/comm/enlargement/pas/europe_agr.htm.

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Frankel, Jeffrey A. Regional trading blocs. Institute for International Economics (1997).

Lawrence, Robert Z. Regionalism, multilateralism, and deeper integration. The Brookings Institution (1996).

Table 3.4: Panel gravity equations in levels using various specifications

Variable	(1)	(2)	(3)	(4)	(5)
ln $RGDP_{it}$	.95 (217.57)	0.98 (231.55)	0.71 (34.52)	1.27 (47.29)	
ln $RGDP_{jt}$	.95 (225.07)	0.97 (236.17)	0.58 (26.53)	1.23 (41.72)	
ln $DIST_{ij}$	-1.04 (-78.42)	-1.02 (-78.34)			
ADJ_{ij}	0.38 (7.66)	0.34 (6.56)			
$LANG_{ij}$	.60 (18.06)	0.53 (16.25)			
EU_{ijt}	-0.25 (-7.16)	-0.11 (-2.77)	0.58 (7.57)	0.89 (11.58)	0.82 (10.65)
$EFTA_{ijt}$	0.33 (7.36)	-0.17 (-3.49)	0.55 (4.23)	0.45 (3.48)	0.50 (3.88)
EEA_{ijt}	-0.12 (-2.83)	-0.11 (-2.53)	0.34 (3.92)	0.57 (6.64)	0.53 (6.24)
$OEIA_{ijt}$	0.72 (10.24)	1.12 (16.07)	0.57 (8.86)	0.65 (10.25)	0.63 (9.92)
RMSE	1.9252	1.8567			
Overall R^2	0.6582	0.6821			
Within R^2			0.2038	0.2273	0.088
Obs.	47081	47081	47081	47081	47081

t-statistics are in parentheses. The dependent variable is the (natural log of the) real bilateral trade flow from country *i* to country *j*. Coefficient estimates of various fixed/time effects are not reported for brevity.

Table 3.5: Panel gravity equations with bilateral fixed and country-and-time effects

Variable	(1)	(2)	(3)	(4)
EU_{ijt}	0.65** (7.86)	0.45** (4.01)	0.47**(3.90)	0.50**(3.74)
EU_{ijt-1}		0.37** (3.13)	0.19 (1.36)	0.04 (0.29)
EU_{ijt-2}			0.24* (1.78)	0.26 (1.57)
EU_{ijt+1}				-0.08 (-0.63)
$EFTA_{ijt}$	-0.01 (-0.09)	-0.18 (-1.10)	-0.12 (-0.61)	0.04 (0.16)
$EFTA_{ijt-1}$		0.29* (1.83)	0.13 (0.60)	0.17 (0.74)
$EFTA_{ijt-2}$			0.07 (0.41)	-0.05 (-0.28)
$EFTA_{ijt+1}$				-0.22 (-1.02)
EEA_{ijt}	0.19* (2.11)	0.05 (0.48)	0.10 (0.85)	0.19 (1.61)
EEA_{ijt-1}		0.29** (2.85)	0.09 (0.76)	0.06 (0.47)
EEA_{ijt-2}			0.27** (2.51)	0.13 (1.00)
EEA_{ijt+1}				-0.24* (-1.66)
$OEIA_{ijt}$	0.46** (7.02)	0.31** (4.55)	0.29** (4.10)	0.39** (3.64)
$OEIA_{ijt-1}$		0.46** (4.77)	0.37** (3.52)	0.29* (1.79)
$OEIA_{ijt-2}$			0.17 (1.26)	0.11 (0.67)
$OEIA_{ijt+1}$				-0.04 (-0.58)
Constant	8.43 (279.58)	8.92 (346.63)	9.00 (263.34)	9.16 (282.92)
Within R^2	0.3106	0.3050	0.2759	0.2523
Obs.	47081	36563	34105	27575
Wald stat [EU]		83.19**	52.00**	17.20**
Wald stat [EFTA]		0.63	0.19	0.05
Wald stat [EEA]		11.80**	12.12**	0.47
Wald stat [OEIA}		65.88**	29.65**	14.37**

t-statistics are in parentheses. *(**) denotes statistical significance at 5 (1) percent level in one-tailed t-test.

The dependent variable is the (natural log of the) real bilateral trade flow. Coefficient estimates for bilateral fixed and country-and-time effects are not reported for brevity.

Table 3.6: Panel gravity equations with bilateral fixed and country-and-time effects with GDP restrictions

Variable	(1)	(2)	(3)	(4)
EU_{ijt}	0.65**(7.85)	0.45**(4.01)	0.46**(3.84)	0.50**(3.67)
EU_{ijt-1}		0.37**(3.12)	0.19 (1.39)	0.05 (0.31)
EU_{ijt-2}			0.24 (1.78)	0.26 (.157)
EU_{ijt+1}				-0.08 (-0.60)
$EFTA_{ijt}$	-0.01 (-0.11)	-0.18 (-1.08)	-0.12 (-0.60)	0.04 (0.18)
$EFTA_{ijt-1}$		0.29 (1.79)	0.13 (0.60)	0.05 (0.31)
$EFTA_{ijt-2}$			0.67 (0.38)	-0.06 (-0.32)
$EFTA_{ijt+1}$				-0.22 (-1.03)
EEA_{ijt}	0.19* (2.10)	0.05 (0.48)	0.08 (0.73)	0.17 (1.47)
EEA_{ijt-1}		0.29**(2.84)	0.10 (0.82)	0.07 (0.52)
EEA_{ijt-2}			0.27* (2.47)	0.13 (0.98)
EEA_{ijt+1}				-0.23 (-1.62)
$OEIA_{ijt}$	0.46**(7.01)	0.30** (4.55)	0.29**(4.08)	0.39**(3.61)
$OEIA_{ijt-1}$		0.46** (4.77)	0.37**(3.52)	0.29 (1.79)
$OEIA_{ijt-2}$			0.17 (1.26)	0.11 (0.67)
$OEIA_{ijt+1}$				-0.04 (-0.57)
Constant	-25.05** (-870.87)	-25.16** (-911.73)	-25.39** (-742.83)	-25.32** (-782.33)
Within R^2	0.1896	0.1824	0.1626	0.1575
Obs.	47081	36563	34105	27575
Wald stat [EU]		83.12**	51.55**	17.14**
Wald stat [EFTA]		0.61	0.18	0.06
Wald stat [EEA]		11.74**	11.51**	0.43
Wald stat [OEIA}		65.91**	29.60**	14.29**

t-statistics are in parentheses. *(**) denotes statistical significance at 5 (1) percent level in one-tailed t-test.

The dependent is the (natural log of the) real bilateral trade flow divided by the product of the real GDPs. Coefficient estimates for bilateral fixed and country-and-time effects are not reported for brevity.

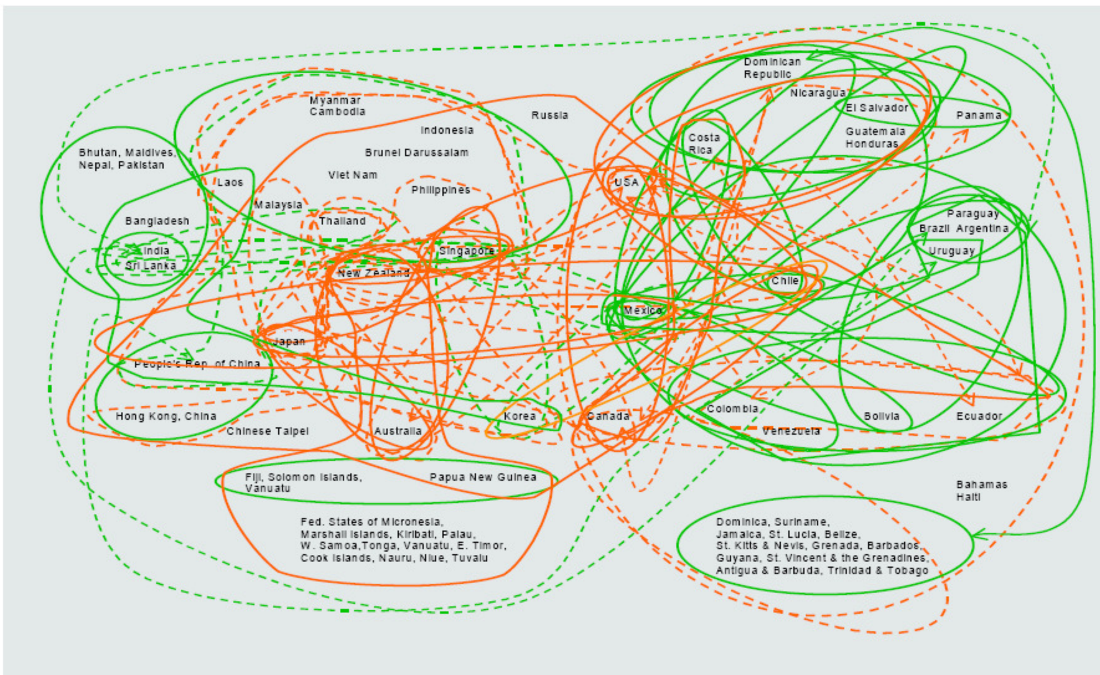
Table 3.7: First-differenced panel gravity equations with country-and-time effects

Variable	(1)	(2)	(3)	(4)
$EU_{ij,t-(t-1)}$	0.48** (8.91)	0.47** (8.63)	0.46** (8.54)	0.46** (8.16)
$EU_{ij(t-1)-(t-2)}$		0.23** (4.41)	0.19** (3.70)	0.04 (0.72)
$EU_{ij(t-2)-(t-3)}$			-0.11** (-2.82)	-0.07 (-1.17)
$EU_{ij(t+1)-t}$				0.06 (0.82)
$EFTA_{ij,t-(t-1)}$	0.08 (1.28)	0.02 (0.27)	0.01 (0.85)	0.03 (0.40)
$EFTA_{ij(t-1)-(t-2)}$		0.20** (3.09)	0.14* (2.06)	0.23** (2.74)
$EFTA_{ij(t-2)-(t-3)}$			0.02 (0.23)	-0.01 (-0.13)
$EFTA_{ij(t+1)-t}$				-0.25* (-2.25)
$EEA_{ij,t-(t-1)}$	0.19** (4.02)	0.17** (3.49)	0.16** (3.43)	0.15** (2.92)
$EEA_{ij,(t-1)-(t-2)}$		0.06 (1.40)	0.05 (1.08)	0.05 (1.00)
$EEA_{ij(t-2)-(t-3)}$			-0.02 (-0.40)	-0.01 (0.09)
$EEA_{ij(t+1)-t}$				-0.20** (-2.59)
$OEIA_{ij,t-(t-1)}$	0.31** (6.66)	0.30** (6.30)	0.28** (6.04)	0.27** (4.55)
$OEIA_{ij(t-1)-(t-2)}$		0.29** (4.57)	0.25** (3.79)	0.30 (1.72)
$OEIA_{ij(t-2)-(t-3)}$			0.05 (0.29)	0.04 (0.21)
$OEIA_{ij(t+1)-t}$				-0.06 (0.91)
Constant	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
R^2	0.0009	0.0011	0.0011	0.0010
Obs.	36563	34105	31172	24642

t-statistics are in parentheses. The dependent variable is the (natural log of the) real bilateral trade flow from country *i* to country *j*.*(**) denotes statistical significance at 5 (1) percent level in one-tailed *t*-test. Coefficient estimates for bilateral fixed and country-and-time effects are not reported for brevity.

Figure 3.1: Mapping of bilateral free trade agreements (the "spaghetti bowl")

The "spaghetti bowl" of FTAs in the Americas and Asia-Pacific (2005)



Intra-regional **Inter-regional** **Concluded** **Under Negotiation**

***Source*: Integration and Regional Programs Department, IDB**

CHAPTER FOUR

TRADE FLOW CONSEQUENCES OF THE EUROPEAN UNION'S REGIONALIZATION OF ENVIRONMENTAL REGULATIONS

Introduction

Groups of countries sometimes impose regulations on themselves. The nature of these regulations ranges from military policy, such as nuclear proliferation restrictions, to trade policy, such as limitations on tariffs due to World Trade Organization membership. One specific form of group regulation is regional environmental regulation: a group of countries in a region imposes environmental regulations on all members of the group. Regional environmental regulation has occurred inside common markets, such as the European Union, and other economic integration agreements (EIAs), such as the North American Agreement on Environmental Cooperation treaty that was designed to accompany the North American Free Trade Agreement (NAFTA)[24,25]. In this paper, I show that the consequences of increasing environmental regulation stringency differs across low income and high income member countries of the European Union. I model and demonstrate empirically the possibility for high income members of a region to benefit from environmental regulations imposed on the entire group as a protectionist measure – that is, as a means of deterring industry from relocating to the lower income

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[24] The EU has passed, beginning in 1980, a series of specific directives with stated limits on, for example, sulfur dioxide concentrations in ambient air.

[25] The North American Agreement on Environmental Cooperation does not create new environmental standards or limits on pollutions. Rather, it is designed to encourage enforcement of existing environmental standards within NAFTA member countries.

countries to take advantage of the lower production costs offered there and of simultaneously increasing sales of exports of domestic producers.

This paper exploits survey data from the World Economic Forum in which business executives rate environmental regulation stringency in various countries. Using these data in a gravity equation context, I test the effect of an increase in environmental regulation stringency on bilateral trade flows from all countries (in the dataset) to all countries, from high income countries to high income countries, from high income countries to low income countries, from low income countries to high income countries, from low income countries to low income countries, and from low income countries to all countries. I control for whether an increase in environmental regulation stringency occurred within a European Union member country, allowing estimation of the effects of environmental regulation inside and outside a region.

Background

Many economists have investigated the relation between international trade flows and environmental regulations. Some research on this subject has tested whether a country can increase its ability to export by reducing the stringency of environmental regulations and therefore lowering the costs of production for exporters (Ederington and Minier, 2003; Levinson and Taylor, 2004; Ederington et al., 2005). Also, the pollution haven hypothesis (PHH) states that "dirty" industries will relocate to those countries that lower their environmental standards, further increasing those countries' exports (Mani and Wheeler, 1999; Levinson and Taylor, 2004). The combination of lowered costs for domestic exporters and the relocation of dirty industries from countries with stringent environmental regulations to pollution havens theoretically leads to predictions of increased exports when a country lowers its environmental regulation stringency. Empirically, however, the effects of changes in environmental regulation stringency have not been clear. Those studies that have found support for the PHH have generally been limited to studies of the United States and some of its trade partners or studies of only European countries.

For over forty years, international trade economists have empirically tested the effects of changes in determinants of trade patterns by using the gravity equation, explained further in Section 4 of this paper. Until recently, most gravity equation estimates had not found empirical evidence to support that a decrease in environmental regulation stringency leads to an increase in exports (Harris et al., 2002). Furthermore, early gravity equation estimates of the effect of environmental regulations on trade flows rely on proxies for environmental regulation stringency that likely introduced endogeneity to the estimates (Jug and Mirza, 2005). In Appendix A, I explicitly show how environmental outcome variables introduce endogeneity into gravity equation estimates of the effects of environmental regulation stringency on trade flows. In addition, I introduce a new proxy for environmental regulation stringency – survey data – and show that it might not introduce endogeneity in Appendix B.

More recent gravity equation estimates that appropriately accounted for unobservable country characteristics that could affect both the choice of environmental regulation stringency and the level of economic activity has found statistically significant, positive effect of lowering environmental regulation stringency on exports (Jug and

Mirza, 2005). Jug and Mirza run instrumental variables estimations of gravity equation estimates of the effect of environmental regulation compliance expenditure and obtain results that are similar to a non-gravity equation study (Ederington and Minier, 2003) that had been conducted using United States data. Both studies' results obtain a significant positive effect on exports when environmental regulation stringency is relaxed. I improve on these studies in multiple ways. The first is by using a proxy for environmental regulation stringency – survey data from the World Economic Forum – that is less likely to introduce endogeneity. Using this proxy also allows me to include many more non-European and low income countries in my dataset than most previous studies. The second is by using gravity equation estimation techniques developed by Baier and Bergstrand that allow the inclusion of an economic integration agreement variable in the gravity equation without biasing estimates (Baier and Bergstrand, 2004). The third is by controlling for the possible interaction between European Union membership and environmental regulations. Regulations imposed by the EU on the entire group might have different effects than unilaterally generated regulations. Finally, I estimate the effects of changes in environmental regulation stringency on trade flows for countries of different income levels, because the effects may drastically differ for high income countries and low income countries.

Unilateral versus Regional Environmental Regulations

When an increase in environmental regulation stringency occurs unilaterally due to changes within the country (e.g. pressure from constituents for a cleaner environment), the effect on exports from that country to other countries could be positive or negative.

Technology spillovers, other countries' taste for "green" goods, establishment and protection of property rights, and signaling of governmental stability could all contribute to a positive effect on exports from a country due to a unilateral increase in environmental regulations in the low income country. Porter and Van der Linde (1995) argue that stringent environmental regulations can benefit a country not only through improved environmental quality but also through the development of comparative advantages in highly-regulated industries.

Conversely, the increased cost of production due to the increase in regulations could contribute to a negative effect on exports because of the resultant higher price of domestically produced goods relative to foreign goods. This could be exacerbated if some "dirty" industries choose to relocate because of the increased cost of production. The net effect of a unilateral increase in environmental regulation stringency therefore seems to be an empirical question.

When an increase in environmental regulation stringency occurs due to changes beyond an individual country's control (e.g. the European Union imposes environmental standards on all members), it is possible that any possible positive effect on exports from that country due to the change would be diminished while the negative effect would be simultaneously magnified. Any positive effect resulting from establishment and protection of property rights and signaling of governmental stability might disappear because the regulations are not self-imposed; externally generated regulations do not necessarily signal stability or protection of property rights: people do not believe that a power-hungry dictator has truly eschewed the development of nuclear weapons when threats of UN sanctions and even war have forced the dictator to stop nuclear weapon development in his country. The cost of production might increase even more than in the case of self-imposed regulations if generalized environmental standards applied across a group of countries ignore differences in individual country characteristics, such as variance in the sulfur content of coal and oil across countries; these characteristics are less likely to be ignored by policymakers in each individual country, and the lowest-cost type of regulation (that achieves the same outcome standard) could be chosen on a tailored basis in the case of a unilateral environmental regulation increase (Oates and Schwab, 1996).

One largely unexplored area in the empirical international trade literature is the interaction of economic integration agreements (EIAs), such as the European Union and NAFTA, and environmental regulations. I show, in a model in Section 3 and empirically in section 5, that the (possibly unintended) consequences of regional environmental regulations that could differ across income levels of countries. Low income countries in an EIA could be more adversely affected by an increase in production costs caused by environmental regulations than high income countries for two possible reasons. The first I term the uneven competitiveness effect, and it is a reframing of the Alchian-Allen hypothesis (Alchian and Allen, 1964). The second reason I term the uneven burden of compliance: because high income countries are more likely than low income countries to have relatively stringent environmental regulations in place prior to the creation of regional environmental regulations, the cost of compliance with a given regional environmental regulation might be lower for high income countries than for low income countries. The remainder of section 2 briefly explains these two effects.

Uneven Competitiveness Effect

The Alchian-Allen hypothesis is that the presence of a per unit transport cost lowers the relative price of high quality goods compared to low quality goods. For example, transportation costs cause firms to export high quality apples while keeping low quality apples for domestic consumption, a phenomenon that Alchian and Allen refer to as "shipping the good apples out." I reframe the Alchian-Allen hypothesis to examine an increase in production cost due to an increase in environmental regulation stringency. This is explained briefly here and shown more explicitly in a model in Section 3.

If production costs in all countries in a region increase by some constant *k* as a result of regional environmental regulations, the percent increase in price will be higher for countries that produce low priced goods than countries that produce high priced goods. If there are other producers outside the region whose costs are not increased by *k*, then the impact on each country's competitiveness (relative to the rest of the world) caused by the increase in price falls more heavily on the low income countries inside the group than the high income countries.[26] In other words, there is an uneven effect on country competitiveness across income groups.

Uneven Burden of Compliance

[26] I employ the term, "competitiveness," to mean a country's ability to export goods – an increase in price of a country's goods, due to an increase in production costs, means that the country cannot export as many goods because of substitution and income effects on the parts of foreign consumers.

The second reason that low income countries could be more adversely affected than high income countries due to an increase in regional environmental regulation stringency is that the costs of compliance with the regulation may not be equally distributed among all countries. High income countries typically have more stringent environmental regulations in place than low income countries prior to the passage of any regional environmental regulations[27]. Compliance with regional environmental regulations would be less costly in those high income countries than in low income countries, if all countries have to meet some constant standard of compliance. Thus, the increase in production costs would be higher in low income countries than in high income countries: the uneven burden of compliance. The uneven burden of compliance is modeled in Section 3.

Model

Consumption

Each of *N* different countries produces a single product, whose exogenous quality is differentiated from the products of other countries.[28] The representative consumer in country *j* maximizes his CES (Constant Elasticity of Substitution) utility function:

$$U_j(x) = \left[\sum_{i=1}^{N} \mu_i x_{ij}^{\rho}\right]^{\frac{1}{\rho}}$$

Subject to a budget constraint:

[27] As evidence that high income countries typically have more stringent environmental regulations than low income countries, note that the mean rating of the environmental regulation stringency of the high income countries in the World Economic Forum's Global Competitiveness Report for years 2000 – 2005 is 5.77 on a scale of 1 to 7 where 7 is "very stringent" and 1 is "very lax", while that of low income countries over the same period is 3.46.

[28] Instead of a single product, it could be that each country produces a variety of products. This variety could even be endogenized, following Dixit-Stiglitz, but that complication seems unnecessary here.

$$M_j \geq \sum_{i=1}^{N} p_{ij} x_{ij}$$

Where M_j is country j's income (real GDP), p_{ij} is the price of country i's good when it is sold in country j, x_{ij} is the quantity of good produced in country i that gets consumed in country j, μ_i is the quality of country i's good, and ρ ($0 < \rho < 1$) is a preference parameter capturing the substitutability between goods: as ρ approaches 1, the goods are nearly perfect substitutes, and as ρ approaches 0, the goods are more complimentary. The FOC of this constrained optimization's LaGrangian is given by:

$$\frac{1}{\rho}\left[\sum_{i=1}^{N} \mu_i x_{ij}^{\rho}\right]^{\frac{1}{\rho}-1} \rho \mu_i x_{ij}^{\rho} = \lambda p_{ij}$$

Dividing the FOC for good i by that of good 1 yields:

$$\left(\frac{x_{ij}}{x_{1j}}\right)^{\rho-1} = \left(\frac{p_{ij} / \mu_i}{p_{1j} / \mu_1}\right) \tag{3.1}$$

Solving for x_{ij}:

$$x_{ij} = \left(\frac{p_{ij} / \mu_i}{p_{1j} / \mu_1}\right)^{\frac{1}{\rho-1}} x_{1j}$$

Let σ denote the elasticity of substitution, i.e. $\sigma = 1 / (1 - \rho)$ and $1 < \sigma < \infty$:

$$x_{ij} = \left(\frac{p_{ij} / \mu_i}{p_{1j} / \mu_1}\right)^{-\sigma} x_{1j}$$

Multiplying both sides by p_{ij} and summing over i to produce country j's income on the LHS, we find:

$$\sum_{i=1}^{N} p_{ij} x_{ij} = M_j = \left(\frac{p_{1j}}{\mu_1}\right)^{\sigma} x_{1j} \sum_{i=1}^{N} \mu_i^{\sigma} p_{ij}^{1-\sigma}$$

Solving this expression for x_{ij} yields:

$$x_{ij} = \frac{\mu_i^{\sigma} p_{ij}^{-\sigma}}{\sum_{i=1}^{N} \mu_i^{\sigma} p_{ij}^{1-\sigma}} M_j \tag{3.2}$$

The denominator of this demand is a quality-adjusted price index for country j, which I will refer to as I_j. This Marshallian Demand immediately implies the total expenditure of those in country j on the goods from country i is given by:

$$p_{ij} x_{ij} = \frac{\mu_i^{\sigma} p_{ij}^{1-\sigma}}{I_j} M_j$$

Because of transport costs and tariffs, the price of an imported good is more expensive than the same good in its home country. I model this accordingly:

$$p_{ij} = p_i D_{ij}^{\delta} e^{-\psi 1\{EIA\}}$$

Where p_i is the price of the good in its home country, D_{ij} is the distance between country i and country j, and 1{EIA} equals 1 iff i and j are members of an EIA (Economic Integration Agreement). I assume that a good's quality is increasing in the GDP of the country where it is produced:

$$\mu_i = \kappa M_i^{\alpha}$$

Substituting these two expressions into the expenditure shares produces the gravity equation, where κ and α are simply parameters:[29]

$$p_{ij}x_{ij} = \frac{\kappa^{\sigma}}{I_j}\left[\frac{M_i^{\alpha\sigma} M_j}{D_{ij}^{\delta(\sigma-1)}}\right] e^{(\sigma-1)\psi 1\{FTA\}} \tag{3.3}$$

Production

The representative producer in each country is a monopolistically competitive firm with a constant marginal cost that varies across countries, c_i. I assume c_i is increasing in μ. The producer's objective is to maximize profits:

$$\pi_i = (p_i - c_i)\sum_{j=1}^{N} x_{ij}(p) = (p_i - c_i)\sum_{j=1}^{N} \frac{\mu_i^{\sigma} p_{ij}^{-\sigma}}{I} M_j$$

I assume that the country is a price index taker. The FOC governs the country's optimal pricing policy:

$$\sum_{j=1}^{N}(1-\sigma)x_{ij} - \sum_{j=1}^{N}(-\sigma)c_i \frac{x_{ij}}{p_i} = 0$$

Making the optimal price a simple mark-up over marginal cost:

$$p_i = \left(\frac{\sigma}{\sigma - 1}\right)c_i \tag{3.4}$$

This yields a simple expression for the country's income:

$$M_i = \pi_i = \frac{c_i}{\sigma - 1}\sum_{j=1}^{N} x_{ij} \tag{3.5}$$

Effects of Environmental Regulatory Compliance

[29] By allowing these parameters to vary depending on whether we are considering trade between rich and poor countries or rich to rich, this model becomes more flexible and implicitly makes these parameters a function of what determines rich and poor.

I investigate the possible effects of changes in environmental regulation stringency by examining comparative statics in a partial equilibrium – one without income effects – and then discuss the potential role of those income effects.

I model environmental regulation as an exogenous change that benefits the representative consumer's utility at the expense of higher marginal cost in production. The benefits are assumed to be accrued in a linearly separable portion of the utility function, which implies that only the costs (and not the benefits) alter the behavior of agents in our existing model.

Substituting (3.4) into (3.1), we reach a reduced form Marginal Rate of Substitution (MRS) for consumers in country *j* considering imports from country *i* and country *k*. To examine the substitution effect of environmental regulations, consider the reduced MRS both before and after an increase in environmental regulation stringency (t=0 and t=1, respectively):

$$\left(\frac{x_{ij}^t}{x_{kj}^t}\right)^{\rho-1} = \frac{(c_i + tr_i)\sigma / \mu_i(1-\sigma)}{(c_k + tr_k)\sigma / \mu_k(1-\sigma)}$$

Where *r* is the increase in marginal cost due to regulation, *t* is both a superscript and dummy variable indicating pre- and post-regulation periods, and two different countries selling goods in country *j* are indexed by *i* and *k*. I compare the pre- and post-regulation MRS to find the condition under which the MRS has decreased as a result of the environmental regulations:

$$\frac{c_i\sigma / \mu_i(1-\sigma)}{c_k\sigma / \mu_k(1-\sigma)} > \frac{(c_i + r_i)\sigma / \mu_i(1-\sigma)}{(c_k + r_k)\sigma / \mu_k(1-\sigma)}$$

Performing some basic algebra yields

$$c_i r_k > c_k r_i \tag{3.6}$$

which holds when $c_i > c_k$ and $r_k \geq r_i$. When the marginal cost of production is higher in country *i* than in country *k*, or when the cost of compliance is greater in country *k* than in country *i*, the effect of an increase in regional environmental regulation stringency is to decrease the MRS. The aforementioned Alchian-Allen hypothesis is a special case of this condition, where the costs of compliance are equal for both countries: $r_k = r_i$.

There is good reason to suspect that this condition holds for the EU. High income member nations typically produce more expensive (and higher quality) products than low income member nations and most nations seeking to join (e.g., financial services produced in London versus textiles in Turkey, an EU candidate state). Likewise, the high income member nations on average have stricter environmental regulations than low income member nations and most nations seeking to join. Consequentially, we would expect that regulatory cost of low income members or candidate members would be greater than high income member nations. If this condition does indeed hold, then:

$$\left(\frac{x_{kj}^0}{x_{ij}^0}\right)^{1-\rho} > \left(\frac{x_{kj}^1}{x_{ij}^1}\right)^{1-\rho} \tag{3.7}$$

Hence, *ex post* exports from country *k* to country *j* are smaller than *ex ante*, relative to the exports of country *i*. The partial equilibrium effect of the regulation is to cause consumers to substitute away from less costly goods to more expensive goods because the costs of the regulation somewhat equilibrates the marginal costs of those goods.

The partial equilibrium results indicate that richer countries grab a larger market share when environmental regulations are increased. However, this can be (somewhat) counteracted by a general equilibrium effect: the size of the overall market is decreased by the income effect of the environmental regulation. In contrast, expanding an EIA lowers tariffs, producing a wealth effect in the opposite direction. Hence, if an increase in environmental regulations is accompanied by a sufficient expansion in EIAs, then the market can grow and rich countries can increase their market share. Thus, the presumed exogeneity of environmental regulations is drawn into question because the unintended consequences of that regulation may disproportionally benefit particular agents.

Following Maloney and McCormick (1982), we could model firms in country *i* lobbying for environmental regulations because their profits vary with environmental regulation. If regulations were determined by a vote of industry representatives, then the median-cost country could effectively choose its first-best alternative. The situation is more interesting when environmental regulations, once passed, are irreversible (i.e. environmental regulations can only be tightened, not slackened). In this case, existing EU members could extract (nearly) all of the gains from integration simply by increasing environmental regulations up until a participation constraint for countries seeking membership. This particular idea is left for future research.

Econometric Issues with the Gravity Equation

The literature on the effects of environmental regulations on trade flows has suffered from the lack of a standard measure of environmental regulation stringency. Previous gravity equation estimates of the effect of environmental regulation stringency on trade flows have relied on outcome measures, such as energy intensity, carbon emissions, and sulfur emissions; as these studies admit, endogeneity is an issue when using these outcome variables as proxies for environmental regulation stringency. I explicitly show the potential endogeneity of such an outcome variable in Appendix A.

Instead of an outcome variable, I use the results of the World Economic Forum's Global Competitiveness Report survey, which asks thousands of executives from around the world to rate each country's environmental regulation stringency (Porter et al., 2001-2006). This survey asks executives to rate overall environmental regulation stringency in each country, compared to all other countries. The rating scale is from one to seven, where one is "lax compared with most other countries" and seven is "among the world's most stringent." I show in Appendix B that endogeneity is possibly avoided by using survey data as a proxy for environmental regulation stringency.

As the topic of regional environmental regulation necessarily requires a regional agreement that imposes the regulation on a group of countries, I first discuss the pitfalls of including an economic integration agreement (EIA) variable in the gravity equation. Specifically, I address the endogeneity inherent in the selection into EIAs and how other authors have dealt with that problem.

Endogeneity in the gravity equation

A typical gravity equation that includes a variable for economic integration agreements is

$$PX_{ijt} = \beta_0 Y_{it}^{\beta_1} Y_{jt}^{\beta_2} D_{ij}^{\beta_3} e^{\beta_4 L_{ij}} e^{\beta_5 A_{ij}} e^{\beta_6 E_{ijt}} \varepsilon_{ijt} \tag{4.1}$$

where PX_{ij} is the value of the merchandise trade flow from exporter *i* to importer *j*, Y_i is the level of gross domestic product in country *i*, D_{ij} is the great circle distance between the economic centers of countries *i* and *j,* L_{ij} is a dummy variable equal to 1 if countries *i* and *j* share a common language, equal to 0 otherwise, A_{ij} is a dummy variable for adjacency that is equal to 1 if countries *i* and *j* share a common land border, equal to 0 otherwise, E_{ijt} is a dummy variable equal to 1 if countries *i* and *j* are both in economic integration agreement (EIA), and ε_{ijt} is assumed to be a log-normally distributed error term. In log form, equation (4.1) becomes

$$\ln PX_{ijt} = \ln\beta_0 + \beta_1 \ln Y_{it} + \beta_2 \ln Y_{jt} + \beta_3 \ln D_{ij} + \beta_4 L_{ij} + \beta_5 A_{ij} + \beta_6 E_{ijt} + \ln\varepsilon_{ijt} \tag{4.2}$$

Early versions of the gravity equation applied to international trade flows did not have formal theoretical foundations (for examples, see Tinbergen (1962), Linnemann (1966), Aitken (1973) and Sapir (1981)); instead, these earlier studies relied either on informal economic foundations or to a physical science analogy. Since 1979, however, formal theoretical economic foundations for a gravity equation similar to equation 1 have surfaced, such as Anderson (1979), Bergstrand (1985), Deardorff (1998), Baier and Bergstrand (2001), Eaton and Kortum (2002), and Anderson and van Wincoop (2003). All of these models include an explicit role for prices across countries in order to generate unbiased estimates. Anderson and van Wincoop specifically include multilateral (price) resistance terms for each country in their system of equations, and solve their system using a custom nonlinear least squares program. Anderson and Van Wincoop (2003) and Feenstra (2004, Ch. 5) both suggest using country-specific fixed effects as an alternative method for accounting for multilateral price terms that will also generate unbiased coefficient estimates.

Extending this literature are Baier and Bergstrand (2004) and Baier and Bergstrand (2007). Baier and Bergstrand (2004) shows that gravity equation estimates of the trade flow effects of free trade agreements that include bilateral pair fixed effects are both plausible and consistent across various econometric specifications. Baier and Bergstrand (2007) contains a formal demonstration that bilateral pair fixed effects and time dummies specifications of the gravity equation yield the same results as the method of generating custom nonlinear least squares programs employed by Anderson and van Wincoop.

Endogeneity in the Economic Integration Agreement variable

An endogeniety bias arises when RHS variables are correlated with the error term. In equation (4.2), the economic integration agreement (EIA) variable, E_{ijt}, could potentially be correlated with the error term, rendering estimates of the effect of EIAs therefore biased; empirically and theoretically, the determinants of whether a bilateral pair chooses to join an EIA tend to be the same factors that explain large trade flows: size and similarities of countries' GDPs, distance between the two countries and distance to the rest of the world, whether they share a common language, and differences in relative factor endowments with respect to each other and the rest of the world (Baier and Bergstrand, 2004). The error term could capture unobservable policy-related barriers, such as intra-country shipping regulations, in one or both countries that affect trade between the two and are not accounted for in a typical gravity model. Joining an EIA might entail not just the liberalization of tariffs barriers and other border costs but also that of internal, unobservable non-tariff barriers. Furthermore, it seems likely that country pairs that have already harmonized many non-tariff barriers could easily choose to adopt an EIA because the costs of implementing it are relatively low. Failure to econometrically account for this would introduce an underestimation of the effect of joining an EIA due to a selection bias (Baier and Bergstrand, 2006). In the case of the European Union, a specific EIA where economic integration of monetary policies, antitrust policies, environmental regulations, and securities regulations is a stated goal, the liberalization of non-tariff barriers clearly poses an important potential welfare gain for EU members.

This paper appears to be the first to include bilateral-pair fixed effects in gravity equation estimates of the effects of environmental regulation stringency on trade flows. Baier and Bergstrand (2007) showed that OLS with bilateral-pair fixed effect terms can correct the omitted variable bias on the EIA variable. Yet, to date, no authors of gravity equation-type estimates of the effects of environmental regulation stringency on trade flows have dealt with the potential endogeneity arising from the inclusion of an EIA variable in the gravity equation. Further, following Egger (2000), time dummies are included as well. Thus, equation 2 with bilateral pair fixed effects (δ_{ij}) and time dummies (λ_t) included becomes

$$\ln X_{ijt} = \ln \beta_0 + \beta_1 \ln Y_{it} + \beta_2 \ln Y_{jt} + \beta_3 E_{ijt} + \delta_{ij} + \lambda_t + \ln \varepsilon_{ijt} \qquad (4.3)$$

Distance, language, and adjacency have all been dropped from equation (4.3) because they are time-invariant and therefore completely captured by the bilateral pair fixed effect term.

The inclusion of bilateral fixed effects in the analysis of the effect of the European Union (and its predecessors, the European Economic Community and European Community) yields striking results when compared to the typical gravity equation estimates that do not include bilateral pair fixed effects. While most estimates of the effect of the European Union on trade flows found little evidence that membership in the EU by two countries increased bilateral trade flow between them (for example, see Frankel [1997] or Sapir [1981]), more recent studies that have included bilateral pair fixed effects have found dramatic increases in bilateral trade flows due to European Union membership of both trading partners (Baier, Bergstrand, Egger, and McLaughlin, 2007).

Avoiding endogeneity with survey data

To avoid endogeneity, I use survey data rating environmental regulation stringency. In this survey, thousands of business executives are asked to rate countries' environmental regulation stringency levels, relative to all other countries, on a scale of one to seven, where one indicates that a country has lax standards compared to others and seven indicates that a country has very strict standards compared to others. I use the mean response of the executives' ratings of each country each year as a proxy for environmental regulation stringency. The model of an ordinal signal on a latent variable presented in Appendix B shows that utilization of this survey variable might avoid the endogeneity issue that outcome variables introduce.

Interaction of European Union membership and regulations

In addition to using a new proxy to test the effects of environmental regulation stringency on trade flows, I also test whether there is any interaction between membership in the European Union and environmental regulations affecting trade flows. Previous studies have sometimes controlled for economic integration agreements (EIAs) (see Harris, Konya, and Matyas (2002)), while others ignore EIAs altogether when analyzing environmental regulation stringency effects on trade flows; no gravity-type estimate has investigated whether EIAs interact with country-level environmental regulations. The European Union (EU) has had specific environmental regulations that apply to all members in force since at least 1980 (for an example, see Council Directive 80/779/EEC of July 15, 1980, on air quality limit values and guide values for sulfur dioxide and suspended particulates). These EU-level regulations are interpreted and acted upon by country-level environmental regulation agencies; hence, an interaction effect should not be ignored.

Data

I examine a panel of 56 countries from 2000 to 2005, listed in Table 1. The 56 countries included in the dataset were the only 56 countries for which survey data exists in all years. Data on membership in the EU were taken from the EU's website and are detailed in Table 2. For the purposes of this paper, I have grouped all countries into either "High Income" or "Low Income." Countries grouped into "High Income" had real per capita GDPs of $10,000 or more in the year 2000 according to IMF data[30]. These groupings are shown in Table 3.

Nominal exports data come from the IMF's Direction of Trade Statistics CD-ROM. These were converted to real exports using country specific CPIs, base year 2000, taken from the IMF. Observations of no recorded trade between two countries for a given year or recorded trade of zero were replaced with trade of $1 to avoid losing observations in the regressions when taking their natural logs. Current GDP, denominated in US dollars, was converted to real GDP using those same CPIs. GDP data also came from the IMF.

Ratings of environmental regulation stringency come from the World Economic Forum's annual World Competitiveness Survey. Only those countries that were rated in all years 2000 – 2005 in the survey were included in this dataset; hence, the dataset is a balanced panel. Summary statistics of all variables as well as definitions are provided in Table 4.

Results

The export flows are analyzed in six different patterns: all countries to all countries, high income countries to high income countries, high income countries to low income countries, low income countries to high income countries, low income countries to low income countries, and low income to all countries. The econometric specification of equation 4.3 is

[30] The choice of $10,000 as the threshold is justified by examining a scatterplot and a kernel density plot of real per capita GDP. If income is bimodal, Figure 1 shows the threshold between the two is at or near $10,000.

$$\ln rxp_{ijt} = \ln \beta_0 + \beta_1 \ln RGDP_{it} + \beta_2 \ln RGDP_{jt} + \beta_3 EU_{ijt} + \beta_4 ENVREGS_{it} + \beta_5 ENVREGS_{jt} + \beta_6 ENVREGS_{it} * EU_{ijt} + \delta_{ij} + \lambda_t + \ln \varepsilon_{ijt} \tag{6.1}$$

where

rxp_{ijt}= value of real exports from country *i* to country *j* in year *t*

$RGDP_{it}$= real GDP in country *i* in year *t*

EU_{ijt}=dummy indicating whether countries *i* and *j* were EU members in year *t*

$ENVREGS_{it}$=environmental regulation stringency in country *i* in year *t*

$ENVREGS_{it}*EU_{ijt}$=interaction term

δ_{ij}=bilateral pair fixed effect term

λ_t=time dummy for year *t*

The primary hypothesis tested is that exports from a low income country will be more negatively affected by an increase in environmental regulation stringency if that country is a member of the EU than if that country were not an EU member. This hypothesis will be supported if coefficient on the *ENVREG* EU* interaction term is negative and significant in the low income to all countries regression. A secondary hypothesis being tested is that a high income country experiences a greater increase in its exports due to an increase in environmental regulation stringency if it is in the EU; this is consistent with the idea that EU-wide regulations affect low income EU members' competitiveness more, relative to high income EU members' competitiveness.

$ENVREGS_{it}$ ranges from a possible minimum of 1 to a possible maximum of 7, where 7 indicates that country *i* has very stringent environmental regulations, compared to other countries, and 1 indicates that country *i* has very lax environmental regulations, compared to other countries. Thus, a positive coefficient on $ENVREGS_{it}$ would indicate that a unilateral increase in environmental regulation stringency in country *i* results in an overall increase in exports from that country its trading partners, ceteris paribus. This could result from technology spillovers, consumer demand for "green" goods in trading partner countries, or signaling of regime stability and property right development. It could also indicate that $ENVREGS_{it}$ proxies for some other factor that affects trade. A negative coefficient on $ENVREGS_{it}$ would indicate that exports from country *i* decrease as a result of an increase in environmental regulation stringency in country *i*, indicating that the increased production costs made firms in country *i* less competitive.

The interaction term, $ENVREGS_{it}*EU_{ijt}$, estimates the effect of an increase in environmental regulation stringency of the exporter, *i*, given that country *i* is in the EU. Its coefficient, β_6, when added to the coefficient on $ENVREGS_{it}$, β_4, estimates the net effect on exports of an increase in environmental regulation stringency in European Union member countries.

Table 5 presents the results of gravity equation estimates of equation (6.1). Each column corresponds to one of the groupings of bilateral pairs: column 1 shows estimates for all bilateral pairs (all-all), column 2 shows estimates for high income countries exporting to high income countries (high-high), column 3 shows high income to low income country pairs (high-low), column 4 shows low income to high income country pairs (low-high), column 5 shows low income to low income country pairs (low-low), and column 6 shows pairs of low income countries exporting to both high and low income countries (low-all).

In Table 5, there are significant differences across groupings in the estimates of the effects of an increase in environmental regulation stringency. Overall, it appears that an increase in environmental regulation stringency leads to an increase in exports, as the positive and significant estimates of *exp_envregs* in Table 5 indicate. This might indicate that the effects on exports of technology spillovers, high income countries' consumer taste for "green" goods, signaling of the establishment and protection of property rights, signaling of governmental stability, or developing comparative advantages in regulated industries more than offset the negative effect resulting from an increase in production costs due to more stringent regulations. It is also possible that these positive coefficient estimates result from some omitted variable, such as an interaction effect with other, unspecified EIAs. It is particularly odd that an increase in environmental regulation stringency leads to a statistically significant increase in exports from high income countries to high income countries (Column 2). One explanation could be that increases in overall environmental regulation stringency sometimes are attached to subsidies or governmental aid to exporting industries, particularly those that would be most harmed by the change.

The coefficient estimates for the interaction term, *exp_EU_envregs*, should be added to the estimates on *exp_envregs* in Table 5 for estimates of the effect on exports of an increase in environmental regulation stringency for EU members. The joint effects of the two estimates are tested for significance with Wald tests. These joint effects and the results of the tests are reported in Table 6.

The results reported in Table 6 elucidate that EU membership changes the effect of an increase in environmental regulation stringency. The results are consistent with the hypothesis that increases in environmental regulation stringency have different consequences for low income, EU members' competitiveness than for low income, non-EU members. The effect of an increase in environmental regulation stringency on exports from low income, EU members to every other grouping is negative and significant in Table 6. Specifically, a one point increase in environmental regulation stringency rating causes exports from low income, EU members to all high income countries to decrease by 28.2%, exports from low income, EU members to all low income countries to decrease by 42.9%, and exports from low income, EU members to all countries to decrease by 36.8%. Conversely, from Table 5, a one point increase in environmental regulation stringency ratings in a low income, non-EU member country causes exports to all the other groups (high income, low income, and all) to increase.

The results are also consistent with the hypothesis that high income, EU member countries are made relatively more competitive vis-à-vis low income countries due to an increase in environmental regulation stringency. The joint effect of an increase in environmental regulation stringency on exports from high income countries to low income countries is positive and significant. From Table 6, the effect of a one point increase in environmental regulation stringency on exports from high income countries in the EU to all high income countries is an increase of about 10.5%, and exports from high income countries in the EU to all low income countries increase by about 13%. Exports from high income, non-EU countries to other high income countries also increase by about 10% when environmental regulation stringency increases by one point, and exports from high income, non-EU countries to low income countries are not statistically affected. High income countries in the EU seem to increase their competitiveness compared to low income countries by increasing regulatory stringency.

The joint effect estimates presented in Table 6 suggest that an EU-level regulation that increases environmental regulation stringency for all EU members could have an enormous impact on exports flowing from those countries. In particular, low income, EU countries' exports might decrease as a result while high income, EU countries actually might experience an increase in exports.

For robustness, Table 7 shows the same regressions with GDP coefficients restricted to unity, as a robustness check. The similarity of the results lends credence to the results presented in Table 5. Table 8 shows the same tests performed in Table 6, corresponding to the results shown in Table 7, where GDP coefficients are restricted to unity. Again, the results when GDP coefficients are restricted to unity are nearly identical to when they are not restricted.

Conclusion

Changes in environmental regulation stringency in a country theoretically and empirically have different effects on bilateral trade flows depending on whether the country is part of the European Union and on whether the country is a high income or low income country. High income countries inside an economic integration agreement, such as the European Union, might have incentive to impose environmental regulations on the entire group of countries in the agreement. Regardless of whether the profit incentive actually exists or whether regulations are imposed on the entire EU, the consequences of an increase in environmental regulation stringency differ dramatically for high income countries in the EU compared to low income countries.

An increase in environmental regulation stringency in the exporting country generally increases exports from high income, EU member countries to all high income countries, although the difference between the estimate for high income, EU members and high income non-EU members is negligible. Exports from high income, EU members to all low income countries increases significantly when environmental regulation stringency is increased in the exporting country, while exports from high income, non-EU members does not change significantly due to a similar change.

Conversely, an increase in environmental regulation stringency unequivocally decreases exports from low income countries in the EU. A similar change in stringency has either no significant effect on low income, non-EU countries or possibly even a positive effect. I conclude that a European Union decree of increased environmental regulation stringency for all countries could have a negative impact on exporting industries in low income, EU countries while the impact on high income countries is possibly positive.

Regional trade blocs have grown rapidly in the last two decades; furthermore, these trade blocs are no longer simple "free trade agreements" but now also include other economic integration objectives like harmonization of competition law policy and monetary policy. This research shows that the interaction effects of regional trade blocs and regulations should not be ignored. Additionally, this paper indicates a possible political economy story behind the proliferation of the regionalization of regulations in general and of environmental regulations specifically. The possible political economy of the regionalization of regulations offers many topics for future research, as does the investigation of its empirical effects on different groups in the region.

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Table 4.1: Countries in dataset

All Countries	
ARGENTINA	JAPAN
AUSTRALIA	JORDAN
AUSTRIA	KOREA
BELGIUM	MALAYSIA
BOLIVIA	MAURITIUS
BRAZIL	MEXICO
BULGARIA	NETHERLANDS
CANADA	NEW ZEALAND
CHILE	NORWAY
CHINA,P.R.: MAINLAND	PERU
CHINA,P.R.:HONG KONG	PHILIPPINES
COLOMBIA	POLAND
COSTA RICA	PORTUGAL
CZECH REPUBLIC	RUSSIA
DENMARK	SINGAPORE
ECUADOR	SLOVAK REPUBLIC
EL SALVADOR	SOUTH AFRICA
FINLAND	SPAIN
FRANCE	SWEDEN
GERMANY	SWITZERLAND
GREECE	THAILAND
HUNGARY	TURKEY
ICELAND	UKRAINE
INDIA	UNITED KINGDOM
INDONESIA	UNITED STATES
IRELAND	VENEZUELA, REP. BOL.
ISRAEL	VIETNAM
ITALY	ZIMBABWE

Table 4.2: European Union countries

European Union Countries
AUSTRIA
BELGIUM
CZECH REPUBLIC
DENMARK
FINLAND
FRANCE
GERMANY
GREECE
HUNGARY
IRELAND
ITALY
NETHERLANDS
POLAND
PORTUGAL
SLOVAK REPUBLIC
SPAIN
SWEDEN
UNITED KINGDOM

Note: The other European Union members during this time period (Cyprus, Estonia, Latvia, Lithuania, Luxembourg, Malta, and Slovenia) are not included in the dataset because those countries' environmental regulation stringencies were not rated in every year from 2000 to 2005.

Table 4.3: High income countries and low income countries

High Income	Low Income	
AUSTRALIA	ARGENTINA	***SLOVAK REPUBLIC***
AUSTRIA	BOLIVIA	SOUTH AFRICA
BELGIUM	BRAZIL	THAILAND
CANADA	BULGARIA	TURKEY
CHINA,P.R.:HONG KONG	CHILE	UKRAINE
CZECH REPUBLIC	CHINA,P.R.: MAINLAND	VENEZUELA, REP. BOL.
DENMARK	COLOMBIA	VIETNAM
FINLAND	COSTA RICA	ZIMBABWE
FRANCE	ECUADOR	
GERMANY	EL SALVADOR	
ICELAND	***GREECE***	
IRELAND	***HUNGARY***	
ISRAEL	INDIA	
ITALY	INDONESIA	
JAPAN	JORDAN	
NETHERLANDS	KOREA	
NEW ZEALAND	MALAYSIA	
NORWAY	MAURITIUS	
SINGAPORE	MEXICO	
SPAIN	PERU	
SWEDEN	PHILIPPINES	
SWITZERLAND	***POLAND***	
UNITED KINGDOM	***PORTUGAL***	
UNITED STATES	RUSSIA	

Note: Countries in ***bold italics*** are EU members.

Table 4.4: Summary Statistics

Variable	Definition	Obs	Mean	Std. Dev.	Min	Max
lrxp	log of real exports	18480	4.283165	3.695479	-9.21034	12.50515
lrgdp_exporter	log of real GDP of exporter	18480	5.025897	1.647459	1.417903	9.304228
lrgdp_importer	log of real GDP of importer	18480	5.025897	1.647459	1.417903	9.304228
envregs_exp	environmental regulation stringency rating, exporter	18480	4.534226	1.284484	2.3	6.8
envregs_imp	environmental regulation stringency rating, importer	18480	4.534226	1.284484	2.3	6.8
EU	dummy = 1 if both exporter and importer are in EU in year *t*, = 0 otherwise	18480	0.072511	0.259339	0	1
EU*envregs_exp	interaction term = env. reg. stringency rating, exporter if exporter in EU in year *t*, = 0 otherwise	18480	1.75119	2.601912	0	6.8

Table 4.5: Gravity estimate with bilateral pair fixed-effects and time dummies

	(1)All-All	(2)High-High	(3)High-Low	(4)Low-High	(5)Low-Low	(6)Low-All
lrgdp_exporter	0.7783	0.6725	0.1783	0.8731	0.7750	0.8173
	(41.97)**	(6.92)**	(1.37)	(26.86)**	(23.25)**	(34.62)**
lrgdp_importer	0.1926	0.7565	0.1808	1.0116	0.2370	0.2085
	(10.39)**	(7.79)**	(9.49)**	(4.57)**	(7.12)**	(6.90)**
envregs_exp	0.1917	0.1042	-0.0718	0.0199	0.2283	0.1399
	(6.13)**	(2.07)*	(1.06)	(0.32)	(3.62)**	(3.13)**
envregs_imp	-0.0675	0.0886	-0.1439	-0.0092	-0.2322	-0.0809
	(2.46)*	(2.47)*	(4.15)**	(0.11)	(3.82)**	(1.81)
EU	-0.0689	0.0968	-0.0346	-0.0547	-0.1166	-0.0785
	(0.92)	(1.35)	(0.40)	(0.37)	(0.46)	(0.59)
EU*envregs_exp	-0.2110	-0.0042	0.1973	-0.3515	-0.7895	-0.5985
	(3.48)**	(0.07)	(2.29)*	(2.27)*	(4.98)**	(5.32)**
Constant	-0.8560	-2.4293	3.0859	-5.2309	-1.7458	-1.2989
	(3.95)**	(2.81)**	(3.80)**	(3.78)**	(5.12)**	(4.34)**
Observations	18480	3312	4608	4608	5952	10560
Bilateral Pairs	3080	552	768	768	992	1760
R^2	0.14	0.21	0.12	0.19	0.14	0.15

Absolute value of t statistics in parentheses
* significant at 5%; ** significant at 1%

Note: Regressions of the natural log of real exports in years 2000 – 2005 from exporting country, *i*, to importing country, *j*, on the natural log of real GDPs of both countries, the level of each countries' environmental stringency rating, an EU dummy (EU) equal to one if both the exporter and importer were in the EU in year *t*, and the exporter's environmental stringency rating interacted with a dummy indicating whether the exporter was in the EU in year *t* (EU*envregs_exp). Dummy variables for years 2001, 2002, 2003, 2004, and 2005 are included in each regression (estimates not reported here; available upon request). Fixed-effects for each bilateral pair are included in each regression.

Column 1 includes all country pairs in the dataset; column 2 includes only pairs where both exporter and importer are considered "high income;" column 3 includes only pairs where the exporter is "high income" and the importer is "low income;" column 4 includes only pairs where the exporter is "low income" and the importer is "high income;" column 5 includes only pairs where the exporter is "low income" and the importer is "low income;" and column 6 includes only pairs where the exporter is "low income" paired with all countries in the dataset.

Table 4.6: Gravity estimate with bilateral pair fixed-effects and time dummies, GDP coefficients restricted to unity

	(1)All-All	(2)High-High	(3)High-Low	(4)Low-High	(5)Low-Low	(6)Low-All
envregs_exp	0.1788	0.1051	-0.0724	-0.0070	0.1791	0.1010
	(5.38)**	(2.08)*	(-0.87)	(-0.11)	(2.71)**	(2.18)*
envregs_imp	-.0925	0.0899	-0.3353	-0.0106	-0.4055	-0.1034
	(-3.17)**	(2.50)*	(-7.98)**	(-0.13)	(-6.38)**	(-2.23)*
EU	-0.3131	0.0410	-0.3952	-0.1012	-0.4615	-0.3142
	(-3.93)**	(0.58)	(-3.79)**	(-0.69)	(-1.73)	(-2.26)*
EU*envregs_exp	-.1992	-0.0013	0.2026	-0.3748	-0.8111	-0.6125
	(-3.09)**	(-0.02)	(1.93)	(-2.42)*	(-4.85)**	(-5.24)**
Constant	-5.8467	-5.7081	-4.6436	-5.618	-5.4038	-5.8431
	(-31.50)**	(-19.22)**	(-12.83)**	(-11.07)**	(-17.57)**	(-22.96)**
Observations	18480	3312	4608	4608	5952	10560
Bilateral Pairs	3080	552	768	768	992	1760
R^2	0.0087	0.13	0.0252	0.0104	0.0432	0.0168

Note: Regressions of the natural log of real exports minus log of real GDP of exporter and importer (restricting their coefficients to unity) in years 2000 – 2005 from exporting country, *i*, to importing country, *j*, on the natural log of real GDPs of both countries, the level of each countries' environmental stringency rating, an EU dummy (EU) equal to one if both the exporter and importer were in the EU in year *t*, and the exporter's environmental stringency rating interacted with a dummy indicating whether the exporter was in the EU in year *t* (EU*envregs_exp). Dummy variables for years 2001, 2002, 2003, 2004, and 2005 are included in each regression (estimates not reported here; available upon request). Fixed-effects for each bilateral pair are included in each regression.

Column 1 includes all country pairs in the dataset; column 2 includes only pairs where both exporter and importer are considered "high income;" column 3 includes only pairs where the exporter is "high income" and the importer is "low income;" column 4 includes only pairs where the exporter is "low income" and the importer is "high income;" column 5 includes only pairs where the exporter is "low income" and the importer is "low income;" and column 6 includes only pairs where the exporter is "low income" paired with all countries in the dataset.

Table 4.7: Net Effects of an Increase in Environmental Regulation Stringency for EU Members from Table 5 Estimates

	All-All	High-High	High-Low	Low-High	Low-Low	Low-All
Net effect	-0.0193	0.1000*	0.1255*	-0.3316*	-0.5612**	-0.4586**
P-Value of Wald test of joint significance	0.7173	0.0291	0.0413	0.0271	0.0003	0.0000

* significant at 5%; ** significant at 1%

Note: The net effect for EU members arises from summing the coefficient on exp_envregs and the coefficient on exp_EU_regs shown in Table 5.

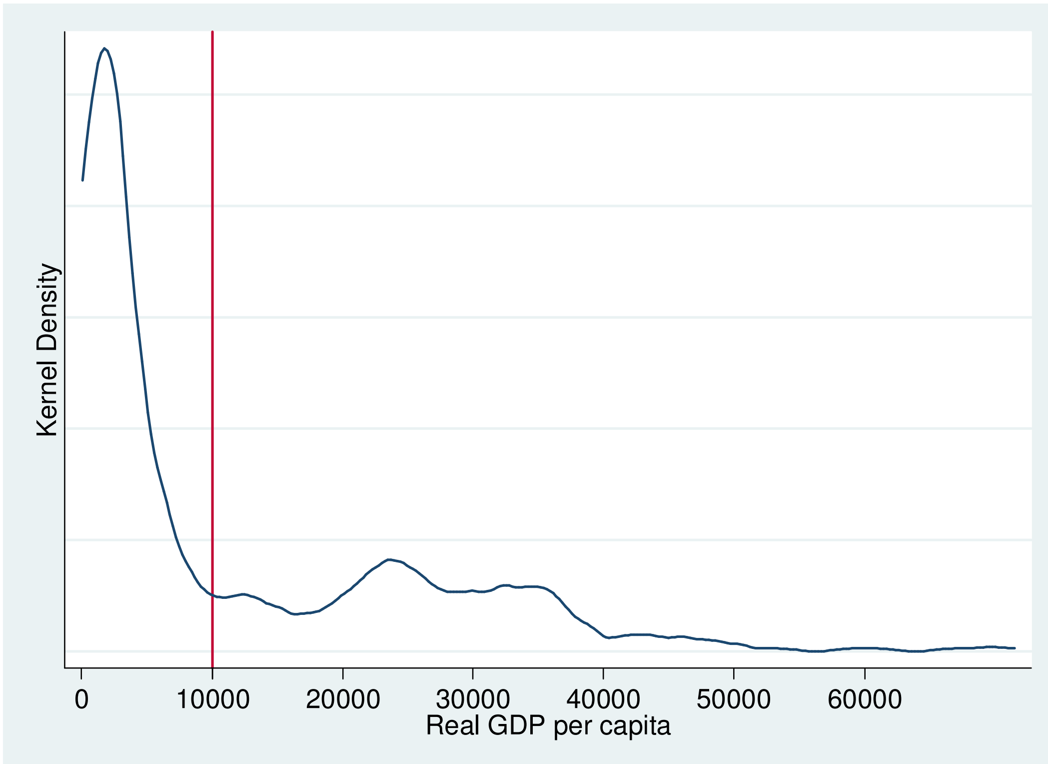
Table 4.8: Net Effects of an Increase in Environmental Regulation Stringency for EU Members from Table 5a Estimates

	All-All	High-High	High-Low	Low-High	Low-Low	Low-All
Net effect	-0.0204	0.10038	0.1302	-0.3818*	-0.6320**	-0.5115**
P-Value of Wald test of joint significance	0.7187	0.0236	0.0828	0.0108	0.0001	0.0000

* significant at 5%; ** significant at 1%

Note: The net effect for EU members arises from summing the coefficient on exp_envregs and the coefficient on exp_EU_regs shown in Table 5a.

Figure 4.1: Kernel Density of Real GDP per Capita with $10,000 cutoff line added



CHAPTER FIVE

CONCLUSION

This dissertation addresses issues in measurement and estimation techniques in the environmental economics and international trade. The first chapter presents research in environmental economics the questions and tests the validity of the assumption of complete information about environmental disamenities in housing markets. I find that the market appears well-informed about the location of groundwater contamination prior to the passage of multiple local regulation pertaining to groundwater in the vicinity.
and international trade, and the third chapter synthesizes the two fields in addressing the effects of environmental regulations on international trade. In all of the chapters, there is a consistent theme of improving metrics, proxies, and estimation techniques currently used in economic research.

APPENDICES

Appendix A

Countries in dataset of chapter three

The following is a list of the 96 countries potentially used in the regressions, depending upon availability of non-zero and non-missing trade flows:

Austria	Belgium–Luxembourg	Denmark
Finland	France	Germany
Greece	Ireland	Italy
Netherlands	Norway	Portugal
Spain	Sweden	Switzerland
United Kingdom	Canada	Costa Rica
Dominican Republic	El Salvador	Guatemala
Haiti	Honduras	Jamaica
Mexico	Nicaragua	Panama
Trinidad and Tobago	United States	Argentina
Bolivia	Brazil	Chile
Colombia	Ecuador	Guyana
Paraguay	Peru	Uruguay
Venezuela	Australia	New Zealand
Bulgaria	Hungary	Poland
Romania	Egypt	India
Japan	Philippines	Thailand
Turkey	Korea	Algeria
Angola	Ghana	Kenya
Morocco	Mozambique	Nigeria
Tunisia	Uganda	Zambia
Zimbabwe	China (Hong Kong)	Indonesia

Iran	Israel	Pakistan
Singapore	Sri Lanka	Syrian Arab Republic
China,P.R.: Mainland	Albania	Bangladesh
Burkina Faso	Cameroon	Cyprus
Côte d'Ivoire	Ethiopia	Gabon
Gambia, The	Guinea–Bissau	Madagascar
Malawi	Malaysia	Mali
Mauritania	Mauritius	Niger
Saudi Arabia	Senegal	Sierra Leone
Sudan	Congo, Dem. Rep. of	Congo, Republic of

Appendix B

Endogeneity from environmental regulation stringency variables

Estimates of the effects of changes in environmental regulation stringency might also suffer from endogeneity in a gravity context when the measure of environmental regulation stringency is an outcome measure, such as energy use per capita, carbon dioxide emissions, or sulfur emissions. Countries' initial endowments of such sulfur- and carbon dioxide- emitting resources as coal and oil, as well as differences in the sulfur content of such resources, are not controlled for in typical gravity specifications but certainly would affect both choice of regulation levels as well as measured outcomes of a given level of regulation.

To formally demonstrate this, let S_{it} represent environmental regulation stringency in country *i* at time *t*. Equation (4.3) implicitly includes this variable of interest in the error term. Thus, the error term from equation (4.3), $\ln \varepsilon_{ijt}$, can be written

$$\ln \varepsilon_{ijt} = \gamma_1 S_{it} + \gamma_2 S_{jt} + \gamma_3 S_{it} E_{ijt} + u_{ijt} \qquad \text{(A1)}$$

where S_{it} is environmental regulation stringency and E_{ijt} still indicates whether both countries are in an economic integration agreement in year *t*. The interaction term accounts for the possibility of EIA-level imposition of environmental regulations differing from unilateral changes in environmental regulations. u_{ijt} is white noise; $E(u_{ijt})=0$.

Most estimates of the effects of environmental regulations on bilateral trade flows rely on proxies for environmental regulation stringency; for example, Van Beers and Van den Bergh (1997) use societal indicators of environmental regulations' effects, such as recycling rates and market share of unleaded gasoline for part of their analysis; Harris et al. (2002), following another method used by Van Beers and Van den Bergh, use energy intensity measures, such as energy consumed per capita in a country in year *t* compared to that consumed per capita in a baseline year, 1980. Usage of such an environmental policy outcome variable as proxy for environmental regulation stringency can easily introduce endogeneity into estimates of the effects of changes in that outcome variable on trade flows. Let Q denote the proxy used for environmental regulation stringency:

$$Q = f(S, O) \qquad \text{(A2)},$$

where *S* is the actual stringency level and *O* represents other relevant factors that could affect the outcome variable such as country endowment of petroleum reserves or the sulfur content of coal and petroleum[31]. I assume a simple functional form for *Q*:

$$Q = \frac{1}{\psi} S - \frac{1}{\psi} O \qquad \text{(A3)}.$$

Solving for S yields

$$S = \psi Q + O \qquad \text{(A4)}.$$

Substituting equation (A4) into equation (A1),

$$\ln \varepsilon_{ijt} = \gamma_1 \psi_1 Q_{it} + \gamma_2 \psi_2 Q_{jt} + \gamma_3 \psi_3 Q_{it} E_{ijt} + u_{ijt} + O_{it} \qquad \text{(A5)}$$

Specification of the gravity equation shown in equation (4.3) to include *Q*, the proxy for environmental stringency, gives

[31] If energy intensity is used as the proxy, then endowment of energy-rich resources is important. If sulfur emissions are used, then the differences in sulfur content of coal, petroleum, and other resources affects the outcome Q.

$$\begin{aligned}\ln X_{ijt} &= \ln\beta_0 + \beta_1 \ln Y_{it} + \beta_2 \ln Y_{jt} + \beta_3 \ln N_{it} + \beta_4 \ln N_{jt} \\ &+ \beta_8 E_{ijt} + \beta_9 Q_{it} + \beta_{10} Q_{jt} + \beta_{11} Q_{it} E_{ijt} + \delta_{ij} + \lambda_t + \ln\varepsilon_{ijt}\end{aligned} \tag{A6}$$

where the error term in equation (A6) differs from that given in equation (4.3) because the first three terms of the RHS in equation (A5) are now explicitly in the RHS of equation A6. The error term in equation (A6) is therefore

$$\ln\varepsilon_{ijt} = u_{ijt} + O_{it} \tag{A7}$$

Because O_{it} determines Q_{it}, the correlation between O_{it} and Q_{it} is non-zero, implying that

$$E(u_{ijt} + O_{it} \mid Q_{it}) \neq 0 \qquad \text{(A8).}$$

Thus, any outcome measure that depends on both environmental regulation stringency and country-specific endowment characteristics introduces bias into gravity equation estimates of the effect of environmental regulation stringency on trade flows.

Appendix C

Modeling an ordinal signal on a latent variable

Let the data generating process be given by

$$\ln y_k = -\ln \mu_k + \ln \varepsilon \qquad \text{(B1)}$$

where y is the regulatory stringency level chosen by the country k, μ is the regulatory laxness signaled by the country, ε is noise in executive i's observation of the signal, and ε~U[0,1]. Rewriting equation (B1) yields

$$y_k = \frac{\varepsilon_i}{\mu_k} \qquad \text{(B2).}$$

Executives are asked to rate between 1 and 7 each country's environmental regulation stringency relative to other countries; I assume some threshold, τ_i, to exist between each two levels, as is illustrated below in Figure B1. If the signaling process for country k yields a result in excess of a given threshold, the executive rates country k's stringency at the next higher level.

Figure B1: Thresholds in rating range

Rating: 1 | 2 | 3 | 4 | 5 | 6 | 7

τ_1 τ_2 τ_3 τ_4 τ_5 τ_6

Note that, despite the appearance of τ_i in Figure B1, the levels of τ_i are not restricted to any range. Rather, these thresholds are simply the information that is signaled to executives. For a simple example, assume the entirety of the signaling process is done by the amount of money spent on enforcement of environmental regulations. Executives rate each country according to the millions of dollars spent on regulations in a given year, while controlling for their expectations of corruption and governmental inefficiency in each country. If the range of expenditure on regulation is from \$1,000,000 to \$71,000,000, then the thresholds could be any transformation of six points on the expenditure line that maintains their collinearity and the ratios of the distances between them.

Let $x_{i,k}$ denote the rating given by executive i to country k. Given the six thresholds, the probability that country k will receive any given rating can be written as

$$prob(x_{ik} = 1) = prob(\ln y_k < \ln \tau_1) \quad \text{(B3.1)}$$

$$prob(x_{ik} = 2) = prob(\ln y_k < \ln \tau_2) - prob(\ln y_k < \ln \tau_1) \quad \text{(B3.2)}$$

$$prob(x_{ik} = 3) = prob(\ln y_k < \ln \tau_3) - prob(\ln y_k < \ln \tau_2) \quad \text{(B3.3)}$$

. .

. .

$$prob(x_{ik} = 6) = prob(\ln y_k < \ln \tau_6) - prob(\ln y_k < \ln \tau_5) \quad \text{(B3.6)}$$

$$prob(x_{ik} = 7) = 1 - prob(\ln y_k < \ln \tau_6) \quad \text{(B3.7)}$$

Using equation (B2), equations (B3.1 – B3.7) can be restated as

$$prob(x_{ik} = 1) = prob(\ln y_k < \ln \tau_1) = prob(\frac{\varepsilon_i}{\mu_k} < \tau_1) = prob(\varepsilon_k < \mu_k \tau_1) = F(\mu_k \tau_1) \quad \text{(B4.1)}$$

$$prob(x_{ik} = 2) = prob(\ln y_k < \ln \tau_2) - prob(\ln y_k < \ln \tau_1) = F(\mu_k \tau_2) - F(\mu_k \tau_1) \quad \text{(B4.2)}$$

$$prob(x_{ik} = 3) = prob(\ln y_k < \ln \tau_3) - prob(\ln y_k < \ln \tau_2) = F(\mu_k \tau_3) - F(\mu_k \tau_2) \quad \text{(B4.3)}$$

. .

. .

$$prob(x_{ik} = 6) = prob(\ln y_k < \ln \tau_6) - prob(\ln y_k < \ln \tau_5) = F(\mu_k \tau_6) - F(\mu_k \tau_5) \qquad \text{(B4.6)}$$

$$prob(x_{ik} = 7) = 1 - prob(\ln y_k < \ln \tau_6) = 1 - F(\mu_k \tau_1) \qquad \text{(B4.7)}$$

Setting up GMM, the expected value of x_i is

$$E(x_{ik}) = 1prob(x_{ik} = 1) + 2prob(x_{ik} = 2) + \cdots + 7prob(x_{ik} = 7) \qquad \text{(B5)}$$

$$E(x_{ik}) = F(\mu_k \tau_1) + 2[F(\mu_k \tau_2) - F(\mu_k \tau_1)] + \cdots + 7[1 - F(\mu_k \tau_6)] \qquad \text{(B6)}$$

$$E(x_{ik}) = 7 - F(\mu_k \tau_1) - F(\mu_k \tau_2) - \cdots - F(\mu_k \tau_6) \qquad \text{(B7)}$$

Along with the assumption that ε~U[0,1], I scale τ_i such that $\sum_{l=1}^{6} \tau_l = 1$. The expected value of x_i is thus

$$E(x_{ik}) = 7 - \mu_k \tau_1 - \mu_k \tau_2 - \cdots - \mu_k \tau_6 = 7 - \mu_k \sum_{l=1}^{6} \tau_l \qquad \text{(B8)}$$

$$E(x_{ik}) = 7 - \mu_k \qquad \text{(B9)}$$

Therefore, by GMM estimation of the parameter μ, equation (B9) is rewritten as

$$\hat{\mu} = 7 - \bar{x} \qquad \text{(B10)}$$

where $\mu = \hat{\mu} + v$ and $v \sim N(0, \bullet)$. Thus, our best guess of μ, the regulatory laxness signaled by a country, is an affine transformation of $\bar{x}$, albeit measured with error, v. However, because

$$\beta\mu = \beta(7 - \bar{x} + v) \qquad \text{(B11)}$$

$$= 7\beta + \tilde{\beta}\bar{x} + \beta v \qquad \text{(B12),}$$

any bias from first and third terms in the RHS of equation B4.2 is lumped into the intercept and error term, respectively, yielding $\tilde{\beta}$ as an unbiased estimate on the sample mean.