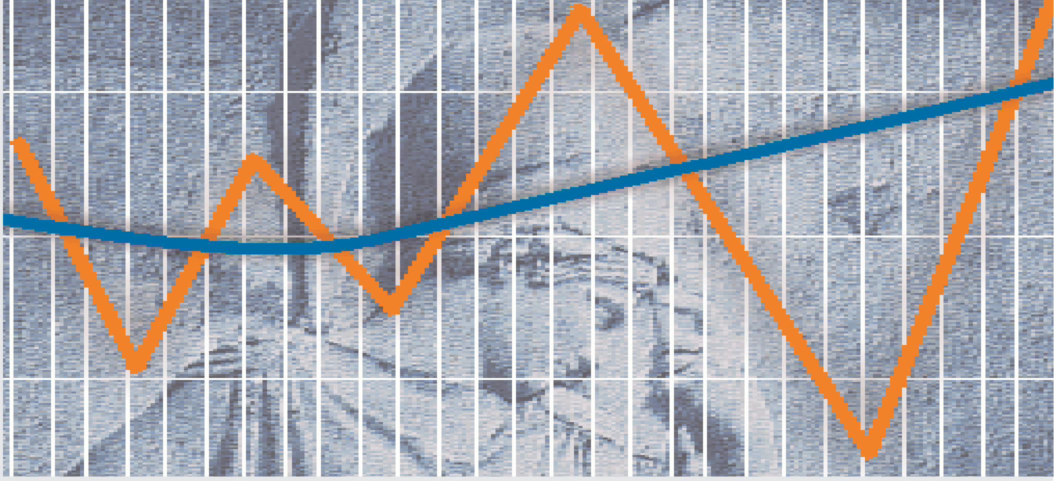

ifo Beiträge
zur Wirtschaftsforschung

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Trade, Climate Policy and Carbon Leakage
Theory and Empirical Evidence

Rahel Aichele

ifo Institut
Leibniz-Institut für Wirtschaftsforschung
an der Universität München e.V.

Herausgeber der Reihe: Hans-Werner Sinn
Schriftleitung: Chang Woon Nam

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Bibliografische Information der Deutschen Nationalbibliothek

Die Deutsche Nationalbibliothek verzeichnet diese Publikation
in der Deutschen Nationalbibliografie; detaillierte bibliografische
Daten sind im Internet über
http://dnb.d-nb.de
abrufbar

ISBN-13: 978-3-88512-541-9

Druck: ifo Institut, München

ifo Institut im Internet:
http://www.cesifo-group.de

Preface

This volume was prepared by Rahel Aichele while she was working at the Ifo Institute. It was completed in December 2012 and accepted as a doctoral thesis by the Department of Economics at the University of Munich. It includes five self-contained chapters.

The chapters investigate how, in a globalized world, partial international climate policy has shaped international trade and the location of emissions. The main research question is whether carbon leakage empirically occurs, thus undermining the effectiveness of unilateral climate policy. The thesis provides new empirical tests for leakage based on a newly developed panel dataset of the carbon content of bilateral trade and country-level carbon footprints for 40 countries over the period 1995-2007. The Kyoto Protocol constitutes a quasi-natural experiment of international climate policy. We exploit policy evaluation techniques to study its effects on CO_2 emissions (Chapter 1), carbon footprints (Chapter 2), trade values (Chapter 3) and the carbon content of trade (Chapter 4). To deal with self-selection into treatment, fixed-effects, instrumental variables or matching econometrics techniques are employed. We find empirical evidence for carbon leakage in the data. The extent of carbon leakage is then quantified with a structurally estimated trade model (Chapter 5). Simulations of counterfactual climate policy scenarios show moderate but non-negligible leakage rates.

Keywords: Carbon content of trade, carbon footprint, carbon leakage, climate policy, CO_2 emission, competitiveness, energy, evaluation model, gravity equation, instrumental variables, international trade, Kyoto Protocol, matching econometrics, treatment effects

JEL-No.: C26, F18, F53, Q48, Q54, Q56, Q58.

Acknowledgements

First and foremost, I would like to thank my supervisor Gabriel Felbermayr. Your enthusiasm for economics is an inspiration and your guidance, encouragement and support during the Ph.D. time have been invaluable to me. I also thank my second supervisor, Scott Taylor, for inviting me to the University of Calgary and his constructive feedback on many parts of this thesis. Furthermore, I thank Karen Pittel for joining my committee.

I thank all my colleagues at the Department International Trade at the Ifo Institute and the University of Hohenheim for their companionship. I enjoyed and profited from a great working environment and take away fond memories. Special thanks go to Jasmin Gröschl (for gravity discussions and numerous fun conference trips), Beno Heid (for structural gravity discussions) and Benjamin Jung (for getting me started in STATA). Your comments and suggestions improved my work a great deal. Another thank you goes to all the mountain retreaters, I had a blast while hiking and discussing research with you. I would also like to thank DFG and BMBF for funding my research.

Part of this doctoral thesis was written during my research stay at the University of Calgary. The Department of Economics provided a welcoming and inspiring environment. In particular, I wish to thank Jevan Cherniwchan and Jennifer Winter for countless interesting discussions and their support during my stay and Jared Carbone for teaching me GAMS. Funding from the DAAD scholarship for Ph.D. research studies is gratefully acknowledged.

Finally, I would like to thank my wonderful family. I am grateful for their constant love and support. Last but not least, my greatest gratitude goes to Klaus Jöns. You have been there for me along the entire way. Thank you for your continuous encouragement, your patience and your loving support!

Trade, Climate Policy and Carbon Leakage: Theory and Empirical Evidence

Inaugural-Dissertation
zur Erlangung des Grades
Doctor oeconomiae publicae (Dr. oec. publ.)
an der Ludwig-Maximilians-Universität München

2012

vorgelegt von

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Promotionsabschlussberatung:	15. Mai 2013

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Introduction

There is an increasing scientific understanding that anthropogenic greenhouse gas (GHG) emissions have led to a warming trend since the industrial revolution (see for example IPCC, 2007). Global warming is associated with adverse effects. A recent World Bank Report cautions that a temperature increase of 4 °C above pre-industrial levels would bring "unprecedented heat waves, severe drought, and major floods in many regions, with serious impacts on ecosystems and associated services." (World Bank, 2012). The well-being of current and future generations are at stake. A temperature increase above 2 °C should be avoided (World Bank, 2012, and United Nations Framework Convention on Climate Change). This requires climate change mitigation efforts.

GHG emissions are a global pollutant: additional GHG in the atmosphere has the same effect on global average temperature no matter where emission takes place. Mitigation is costly but the benefits of forgone temperature increases arise everywhere. Hence, the atmosphere is a global public good. Each nation and each individual has an incentive to free-ride on others' GHG saving efforts. Indeed, the Stern Review describes climate change as "the greatest and widest-ranging market failure ever seen." (Stern, 2007, Executive Summary).

The threat of detrimental global warming has long been acknowledged in the international policy arena. In 1992, the United Nations Framework Convention on Climate Change (UNFCCC) was launched during the "Earth Summit" in Rio de Janeiro. It sets the legal framework to negotiate international treaties governing GHG emission reductions. With the Kyoto Protocol of 1997, promises of emission cutbacks were fixed in such an international treaty. 37 industrialized countries and the EU committed to reduce their emissions in the 2008-2012 period by 5.2% compared to the base year 1990. Each nation has a country-specific emission target, ranging from emission cutbacks of 8% (e.g. in the EU and Switzerland) to a maximum emission increase of 10% in Iceland. To ensure cost-minimizing emission abatement, the Kyoto Protocol features so called flexible market mechanisms: Emission Trading, Clean Development Mechanism and Joint Implementation. During the Conference of Parties in Doha (COP 18) in December 2012 the EU and eleven other Kyoto countries agreed on a second commitment period under the Kyoto Protocol from 2013-2020. But Japan, Russia, and Canada, which were subject to GHG emission limits in the first Kyoto period, are no longer part of the deal.

Even though the Kyoto Protocol marks a historical milestone in international climate policy, it is subject to criticism. First, the Kyoto Protocol is a voluntary international agreement. There is no credible enforcement mechanism ensuring that Kyoto countries engage in GHG mitigation efforts and comply with their emission targets. And indeed, as of 2010 – the latest year reported to the UNFCCC – many countries are well above their emission limits. To give some examples, Australia has increased its GHG emissions by 29.8%; its target is 8%. Or Canada has increased its emissions by 17.4%; before Canada withdrew from the Kyoto Protocol in December 2011 its target was -6%.

Second, due to equity reasons industrialized countries are supposed to take the lead in climate change mitigation efforts (the UNFCCC's *principle of common but differentiated responsibility*). Consequently, developing countries like China and India – two large GHG emitters – are exempt from GHG emission limitations under the Kyoto Protocol. In addition, the USA was the only industrialized country not to ratify the Kyoto Protocol because the US Administration faced internal political constraints.[1] Hence, a mere 25% of worldwide GHG emissions are covered by the first Kyoto period. Due to the withdrawal of Canada, Japan and Russia, this share has been further reduced to 15% in the proposed second commitment period (*Kyoto II*).

International trade is an integral part of our economic system. Trade volumes have drastically surged over the last decades. Falling trade costs have increasingly integrated the global production chain. And China and Southeast Asian countries have entered the world market. In such an environment, incomplete country coverage of a climate treaty like the Kyoto Protocol implies a *carbon leakage* threat: Climate policy leads to cost increases in production, and particularly so in the production of energy- and CO_2-intensive goods. International trade increases in response. Consumers switch to lower-cost products from countries with less stringent climate policy. Part of the emission savings in Kyoto countries

[1] Ratification would have implied a 7% US emission reduction target.

are undone by induced additional emissions elsewhere. This undermines the environmental effectiveness of partial climate deals.

At the COP 18, UNFCCC member countries expressed their intent to adopt a global climate treaty by 2015. However, it is unclear whether the various policy constraints can be overcome. It is a real possibility that future international climate policy efforts remain partial like Kyoto II or the EU Emission Trading System. In such a scenario of partial climate deals, it is crucial to understand how international trade and climate policy interact. Is carbon leakage a mere theoretical possibility or does it actually occur? And if it occurs, what is its magnitude?

This dissertation comprises a collection of five articles shedding light on this question. So far, the literature predominantly employs computable general equilibrium models to study competitiveness and leakage effects of climate policy (see for example work by Felder and Rutherford, 1993; Böhringer and Vogt, 2003; Babiker and Rutherford, 2005; Elliott et al., 2010; Burniaux and Oliveira Martins, 2012). In contrast, this dissertation provides theory-guided empirical estimation of trade and leakage responses to unilateral climate policy.

Empirical estimates on trade effects of climate policy are scant and empirical carbon leakage estimates do not exist. This is in part due to the complex nature of climate policy. Countries can reduce their GHG emissions with carbon taxes, emission cap-and-trade systems, subsidies to research and development of green technologies, feed-in tariffs for alternative energy sources like wind power and solar energy, or regulations of technology standards. In short, measuring climate policy stringency is a complex task. In the end, all these measures lead to energy price increases. Hence, an alternative approach would be to directly study responses of trade flows to energy price shocks. But there is not yet a broad database on energy prices covering many countries.

Therefore, we propose to explore a historical quasi-natural experiment in climate policy: the Kyoto Protocol. We *ex-post* evaluate its effects on CO_2 emissions, carbon footprints and bilateral trade and carbon content of trade patterns.[2] The main contributions are threefold: we develop a new panel dataset of carbon footprints and the bilateral carbon content of trade. Second, we provide first empirical tests for carbon leakage with this new data. Last, we provide counterfactual analysis of leakage in general equilibrium with a structural gravity model.

[2] Due to data availability issues, the focus is on carbon dioxide (CO_2) which makes up about 80% of greenhouse gas emissions.

A country's ratification of GHG emission limitations under the Kyoto Protocol is the outcome of a political process. In other words, Kyoto commitment is not random. Country-specific conditions like historical emission levels, climate preferences, availability of alternative energy sources, exposure to climate shocks, expected competitiveness effects, or technology influence this choice. These are partly not observable. If we do not control for policy endogeneity, empirical estimates are confounded and lead to spurious correlation. We deal with omitted variables bias and self-selection by employing differences-in-differences estimation, and using instrumental variables (IV) as well as matching econometrics strategies.

In the following, we briefly highlight the main contributions and findings of each of the five chapters in turn. Chapter 1 is joint work with Gabriel Felbermayr. It explores whether the Kyoto Protocol has made a difference for climate policy and CO_2 emissions. This constitutes a prerequisite for the leakage phenomenon. GHG emissions have increased in many Kyoto countries during the last decade. This leads to a widespread belief that the Kyoto Protocol has not helped to bring down GHGs. However, we do not observe a counterfactual world without Kyoto commitments. To answer the question of Kyoto's effect on emissions, one has to resort to empirical estimation. The carbon Kuznets curve literature (see, e.g. Martínez-Zarzoso and Bengochea-Morancho, 2004; Stern, 2004; Galeotti et al., 2006; Wagner, 2008) focuses on GDP per capita and demographic factors as drivers of CO_2 emissions. We extend this analysis and study the role of the Kyoto Protocol. We propose an instrumental variables approach to deal with the endogeneity of Kyoto commitment. The gist of the instrumentation strategy is as follows: membership to the International Criminal Court (ICC) – an international body dealing with the prosecution of war crimes – and Kyoto commitment are strongly correlated. Some countries are more willing to give up their national sovereignty for international matters (like global warming or prosecution of war criminals) than others. Furthermore, we argue that a country's CO_2 emissions are not related in any way with its commitment to put war criminals in front of an international court. So the exclusion restriction holds and ICC membership provides a valid instrument for Kyoto commitment. Endowments with energy resources, technology and other unobservable country-specific conditions influence CO_2 emission levels. These factors are also related to the ratification decision. Thus, we use country-fixed effects in the empirical specification. Applying our IV strategy, we find that Kyoto countries reduce their domestic CO_2 emissions by about 10%. To support our finding, we also analyze Kyoto's effect on policy outcomes like the energy and electricity mix or fuel prices.

Chapter 2 offers a novel approach to empirically study the leakage phenomenon with carbon footprint data. It is joint work with Gabriel Felbermayr. A country's *carbon footprint* measures all CO_2 emissions associated with a country's goods consumption and investment vector. Emissions occurring in the upstream production chain of a good are factored in; no matter whether they are emitted domestically or abroad. Carbon leakage implies that Kyoto countries have saved emissions domestically but at the same time started to net import more CO_2 embodied in traded goods from abroad. In other words, it drives a wedge between a country's domestic emissions and its footprint. We are the first to empirically evaluate Kyoto's effect on CO_2 emissions and carbon footprints. A number of descriptive studies (see, e.g. Ahmad and Wyckoff, 2003; Hertwich and Peters, 2009; Davis and Caldeira, 2010) construct footprint estimates for a cross-section of countries. Peters et al. (2011) show that the emissions embodied in goods and services trade have increased by about 80% between 1990 and 2008. But their footprint times series is based on emission coefficients of the year 2004, while other year's emission coefficients are constructed with aggregate output growth. For our estimation, we develop a panel dataset of carbon footprints for 40 countries covering the years 1995-2007. The dataset's novelty arises from its time dimension as well as the use of country-sector-and-time specific emission coefficients. Since countries differ greatly in their emission intensities across sectors and across time this is important to get precise footprint estimates. The dataset's panel dimension is crucial to control for country-specific confounding factors outlined above. Applying the IV strategy laid down above, we find that ratifying Kyoto commitment reduces CO_2 emissions by 7% but has no effect on carbon footprints. The Kyoto Protocol has indeed driven a further wedge between the CO_2 a country produces and the CO_2 it consumes. This is consistent with carbon leakage.

As we have pointed out above, climate policy has a direct effect on trade flows. Chapter 3 analyzes how Kyoto commitment of the exporting country affects bilateral trade flows with the workhorse model in international trade: the gravity equation. It is joint work with Gabriel Felbermayr. The study is embedded in a large empirical literature investigating the effects of environmental regulation on trade flows (*pollution haven effect*) in the gravity framework (see, e.g., van Beers and van den Bergh, 1997; Ederington et al., 2005; Levinson and Taylor, 2008; Grether et al., 2012). However, this literature typically focuses on local pollutants like sulphur dioxide while evidence for climate policy is scant. In the context of GHGs, the only study by the World Bank (2008) finds small effects of carbon taxes on aggregate bilateral trade flows. However, the authors do not take into account endogeneity of carbon taxes. In addition, Hallak (2010) points out a systematic aggregation bias when looking at aggregate trade. Therefore, we study aggregate and sectoral trade flows at the two digit SITC classification level. We choose a matching econometrics strategy as in Baier and Bergstrand (2009b) to deal with self-selection. To control for unobserved country-pair heterogeneity, we employ a regression-adjusted differences-in-differences matching estimator using various propensity score and a non-parametric nearest neighbor matching approach. Based on the literature on international environmental agreement formation (see, e.g., Murdoch and Sandler, 1997; Carraro and Siniscalco, 1998; Beron et al., 2003; York, 2005) and gravity models, we identify matching variables which drive self-selection: joint GDP, population, real GDP per capita, energy intensity differences as well as trade cost proxies. Our estimates suggest a negative average treatment effect of the exporter's Kyoto commitment on exports. However, the average treatment effect *on the treated* is smaller in absolute size and ranges between -10 and -13%. In other words, not accounting for self-selection overstates the negative effect of Kyoto commitment. Results are robust to the inclusion of variables reflecting countries' political institutions. Our study also highlights differences in sectoral responses to Kyoto commitment. Only 17 out of 51 sectors (two digit SITC level) display statistically significant negative effects. These are mostly energy-intensive sectors producing rather homogeneous goods like iron and steel or non-ferrous metals.

Chapter 4 provides a theoretical gravity framework for the carbon content of trade and empirically tests the existence of carbon leakage with sectoral carbon content of trade data. It is joint work with Gabriel Felbermayr. The idea behind our approach is as follows: if we observe higher carbon imports from non-Kyoto into Kyoto countries after ratification, this implies that emission has relocated to unconstrained countries. This is sufficient to show the existence of carbon leakage. The approach goes beyond previous work estimating trade effects of climate policy (World Bank, 2008; Aichele and Felbermayr, 2013) and bears two major advantages. Unlike bilateral trade flows, bilateral carbon content of trade flows reflect country-specific carbon intensities. In addition, whether intermediate inputs for a good are sourced domestically or imported matters for emission relocation when the good's production moves abroad. The carbon content of trade comprises this information. In short, the carbon content of trade provides a better understanding of emission relocation between countries than trade flows. In a similar vein, Grether et al. (2012) empirically test the pollution haven effect with data on embodied pollution of sulphur dioxide. To guide our empirical strategy, we set up a gravity model with several sectors and intermediate input linkages. Countries differ in their carbon prices. The theoretical framework establishes an estimable gravity equation for the carbon content of trade. Kyoto commitment is a country- and time-specific decision. Hence, a panel dataset of bilateral trade and carbon content of trade flows allows us to control for Kyoto's endogeneity with country-and-time specific dummies. We find that Kyoto commitment in the importer but not the exporter increases imports by 5% and carbon imports by 8%. This finding is robust to sample variations and a long differences-in-differences approach. We conclude that carbon leakage is an empirically relevant phenomenon.

Chapter 5 quantifies the extent of carbon leakage in counterfactual climate policy scenarios with a structural gravity model. Typically, the magnitude of carbon leakage is assessed with computable general equilibrium models (see applications by Felder and Rutherford, 1993; Böhringer and Vogt, 2003; Babiker and Rutherford, 2005; Elliott et al., 2010; Burniaux and Oliveira Martins, 2012). However, resulting leakage rates are ambiguous. Results depend on parameter choices, for example for the elasticity of substitution, which are often not founded by empirical estimates. On the other hand, Chapters 2 and 4 provide empirical evidence for the existence of carbon leakage. Yet, the extent of carbon leakage cannot be quantified. Climate policy has non-trivial general equilibrium effects via income and third-country effects. These are absorbed in country or country-and-time fixed effects in the empirical gravity estimation. Chapter 5 resorts to structural estimation and counterfactual simulation techniques to quantify leakage in general equilibrium. While structural gravity is a standard tool in the evaluation of the gains from trade (see applications by Anderson and van Wincoop, 2003; Eaton and Kortum, 2002; Dekle et al., 2007; Alvarez and Lucas, 2007; Egger et al., 2011b; Bergstrand et al., 2013, amongst others) a structural gravity approach has not been applied to the leakage context so far. The point of departure is a new trade theory gravity model. The model's key parameters, i.e. bilateral trade costs and the elasticity of substitution, are empirically estimated. The model fit with observed data on GDPs, emissions and bilateral trade flows is high and creates a credible benchmark for counterfactual policy experiments. The counterfactual analysis focuses on two climate policy scenarios: a certificate price increase in the European Union's Emission Trading System and GHG commitments under the second period of the Kyoto Protocol. We find that in 2007 an EU emission certificate price of 15 US-$ per ton of CO_2 brings EU emissions down to a level promised for the first Kyoto period. At the same time, it leads to 10% emission relocation. A carbon tax of 39.1 US-$ per ton of CO_2 would bring Kyoto II countries in line with their promised emission targets for the second Kyoto period. However, 8% of the achieved CO_2 emission savings leak away. The predicted extent of carbon leakage is moderate but non-negligible.

In summary, the articles comprised in this dissertation show that carbon leakage is a non-negligible real world phenomenon. We conclude this Introduction with a brief discussion of possible policy implications. A *global* climate deal is first-best. Negotiating such a treaty is crucial for climate change mitigation efforts. Otherwise a non-negligible part of costly emission cutbacks will leak away. If a global deal proves to be politically infeasible, partial climate deals constitute a second-best. However, to ensure their environmental effectiveness they should be designed such that leakage is prevented. The literature discusses several options. Climate policy could target footprints, i.e. the GHG embodied in consumption, instead of domestic production emissions (see proposals in Bastianoni et al., 2004; Eder and Narodoslawsky, 1999; Peters, 2008). Similar in vein to a value added tax, governments could charge embodied CO_2 consumption taxes. This limits incentives to shift production abroad. However, such a system is hard to administer. It requires information on each good's emission intensity and its intermediate usage. Alternatively, a CO_2 tax on domestic production could be accompanied by carbon-related border tax adjustments for imports and tax exemptions for exports. The conformity of such measures under the World Trade Organization is an open issue (see the discussions in Bhagwati and Mavroidis, 2007; Ismer and Neuhoff, 2007; Goh, 2004; Sindico, 2008). Concluding, the discussed systems have advantages and drawbacks. How to address carbon leakage in partial climate deals is an open and ongoing debate meriting further research.

All five chapters of this dissertation are self-contained and include their own introductions and appendices such that they can be read independently.

Chapter 1

What a Difference Kyoto Made: Evidence from Instrumental Variables Estimation*

1.1 Introduction

A large and insightful theoretical literature studies the problems of international climate policy. Due to the public goods nature of CO_2 emissions, it is individually rational for a country to free-ride on others' emission reductions (see e.g. Congleton, 2001, for a survey). International environmental agreements (IEA) are discussed as a solution to the dilemma (for examples, see Andreoni and McGuire, 1993; Welsch, 1995; Hoel, 1997; Carraro and Siniscalco, 1998; Lange and Vogt, 2003).[1] In the Kyoto Protocol, 37 industrialized nations and the European Union (EU) have agreed to cap their levels of greenhouse gas emissions to an average of 94.8% of their 1990 emissions by the period 2008-12. Theorists have long pointed out that, due to the lack of a strong enforcement mechanism, the Kyoto Protocol is unlikely to solve the prisoner's dilemma. Others have pointed out that even voluntary IEAs can be effective and better than none (Ringquist and Kostadinova, 2005). IEAs such as the Montreal or Helsinki Protocol have been scrutinized, with inconclusive results on their effectiveness to bring emissions down (see e.g. Finus and Tjøtta, 2003; Murdoch et al., 2003;

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* This chapter is based on joint work with Gabriel Felbermayr. It is a revised version of ifo Working Paper No. 102, 2011.

[1] Gerber and Wichardt (2009) propose a mechanism that induces compliance even in the absence of a central institution empowered to enforce the agreement.

Ringquist and Kostadinova, 2005; Aakvik and Tjøtta, 2011). So far, there is no empirical evidence on the Kyoto Protocol's impact on countries' emissions. This paper tries to fill this gap.

In 2011, many countries are still far from achieving their promised CO_2 emission reductions. Our main argument is that failure to meet a promised target does not imply that Kyoto has been completely unsuccessful in bringing down emissions relative to the counterfactual situation of "Non-Kyoto". We propose an instrumental variables (IV) strategy to deal with the endogeneity of Kyoto commitment and to ex-post evaluate Kyoto's effect on emissions. Using fixed-effects estimation, we find robust evidence that Kyoto commitment reduces CO_2 emissions by some 10% on average. To corroborate this surprisingly high effect, we investigate possible channels through which Kyoto may have affected CO_2 emissions. We identify effects on countries' energy and electricity mix, fuel prices, energy and electricity use. We believe that these results are potentially important for negotiations about future climate deals. They imply that even a highly imperfect international climate deal may be better than no deal at all.

An insight from the public economics literature is that selection into IEAs is most likely *non-random*. GDP per capita, initial emissions, development status and political freedom are important determinants of IEA membership, see e.g. Murdoch and Sandler (1997); Beron et al. (2003). York (2005) highlights the importance of demographic change for a country's decision to ratify the Kyoto Protocol. Also, historical country characteristics play a large role in countries' decisions to ratify a treaty. Unfortunately, these determinants cannot be excluded from the second-stage regression that strives to explain emissions and therefore fail to be valid instruments.

We show that countries' membership in the *International Criminal Court (ICC)*, based in The Hague, Netherlands, correlates robustly to countries' commitments under the Kyoto Protocol. The Rome Statute, governing the ICC, was adopted in 1998 and ratified by the necessary quorum of 60 countries by the end of 2002. The Kyoto Protocol was negotiated one year earlier, and has been ratified by countries starting from 2001. The timing of the two multilateral initiatives coincides nicely. The two treaties also posed similar domestic policy issues. For example, commentators such as Groves (2009) describe both the Kyoto Protocol and the Rome Statute as threats to the sovereignty of the U.S., who has ratified neither. In terms of content, in contrast, the two treaties have nothing in common. ICC membership has nothing to do with environmental outcome variables such as the level of CO_2 emissions;

nor is it likely to directly cause those variables. These features make ICC membership and its spatial lag (i.e., other countries' membership dummies, weighted by their distance and size) candidate instruments for Kyoto commitment.

We embed the IV strategy into a model that explains CO_2 emissions. We use variables capturing economic development, population growth, political preferences and trade openness to model emissions. To account for relevant time-invariant country features such as endowments with natural resources, industrial structure, geographical position or climate as well as for unobserved heterogeneity we use a fixed-effects estimation strategy as in Ringquist and Kostadinova (2005) or Aakvik and Tjøtta (2011).

Our second stage model is related to the carbon *Kuznets curve* literature which stresses a dynamic relationship between development (measured by GDP per capita) and CO_2 emissions per capita.[2] The purpose of those papers is to estimate the "turning point" beyond which further GDP per capita growth lowers emissions per capita. The evidence for the existence on such a turning point is mixed; see, e.g., Azomahou et al. (2006) for a skeptical view. More closely related to our work is a study by Grunewald and Martínez-Zarzoso (2009) who include a dummy for Kyoto ratification in the carbon Kuznets curve framework. In a panel of 123 countries over the period 1974 to 2004, the authors find that Kyoto commitment reduces CO_2 emissions. However, they do not instrument Kyoto ratification.

A key conceptual question is why a voluntary, non-enforceable agreement such as Kyoto should matter at all. The public economics literature discusses mechanisms through which voluntary IEAs could be effective, see e.g. Ringquist and Kostadinova (2005) for a survey.[3] First, IEAs could induce scientific research, raise environmental awareness and change preferences, thereby affecting technological options and the regulatory environment. Second, non-compliance with a voluntary IEA could entrain a loss of trust in other international policy arenas like international lending so that countries find it optimal to comply (Rose and Spiegel, 2009). Third, public pressure creates an informal enforcement mechanism to mitigate emissions. Under the Kyoto Protocol, participating countries have the obligation to monitor and report emissions to the United Nations Framework Convention on Climate Change

[2] The environmental Kuznets curve literature started as a purely empirical relationship, see e.g. Grossman and Krueger (1995) and Holtz-Eakin and Selden (1995) for early contributions. Subsequently, theoretical explanations have been put forward (for example Andreoni and Levinson, 2001 or Brock and Taylor, 2010). For a survey see Dinda (2004) or Galeotti et al. (2006).

[3] Parallels are found in other areas. In a recent study, the IMF (2009) finds that voluntary or unenforceable *fiscal rules* have had significant effects on countries' debt levels. Countries may have dramatically fallen short from proclaimed targets, but this does not imply that the rules have not had any effect.

(UNFCCC), which summarizes emission reduction achievements in annual reports. So, the lack of formal sanctions has certainly hampered the Protocol, but it does not automatically imply that Kyoto has not added incentives to engage in mitigation policies with the objective to save emissions.

The rest of the paper proceeds as follows. Section 1.2 describes empirical challenges and our strategy and motivates our instrument. Section 1.3 presents results from first-stage regressions. Section 1.4 presents our core results; it investigates how Kyoto has affected CO_2 emissions and presents a host of robustness checks. Section 1.5 turns to the channels through which Kyoto may have affected emissions. The last section contains concluding remarks. Additional sensitivity results are collected in Appendix A.

1.2 Empirical strategy and data

1.2.1 A model of CO_2 emissions: The second-stage

We are interested in understanding the effect of Kyoto commitments on countries' CO_2 emissions. In Section 1.5, we will also investigate other dependent variables such as the energy mix, fuel prices, or per capita energy use.

Denoting country i's outcome variable of interest at time t by Y_{it}, we want to estimate the parameter β_1 in the following relationship

$$Y_{it} = \beta_0 + \beta_1 Kyoto_{it} + \boldsymbol{\beta}_2 \mathbf{X}'_{it} + \alpha_t + \alpha_i + \varepsilon_{it}. \tag{1.1}$$

In our core exercise, Y_{it} is the log of CO_2 emissions. $\mathbf{X}_{it}$ is a vector of controls that influence emissions. These controls can be divided into three different categories. The first set of controls are economic and demographic determinants of emissions (the log of GDP and its square, the log of population, the log of the share of agriculture, manufacturing and services in GDP, economic openness). We expect that an economically large country has higher emissions, all else equal. Population growth will increase emissions. Countries with a high share of manufacturing will experience higher emissions, whereas a large share of services and agriculture in GDP should be associated with less emissions. The second category includes measures for preferences and policy (the stock of other IEAs, a country's political orientation measured by the chief executive's party affiliation, a dummy variable for WTO membership, and the Polity index).[4] The stock of other IEAs is a proxy for environmental awareness. So we expect a country that has signed up for more international environmental agreements to have lower emissions. Last, $\mathbf{X}_{it}$ contains the spatial lag of Kyoto commitment, i.e. the Kyoto status of other countries weighted with their respective size over distance squared. This measure reflects leakage effects. We expect that countries with large Kyoto countries nearby (i.e. with a larger value of the spatial Kyoto lag) are less prone to competitiveness effects and carbon leakage and thus have higher own emissions. To account for time-invariant country-specific determinants such as endowments of fossil fuels, patterns of comparative advantage, climatic and geographic conditions, or historical features (such as historical emission levels), we add a full set of country dummies α_i. We also include a full set of year dummies α_t to control for the world business cycle or the oil price.

The key independent variable is a Kyoto commitment dummy ($Kyoto_{it}$) that takes the value of one if a country i has ratified the Protocol at time t *and* has thus a cap on domestic CO_2 emissions. It takes the value of zero otherwise:

$$Kyoto_{it} = \begin{cases} 0 & \text{no ratification in } t \\ 1 & \text{ratification and cap in } t \end{cases} .$$

It implies that the Protocol starts to matter for committed Annex B countries once ratification through the parliament has occurred. Studies evaluating the treatment effects of IEAs such as the Montreal, Helsinki or Oslo Protocol take a similar stance and also use ratification as the decisive treatment date, see e.g. Ringquist and Kostadinova (2005).

1.2.2 Econometric issues

Our default strategy to eliminate the country-specific unobserved heterogeneity α_i from equation (1.1) is fixed-effects (FE) estimation on yearly data. It is fairly standard and used in much of the empirical Kuznets curve and IEA treatment effects literature. Yet, this strategy is subject to some criticism. Bertrand et al. (2004) argue that standard errors of treatment effects in fixed-effects estimation are inconsistent and the estimator's standard deviation is underestimated if the outcome and treatment variable are both serially correlated over time.

[4] We do not include an EU dummy. In our fixed-effects approach, only the change in EU status matters. In our time period, with the exception of Malta and Cyprus new EU members are economies in transition (EIT) which have experienced more changes than just the one in EU status. So, including an EU dummy would not be informative about the effect of becoming an EU member country. However, in a robustness check we exclude transition countries from the sample.

This might lead to an overrejection of the Null of no effect. The authors suggest to apply a fixed-effects estimator to the *pre- and post-treatment averages* ("long fixed-effects estimator") to cure the spurious correlation problem. Although there has been some heterogeneity in the timing of Kyoto's ratification across countries, most countries have ratified between 2001 and 2003. So we assume treatment takes place in this period, but conduct robustness checks pertaining to this choice. The pre- and post-treatment windows of 1997-2000 and 2004-2007 are chosen to be symmetric around this treatment window.[5] Whereas the within-transformation on yearly data is subject to the Bertrand et al. (2004) critique, it has the advantage that it does not require assumptions about a treatment window. Moreover, the number of useable observations is about 10 times larger than in the long FE model. For these reasons we show results for both methods.

The key empirical challenge consists in finding a valid instrument for Kyoto commitment. Ordinary least squares (OLS) estimates could be biased for two reasons: (i) reverse causality, and (ii) omitted variables. First, countries that are on a negative emission trajectory due to prior investments in green technology or sectoral restructuring toward services might be more willing to self-select into Kyoto. This *reverse causality* results in a bias of the OLS estimate. Second, preferences for environmental quality, expected damage from global warming or expected negative competitiveness effects may vary differently across countries over time, thus creating *omitted variables* bias. Instrumenting the Kyoto status can cure these biases. In the following, we discuss two instruments: membership to the International Criminal Court and its spatial lag.

1.2.3 Instrumental variables strategy

In order to identify the causal effect of Kyoto on Y_{it} (emissions and other outcomes), we require instruments $\mathbf{Z}_{it}$ that (i) correlate with $Kyoto_{it}$ and that (ii) are uncorrelated with the error term in equation (1.1). Condition (ii) implies that $\mathbf{Z}_{it}$ must not have an effect on Y_{it} except through $Kyoto_{it}$ and that $\mathbf{Z}_{it}$ has explanatory power for $Kyoto_{it}$ conditional on the vector of controls $\mathbf{X}_{it}$ included into the equation of interest (1.1). An IV approach then exploits the exogenous variation in instruments for causal inference. Thus, the first-stage model is

$$Kyoto_{it} = \alpha + \boldsymbol{\gamma}\mathbf{X}'_{it} + \boldsymbol{\zeta}\mathbf{Z}'_{it} + \nu_i + \nu_t + \upsilon_{it}, \tag{1.2}$$

[5] Note that Russia and Ukraine have ratified Kyoto in 2004 and Belarus in 2005 but are treated as Kyoto country. Australia and Croatia have ratified in 2007 and are assigned to the control group.

where ν_i is a country-specific fixed effect and ν_t a year dummy.

In this paper, we propose countries' membership to the *International Criminal Court* as an instrument for Kyoto commitment. The ICC, headquartered in The Hague, Netherlands, *"[...] is the first permanent, treaty based, international criminal court established to help end impunity for the perpetrators of the most serious crimes of concern to the international community."*[6] The ICC is an independent international organization. The Rome Statute governing the ICC was finally signed in 1998 and ratified, until December 2010, by 114 countries. 34 countries, including the U.S., India, and China, have decided not to ratify the Statute.

We hypothesize that membership to the ICC and the Kyoto Protocol are closely linked. The idea behind our instrumentation strategy is that both treaties reflect a country's preferences for international policy initiatives. Some countries are more willing than others to give up national sovereignty and subject themselves to an international organization. And indeed, Groves (2009) argues that both the Kyoto Protocol and the Rome Statute constitute a threat to U.S. sovereignty.[7] In Section 1.3 we test this link – i.e. condition (i) – and estimate the first-stage equation (1.2) with a linear probability model.[8]

The exclusion restriction (ii) cannot be tested formally. It requires that ICC involvement of a country is not caused by carbon emissions and that it does not directly affect the outcome variables, neither. That these requirements are met appears plausible enough. The exclusion restriction also requires that the instrument is not correlated with the error term. One concern may be that altruistic or cooperative countries have lower emissions and a higher likelihood of ICC ratification. However, our fixed-effects estimation strategy deals with country-specific heterogeneity. Note that this strategy makes the exclusion restriction more likely to hold. Moreover, we include the stock of other IEAs that captures how a country's environmental preferences evolve over time. Additionally, we include variables related to the political orientation of governments, the Polity index and a WTO dummy to capture any political preferences that may be related to the ratification of the ICC and Kyoto and also matter for emissions. Changes in the production structure of a country are taken into account in the second stage. Components left in the error term of the second

[6] `http://www.icc-cpi.int/Menus/ICC/About+the+Court/`

[7] Similarly, Mike Huckabee (2007), former Governor of Arkansas, argues that the Kyoto Protocol *"would have given foreign nations the power to impose standards on us."* China expressed similar concerns in the Copenhagen climate change negotiations.

[8] The correlation also holds in cross-sectional logit or probit models, or in a panel logit framework.

stage equation are certainly *unobservable* changes in technological change or changes in comparative advantage which influence emissions as well as Kyoto commitment. However, this is unrelated to ICC membership. With these considerations in mind, we argue that we can exclude ICC_{it} from the second-stage equation (1.1).[9]

1.2.4 Data

We briefly describe the data used in our empirical exercise. CO_2 emissions for the years 1997-2007 for 133 countries are from the World Bank's World Development Indicators (WDI) 2010. They comprise emissions due to the burning of fossil fuels and the manufacture of cement and include carbon dioxide emissions produced during consumption of solid, liquid, and gas fuels and gas flaring. Data on diesel and gasoline pump prices[10], electricity and energy use per capita and the shares of different energy sources in energy and electricity production were also compiled from WDI 2010.

The Kyoto dummy is constructed from the UNFCCC homepage. Kyoto's spatial lag is constructed as weighted average over foreign Kyoto status with population over squared bilateral distance used as weight. Distance data come from CEPII (Paris). Data on ICC membership stem from the UN Treaty Series database. The ICC dummy takes value of one if a country has ratified the Rome Statute governing the International Criminal Court and value zero otherwise. The spatial lag of ICC membership is the 'average' ICC membership of other countries (all other countries' membership dummies weighted by population over distance squared, and averaged).[11]

GDP, population and openness data stem from the Penn World Table 6.3. Openness is the usual ratio of exports plus imports over GDP (measured in current prices). The shares of manufacturing, agriculture and services (value added) in GDP are obtained from the WDI 2010. The stock of other IEAs was calculated using the International Environmental Agreements Database Project.[12] It gives the number of IEAs other than Kyoto a country has ratified or accepted up to a given year. The chief executive's party orientation is from the World Bank's Database on Political Institutions (DPI) 2010, and codes the govern-

[9] Other international treaties, such as those governing the WTO or international environmental questions cannot be easily excluded since they will affect emissions directly either through "green" preferences of voters and consumers, or through trade policy.

[10] Pump prices of diesel and gasoline are only available every other year from 1998-2006.

[11] The exact calculation of the spatial lag does not make a significant difference.

[12] `http://iea.uoregon.edu/`

ment's orientation with respect to economic policy as 1 (right-wing, conservative, Christian democratic), 2 (centrist) or 3 (left-wing, socialist, social democratic, or communist). A zero indicates cases where non of the previous categories fit or the party does not focus on economic issues. The WTO dummy takes a value of one if a country is member to the WTO and zero otherwise, and was compiled from the WTO homepage. The Polity Index was obtained from the Center of Systemic Peace's Polity IV Project Database. The index classifies countries according to their political authority characteristics and ranges from -10 to 10, where -10 is a perfectly autocratic regime and 10 is a full democracy. Table 1.1 lists summary statistics and sources.[13]

1.3 Selection into Kyoto: The role of ICC membership

In our sample of 133 independent countries, 32 have commitments under the Kyoto Protocol.[14] Within the group of countries that had commitments as of 2007, there is some variation as to the timing of national ratification. The first countries to ratify a commitment were Romania and Czech Republic (in 2001), 27 countries ratified in 2002, Lithuania (2003), Ukraine (2004), Belarus (2005) and finally Australia and Croatia (2007) followed. To examine what drives Kyoto ratification and verify condition (i), we now estimate (1.2) with a *linear probability model* for Kyoto commitments for the time span 1997-2007, applying the same methods (FE and long FE estimation) than on (1.1).

Table 1.2 presents results on the first-stage regressions. Columns (1) to (4) report fixed-effects estimations on levels of yearly data. We adjust the variance-covariance matrix for heteroskedasticity and for clustering of standard errors within countries (Stock and Watson, 2008). Column (1) shows that ratification of the Rome Statute governing the International Criminal Court correlates strongly with ratification of the Kyoto Protocol. The estimated coefficient of 0.19 implies that ICC ratification increases the odds of Kyoto ratification by 19 percentage points. The estimate is different from zero at the 1% level of statistical significance and explains about 18% of the variance in the Kyoto dummy. In column (2), adding the

[13] Additional information on Kyoto status, average emission growth from pre- to post-treatment period and sample info (rich, large, OPEC, transition country) is given in Appendix A, Table A.1.

[14] Five committed Kyoto countries (Iceland, Liechtenstein, Luxembourg, Russia and Switzerland) are not included due to data availability.

Table 1.1: Summary statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max	Source
Dependent variables, 2nd stage						
Ln CO_2 emissions (Mt)	1,456	9.78	2.28	4.19	15.69	(a)
Diesel pump price (USD/l)	622	0.58	0.33	0.01	1.73	(a)
Gasoline pump price (USD/l)	622	0.73	0.35	0.02	1.90	(a)
Renewables, share in energy use	1,209	22.64	27.45	0.00	93.77	(a)
Alternative energy, share in electricity production	1,209	2.52	5.11	0.00	29.26	(a)
Fossil fuels, share in energy use	1,209	67.82	26.81	5.32	102.43	(a)
Coal, share in electricity prod.	1,209	18.04	27.11	0.00	99.46	(a)
Ln electricity use (kWh/capita)	1,198	7.29	1.49	3.02	10.15	(a)
Ln energy use (kg oil eq./capita)	1,209	7.17	0.99	4.83	9.38	(a)
Kyoto variables						
Kyoto ratification (0,1)	1,456	0.12	0.33	0.00	1.00	(b)
Kyoto, spatial lag	1,456	0.28	0.97	0.00	13.29	(b)
Kyoto stringency (0,1,2)	1,456	0.20	0.57	0.00	2.00	(b)
Instruments						
ICC ratification (0,1)	1,456	0.33	0.47	0.00	1.00	(c)
ICC, spatial lag	1,456	0.12	0.38	0.00	3.46	(c)
Additional controls						
Ln GDP	1,456	17.95	1.91	13.23	23.28	(d)
Ln GDP, squared	1,456	325.94	69.64	175.05	542.06	(d)
Ln GDP per capita	1,456	8.67	1.12	5.73	10.85	(d)
Ln population	1,456	9.28	1.51	6.04	14.09	(d)
Ln manufacturing (in % of GDP)	1,421	2.62	0.56	0.35	3.79	(a)
Ln agriculture (in % of GDP)	1,440	2.30	1.11	-2.63	4.36	(a)
Ln service (in % of GDP)	1,437	3.93	0.35	1.01	4.40	(a)
Ln stock of other IEA	1,456	3.28	0.59	1.79	4.84	(e)
Chief executive party orientation	1,456	0.17	0.09	0.10	0.30	(f)
Openness (current price)	1,456	0.86	0.48	0.05	4.57	(d)
WTO dummy (0,1)	1,456	0.82	0.39	0.00	1.00	(g)
EU dummy (0,1)	1,456	0.12	0.33	0.00	1.00	(h)
Polity index	1,456	3.66	6.41	-10.00	10.00	(i)

Note: The table shows summary statistics for variables over the period 1997-2007. Sources: (a) World Bank WDI 2010, (b) www.unfccc.int, (c) UN Treaty Series database, (d) PWT 6.3, (e) iea.uoregon.edu, (f) World Bank DPI 2010 (series: execrlc), (g) www.wto.org, (h) europa.eu, (i) www.systemicpeace.org.

spatial lag of ICC ratification, i.e., the ratification of ICC by spatially close countries, further increases the share of variance explained to 47% in the FE model.[15]

[15] We have also experimented with other international agreements such as the Comprehensive Nuclear-Test-Ban Treaty or the Anti-Personnel Land Mines Convention. Ratification of those texts also tends to make Kyoto commitments more likely; however, the effects are weaker and less statistically significant.

Table 1.2: First-stage regressions: Explaining Kyoto commitment

Dependent variable: Kyoto commitment (0,1)						
Method:	FE				long FE	
	(1)	(2)	(3)	(4)	(5)	(6)
Excluded instruments						
ICC (0,1)	0.19***	0.11***	0.10***	0.09***	0.25***	0.24***
	(0.05)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)
ICC, spatial lag		0.51***	0.41***	0.41***	0.37***	0.39***
		(0.09)	(0.08)	(0.08)	(0.08)	(0.09)
Other controls						
Kyoto, spatial lag			-0.01	-0.01	-0.01	-0.01
			(0.00)	(0.00)	(0.01)	(0.01)
Ln GDP			-0.94*		-2.46***	
			(0.49)		(0.68)	
Ln GDP, squared			0.02*		0.07***	
			(0.01)		(0.02)	
Ln population			-1.72***	-1.94***	-1.31***	-1.25***
			(0.36)	(0.39)	(0.30)	(0.30)
Ln manufacturing (% of GDP)			-0.01	-0.01	-0.02	-0.09
			(0.04)	(0.04)	(0.07)	(0.07)
Ln agriculture (% of GDP)			-0.14**	-0.14**	-0.29***	-0.31***
			(0.06)	(0.06)	(0.09)	(0.09)
Ln services (% of GDP)			-0.01	-0.01	0.15	0.23*
			(0.07)	(0.07)	(0.11)	(0.12)
Ln stock of other MEA			-0.02	-0.05	0.27**	0.17
			(0.08)	(0.08)	(0.11)	(0.11)
Governm. orientation (0.1,0.2,0.3)			0.03	0.04	0.07	0.06
			(0.18)	(0.18)	(0.40)	(0.42)
Openness, (Exp+Imp)/GDP			-0.22***	-0.20***	-0.27**	-0.12
			(0.06)	(0.07)	(0.12)	(0.11)
WTO (0,1)			-0.15***	-0.15***	-0.28***	-0.27***
			(0.06)	(0.06)	(0.09)	(0.09)
Polity (-1 to 1)			-0.00	-0.00	-0.02**	-0.02**
			(0.00)	(0.00)	(0.01)	(0.01)
Ln GDP per capita				-0.12		0.02
				(0.08)		(0.09)
No. of observations	1,456	1,456	1,418	1,418	266	266
adj. R^2	0.18	0.47	0.56	0.56	0.34	0.29
F-stat	4.76	7.37	8.14	7.86	12.05	11.00

Note: Linear probability models. Sample: 133 countries. Heteroskedasticity-robust standard errors (clustered at country-level) in parentheses. * p<0.1, ** p<0.05, *** p<0.01. Year dummies and constant included (not shown).

Column (3) adds the vector of controls $\mathbf{X}_{it}$ which also features in the second-stage regressions. The spatial lag of Kyoto ratification adds no explanatory power given the spatial lag of ICC and the other covariates.[16] The log of GDP enters negatively, its square positively. This signals that economic growth deters countries from ratifying their Kyoto commitment, but the effect eventually levels off. Population size has a large negative effect on the odds of Kyoto ratification. Since we identify all effects in Table 1.2 by time variation at the country-level, our results suggest that countries with higher own population growth are less likely to have commitments. This finding is in line with York (2005) who underlines the importance of demographic factors for the ratification of the Kyoto treaty. A country's industrial structure matters to a certain degree for Kyoto commitment. Countries with a higher share of agriculture in GDP are less inclined to ratify the Kyoto Protocol. The logs of the manufacturing and services shares are not relevant for Kyoto commitments.

We include two variables to proxy for *green preferences*. The first variable, the log stock of other (than Kyoto) IEAs ratified by a country is expected to affect the likelihood of commitment positively, but does not show up significantly in the analysis. The second variable is the country's chief executive party's political orientation. One would think that left-leaning governments are more likely to accept commitments, but this does not show up in our regression.

The next two variables measure openness to international trade: exports plus imports over GDP and the WTO dummy. Both variables correlate negatively with Kyoto ratification. On average, doubling openness makes ratification less likely by 22 percentage points. Note that 15 countries become WTO members in the sample period;[17] with the exception of the three Baltic states, none of these countries has commitments under Kyoto.

The principle of common but differentiated responsibilities stipulates that richer countries should bear the largest share of adjustment. Indeed, in a pure cross-section, GDP per capita is the key determinant of Kyoto commitment. However, in our model, which identifies parameters by within-variation only, GDP per capita is not significant (column (4)). This implies that the growth rate of GDP per capita (in contrast to the level) is not important for the ratification decision. Column (3) corresponds to our first-stage regression. Compared to model (4), it includes the log of GDP and the square thereof (these variables are deemed

[16] Note that a positive and statistically significant effect is obtained when the spatial lag of ICC is not included into the equation.

[17] These countries are Albania, Armenia, Cambodia, China, Croatia, Estonia, Georgia, Jordan, Lithuania, Latvia, Moldova, Macedonia, Nepal, Saudi Arabia and Vietnam.

important in the Kuznets curve literature), but drops ln GDP per capita (which would be collinear).

As a robustness check, columns (5) and (6) apply the long fixed-effects model, where the *change* of Kyoto status (i.e., ratification) is regressed on instruments and other controls. Comfortingly, results are very similar to the FE model. Summarizing, we have used a fixed-effects approach on yearly data to show that ICC membership and its spatial lag robustly correlate to ratification of Kyoto commitments. So, the two variables fulfill condition (i) and make up good instruments for a country's Kyoto status. In the next section, we will use the instruments to estimate Kyoto's effects on emissions.

1.4 Kyoto's effect on emissions

In this section we present our core results about the effect of Kyoto commitment on CO_2 emissions. First, we present the evidence in a picture, then we employ more formal econometric fixed-effects models. Finally, we turn to several robustness checks.

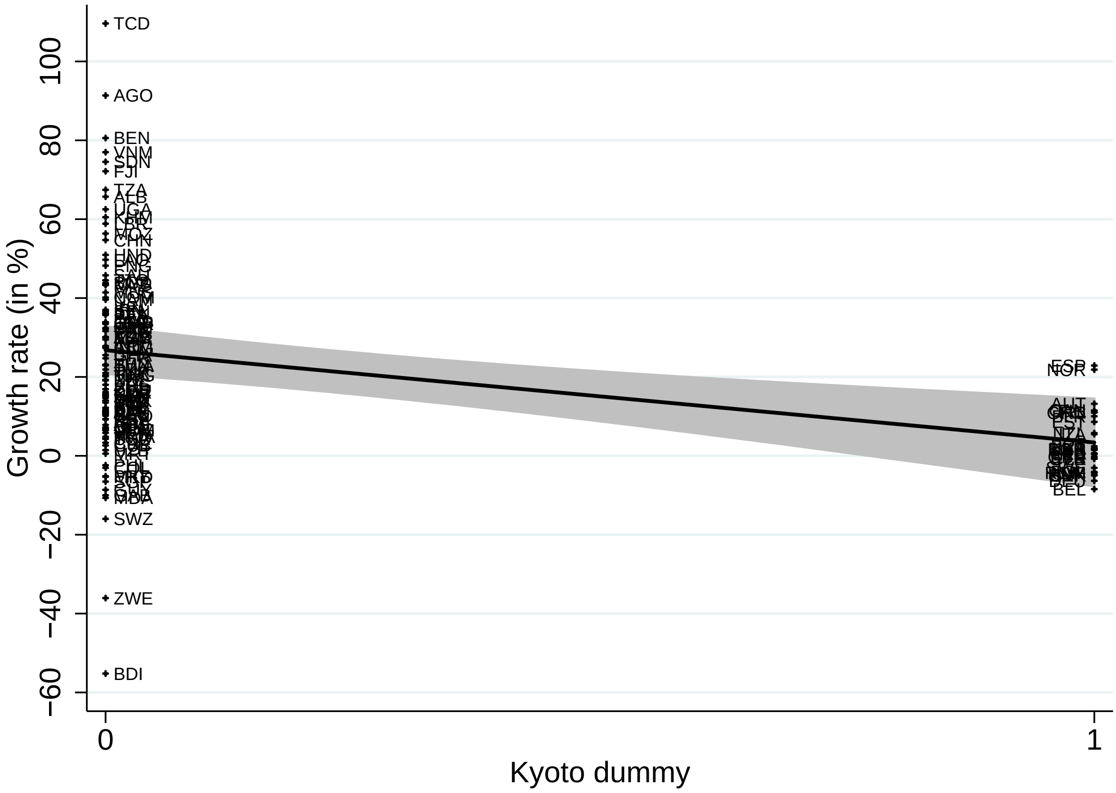
1.4.1 Graphical inspection of Kyoto's effects

Figure 1.1 shows the change in the log of CO_2 emissions over two groups of countries: countries who end up with emission caps, and countries who do not. The changes are computed over period averages 1997-2000 (before the first country has ratified the Protocol) and 2004-2007 (following the ratification of Russia and Ukraine in 2004). Ratification occurred later only in Belarus (2005), Australia and Croatia (2007).[18] Between the two periods, emissions have increased on average by 27% in the group of non-committed countries while they have increased on average by 3.4% in the group of committed countries. In both groups, there is substantial variation. Emissions fell substantially in some developing countries affected by civil war (such as Burundi), and increased strongly in countries recovering from crises (such as Chad or Angola). In the group of committed countries, Norway and Spain have increased emissions by more than 20%, while they fell by roughly 8% in Belgium or Germany. Observations cluster strongly around the means (marked by the end points of the line). Fitting a linear regression $\Delta \ln EM_{it} = const. + \beta Kyoto_{it} + \upsilon_{it}$ with $t \in \{\text{pre,post}\}$

[18] Note that Belarus is considered as treated, while Australia and Croatia are put in the control group.

into the cloud reveals a coefficient of -0.23, statistically different from zero at the one percent level of significance.[19]

Figure 1.1: Differences in emission growth rates in Kyoto and non-Kyoto countries



Note: The figure shows a scatter plot of CO_2 emission growth rates (i.e. $\Delta \ln EM_{it}$) of non-Kyoto (0) and Kyoto countries (1), where $t = 0$ is the pre- (1997-2000) and $t = 1$ is the post-treatment (2004-2007) average. The graph also shows a fitted linear regression line with 95% (heteroskedasticity-robust) confidence interval. Regression coefficient and robust standard error (in parentheses): $-0.23^{***}(0.04)$. $N = 133$.

1.4.2 Regression results

Figure 1.1 does not control for the effects of time-varying controls and self-selection into the Kyoto Protocol. Therefore, we turn to more elaborate estimation techniques. Table 1.3 reports our benchmark results. Column (1) uses ratification of Kyoto commitment to explain variation in yearly emissions.[20] The Kyoto dummy's coefficient is highly statistically

[19] The result may be driven by outliers. Using robust regression techniques that downweight outliers yield negative significant results (applying the usual tuning weight of 7), too, but the estimated coefficient is typically smaller, here -0.19.

[20] In principle, this replicates Figure 1.1 but with yearly data.

significant and suggests that Kyoto commitment reduces emissions by about 17%. The simple model explains about 19% of the variation in emission growth across countries.

Column (2) turns to a more comprehensive model. Adding the control vector $\mathbf{X}_{it}$ more than halves the Kyoto effect and also weakens statistical significance to the 5% level.[21] The explanatory power of the model rises to about 42%. We find that a one-standard-deviation increase in the spatial lag of Kyoto ratification (more close-by countries ratify) increases emissions by 2%.[22] This finding is consistent with the carbon leakage hypothesis: non-committed countries increase emissions as they step up exports of carbon-intensive goods to committed countries. The coefficients on log GDP and its square are positive and negative, respectively, and not statistically different from zero. Jointly, however, they are highly signficant (the p-value of the Wald Chi $^2(1)$ test statistic of joint significance is 0.00), so this is a problem of collinearity. Dropping the GDP squared term yields a coefficient on the log of GDP of 0.56. So, a 1% increase of GDP is associated with 0.56% higher CO_2 emissions. Population increases emissions; the elasticity is highly significant and statistically not-distinguishable from unity.[23] This result is in line with the literature, see e.g. Cole and Neumayer (2004). The share of manufacturing in GDP has a positive though not statistically significant effect on emissions, while a higher share of agriculture or services in GDP reduces CO_2 emissions. The chief executive's party (government) orientation has a positive effect on emissions: left-leaning governments are associated with higher emissions. Openness appears to lower emissions, but the effect is not statistically significant. The WTO dummy is also insignificant. Political freedom (the polity variable) does not affect emissions either.

Column (3) applies our IV strategy, using both the ICC dummy and its spatial lag as instruments. This strategy turns out successful: The weak identification test yields a Kleibergen-Paap-Wald F-statistic of 19.09. This is comfortingly above the canonical 10% level suggested by Staiger and Stock (1997) and gives a maximal IV size bias of 15% as tabulated by Stock and Yogo (2005). The overidentification test yields a Hansen J-statistic of 0.59 with the associated Chi$^2(1)$ p-value of 0.44. Hence, the Null that the instruments are uncorrelated with the error term is not rejected and the instruments appear valid. Results on the controls differ only slightly across the OLS and IV models. Instrumentation slightly increases the estimated Kyoto effect; this feature is present in most of our IV regressions. The

[21] In the discussion paper version of this paper, we have used a larger set of controls and obtained very similar results.

[22] $0.97 \times 0.01 = 0.01$.

[23] The p-value of the Wald Chi$^2(1)$ test statistic is 0.66.

Table 1.3: Second-stage regressions: The effect of Kyoto on CO_2 emissions

Dependent variable: Ln CO_2 emissions					
Method:	FE-OLS	FE-OLS	FE-IV	long FE-OLS	long FE-IV
	(1)	(2)	(3)	(4)	(5)
Kyoto (0,1)	-0.17***	-0.06**	-0.10**	-0.09**	-0.12*
	(0.03)	(0.02)	(0.05)	(0.04)	(0.07)
Kyoto, spatial lag		0.01**	0.02***	0.03***	0.03***
		(0.01)	(0.00)	(0.01)	(0.01)
Ln GDP		0.65	0.62	1.13**	1.02*
		(0.51)	(0.50)	(0.54)	(0.56)
Ln GDP, squared		-0.00	-0.00	-0.02	-0.01
		(0.01)	(0.01)	(0.01)	(0.02)
Ln population		1.10***	0.95***	1.12***	1.05***
		(0.23)	(0.26)	(0.26)	(0.29)
Ln manufacturing (% of GDP)		0.12	0.12	0.25***	0.25***
		(0.09)	(0.09)	(0.09)	(0.09)
Ln agriculture (% of GDP)		-0.09**	-0.10**	-0.26***	-0.27***
		(0.04)	(0.04)	(0.08)	(0.09)
Ln services (% of GDP)		-0.10	-0.10	-0.26*	-0.26*
		(0.09)	(0.09)	(0.15)	(0.15)
Ln stock of other MEA		-0.18***	-0.18***	-0.23**	-0.21*
		(0.07)	(0.07)	(0.10)	(0.11)
Government orientation (0.1,0.2,0.3)		0.15***	0.15**	0.29**	0.28*
		(0.06)	(0.06)	(0.15)	(0.15)
Openness, (Exp+Imp)/GDP		-0.01	-0.02	-0.16	-0.16
		(0.07)	(0.07)	(0.12)	(0.12)
WTO (0,1)		0.01	0.00	0.04	0.03
		(0.04)	(0.04)	(0.05)	(0.06)
Polity (-1 to 1)		-0.00	-0.00	-0.01	-0.01
		(0.00)	(0.00)	(0.01)	(0.01)
No. of observations	1,456	1,418	1,418	266	266
No. of countries	133	133	133	133	133
First-stage diagnostics					
Hansen-Sargan J-stat (p-value)			0.44		0.71
Weak-ID test (F-stat)			19.09		37.70
Second-stage diagnostics					
adj. R^2	0.19	0.42	0.42	0.49	0.48
F-stat	13.20	15.31	17.44	21.40	23.09

Note: Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. * p<0.1,** p<0.05, *** p<0.01. Instruments for Kyoto variable: membership in International Criminal Court and its spatial lag. All regressions use a comprehensive set of year dummies (not reported).

estimated coefficient implies that Kyoto commitment caused a reduction of CO_2 emissions of 10%.

To meet the Bertrand et al. (2004) critique of spurious correlation due to serial correlation in treatment and outcome variable, we also use the long fixed-effects estimator. The long FE estimates on average pre- and post-treatment data yield very similar results than the yearly fixed-effects model. The sign and significance of covariates is mostly unaffected. In the long FE model, the positive effect of GDP growth on emissions is now significant, and also the industrial structure matters for emissions. First-stage diagnostics signal instrument validity. The Kyoto coefficients obtained in both the OLS model in column (5) and the instrumented one in column (6) are slightly larger, but estimated at a somewhat smaller degree of precision. Yet, the finding of a negative effect of Kyoto commitment on emissions is not just spurious correlation.

1.4.3 Robustness checks

Table 1.4 summarizes robustness checks on our IV regressions.[24] Panel (A) investigates different samples of countries, panel (B) employs alternative IV strategies, panel (C) uses a different definition of the Kyoto variable. Panel (A) through (C) use FE models on yearly data. Panel (D) uses different definitions of the treatment window for long fixed-effects estimations.

Column (A1) excludes economies in transition (EIT) from the sample. The IV strategy remains valid. The effect of Kyoto remains negative and of similar size than in earlier regressions. However, the effect is no longer statistically significant at the usual levels (t-value of 1.4). Yet, results are broadly robust to excluding OPEC countries in column (A2) or focusing on the 50% richest countries (in terms of per capita income as of 2007) in column (A3).[25] Limiting attention to countries with more than 5 million inhabitants in column (A4) leaves the magnitude of the Kyoto effect unchanged, but it is not statistically significant. And in this case the IV strategy is not successfull; instrument endogeneity is rejected at the 7% level.

[24] Full regression results are in Appendix A, Table A.2.

[25] The F-stat of 8.3 in column (A3) signals a possible weak instrument problem. However, limited information maximum likelihood (LIML) estimation, as suggested by Stock and Yogo (2005) for such cases, shows that the point estimate of -0.11 is robust and statistically significant at the 5% level. The maximal LIML size bias is 15%.

Table 1.4: Robustness checks: Summary table IV estimates on CO_2 emissions

	(A) Alternative Samples			
Sample:	w/o EIT (A1)	w/o OPEC (A2)	rich only (A3)	large only (A4)
Kyoto (0,1)	-0.08	-0.10**	-0.11***	-0.09
	(0.06)	(0.05)	(0.04)	(0.06)
No. of observations	1,298	1,354	720	1,007
No. of countries	122	127	67	94
First-stage:				
Over-ID test (p-value)	0.42	0.67	0.10	0.07
Weak-ID test (F-stat)	22.56	18.18	8.30	16.28
Second-stage:				
adj. R^2	0.43	0.41	0.43	0.51

	(B) Alternative IV Strategy		(C) Kyoto definition	(D) Alternative treatment window	
	LIML (B1)	GMM (B2)	Stringency (C)	narrow (D1)	broad (D2)
Kyoto (0,1)	-0.10**	-0.10**	-0.05**	-0.12**	-0.17**
	(0.05)	(0.04)	(0.02)	(0.06)	(0.07)
No. of observations	1,418	1,418	1,418	266	258
No. of countries	133	133	133	133	129
First-stage:					
Over-ID test (p-value)	0.44	0.44	0.41	0.85	0.82
Weak-ID test (F-stat)	19.09	19.09	21.94	34.06	37.47
Second-stage:					
adj. R^2	0.42	0.42	0.42	0.38	0.32

Note: Dependent variable is ln CO_2 emissions. Fixed-effects estimation. In Panel (D) long fixed-effects estimation. Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. * p<0.1,** p<0.05, *** p<0.01. Instruments for Kyoto: membership in ICC and its spatial lag. All regressions use year dummies and additional controls: ln GDP; ln GDP, squared; ln population; ln IEAs; Chief executive party orientation; Openness; WTO dummy; Polity. Full results in Appendix A, Table A.2.

Columns (B1) and (B2) show that the benchmark results of Table 1.3 are robust to using different IV strategies. Using limited information maximum likelihood (LIML) or a two-step generalized method of moments (GMM) estimation yields identical results than employing two-stage least squares. Column (C) suggests that the baseline results are robust to defining Kyoto commitment in a somewhat finer fashion: the Kyoto stringency variable takes the value of zero if a country has no obligations under Kyoto, a value of one if the country has a cap that does not bind (as of 2007) and a value of two if it has a binding cap. The IV strategy continues to work nicely; the Kyoto effect is statistically significant and quantitatively comparable to the baseline estimations.[26]

Panel (D) turns once more to long FE models and asks whether our definition of the treatment window influences the results. Almost 90% of the countries ratified in 2002. So, column (D1) uses only 2002 as treatment year.[27] Column (D2) defines a broad treatment window from 2000-04.[28] The results are robust to these modifications and broadly in line with the earlier long FE results both in terms of magnitude and significance.

Summarizing, we employ our instruments in a second-stage regression that estimates the effect of Kyoto commitment on CO_2 emissions. Our instruments perform very well: the weak identification test yields an F-statistic of at least 16.28 and often substantially higher; the overidentification tests are easily passed. The effect of Kyoto ratification on emissions is robustly negative over all econometric models and survives a host of robustness checks. The effect is economically substantial: Kyoto has caused emissions to fall relative to the counterfactual by about 10%. However, the estimates are not always very precise (though typically different from zero at the 10% to 5% level). In particular, statistical significance suffers when excluding transition countries from the sample.

1.5 Channels: Fuel mix, fuel prices, and electricity use

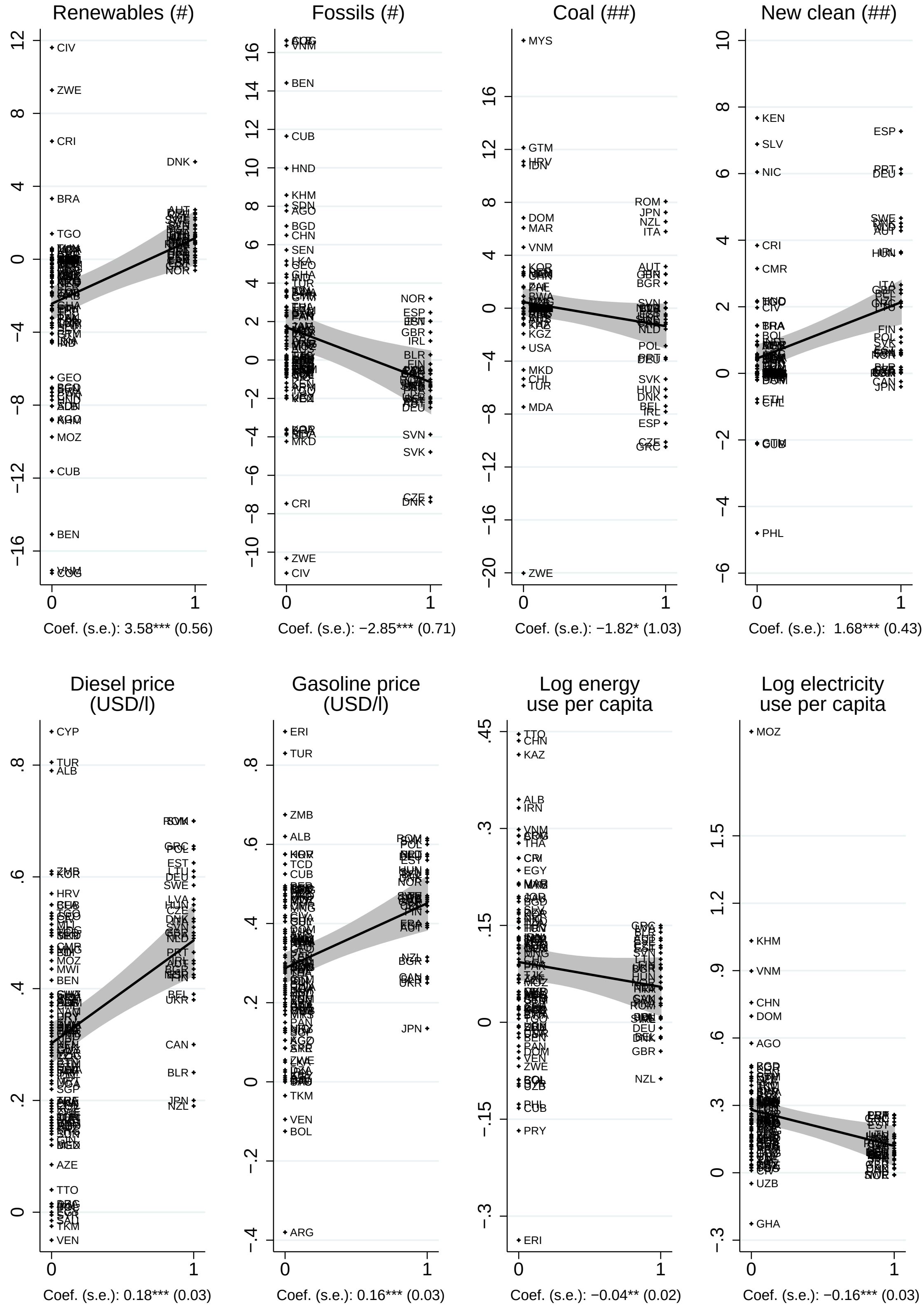
Is the finding of a negative relationship between Kyoto commitment and CO_2 emissions plausible? In the following sections, we present evidence that sheds light on the channels through which Kyoto may have led to lower emissions. We focus on energy and electricity mix, fuel prices as well as energy and electricity use. The selection of channels is certainly exploratory, but supports the findings in Section 1.4. As discussed for the case of CO_2 emissions, the same endogeneity issues arise. For example, countries with a higher share of wind, solar or nuclear energy may find it easier to commit to Kyoto. Therefore, we provide fixed-effects OLS and IV results based on the same models and instrumentation strategy.

[26] Note that the support of the stringency variable is in $[0, 2]$ instead of in $[0, 1]$.

[27] Kyoto countries that ratified after 2002 are still considered to be treated. The only exceptions being Australia and Croatia which ratified in late 2007.

[28] Only Belarus's ratification lies outside the broad treatment window.

Figure 1.2: Changes in energy mix, fuel prices, energy use and Kyoto commitment



Note: The diagrams show scatter plots of changes in respective variables (i.e. $\Delta \ln Y_{it}$) of non-Kyoto (0) and Kyoto countries (1), where $t = 0$ is the pre- (1997-2000) and $t = 1$ is the post-treatment (2004-07) average. The diagrams also show fitted linear regression lines with 95% (heteroskedasticity-robust) confidence interval. Regression coefficients and robust standard errors below diagrams. New clean refers to wind and solar energy. #: share in total energy use. ##: share in electricity production.

Table 1.5: Through which channels does Kyoto operate? Summary table OLS and IV estimates

Panel:	(A) Shares in energy use				(B) Shares in electricity production			
	(A1)	(A2)	(A3)	(A4)	(B1)	(B2)	(B3)	(B4)
Dep. Var.:	Renewables		Fossil fuel		Coal		Alternative energy	
Method:	FE-OLS	FE-IV	FE-OLS	FE-IV	FE-OLS	FE-IV	FE-OLS	FE-IV
Kyoto (0,1)	1.38***	2.41***	-0.67	-2.46**	0.12	-1.43	1.07***	1.66***
	(0.53)	(0.93)	(0.60)	(1.16)	(0.87)	(1.76)	(0.25)	(0.56)
No. of observations	1,180	1,180	1,180	1,180	1,180	1,180	1,180	1,180
No. of countries	110	110	110	110	110	110	110	110
Over-ID test (p-value)		0.68		0.47		0.60		0.63
Weak-ID test (F-stat)		18.80		18.80		18.80		18.80
adj. R^2	0.22	0.21	0.08	0.06	-0.04	-0.05	0.09	0.08

Panel:	(C) Pump prices (USD/l)				(D) Log per capita use of			
	(C1)	(C2)	(C3)	(C4)	(D1)	(D2)	(D3)	(D4)
Dep. Var.:	Diesel fuel		Gasoline		Energy		Electricity	
Method:	FE-OLS	FE-IV	FE-OLS	FE-IV	FE-OLS	FE-IV	FE-OLS	FE-IV
Kyoto (0,1)	0.10***	0.22***	0.13***	0.25***	-0.05***	-0.05*	-0.04	-0.08**
	(0.03)	(0.05)	(0.03)	(0.05)	(0.02)	(0.03)	(0.03)	(0.04)
No. of observations	608	608	608	608	1,180	1,180	1,169	1,169
No. of countries	127	127	127	127	110	110	109	109
Over-ID test (p-value)		0.61		0.32		0.36		0.21
Weak-ID test (F-stat)		20.30		20.30		18.80		18.84
adj. R^2	0.71	0.69	0.60	0.58	0.31	0.31	0.47	0.47

Note: Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Instruments for Kyoto: membership in ICC and its spatial lag. All regressions use year dummies and additional controls: ln GDP; ln GDP, squared; ln population; ln share of manufacturing/services/agriculture; ln IEAs; Chief executive party orientation; Openness; WTO dummy; Polity (not reported). Full results in Appendix A, Table A.3.

1.5.1 Energy mix

We start with a graphical inspection of the effect of Kyoto commitments on countries' energy mix. The upper, left-most diagram in Figure 1.2 plots changes in the share of renewables and waste in total energy use[29] between the pre- and post-treatment period. On average, committed countries increased this share by about one percentage point, while it decreased by about 2.4 percentage points in the sample of non-committed countries. The difference of 3.58 percentage points is statistically significant at the 1% level. The next diagram shows changes in the share of fossil fuel[30] in total energy use. The difference between the group averages is -2.85 percentage points, statistically significant at the 1% level. It appears, thus, that changes in the fossil fuel share correlate negatively with Kyoto commitment.

Table 1.5 provides fixed-effects regression results on yearly data of Kyoto's effect.[31] Columns (A1) and (A2) investigate the renewables share. The OLS effect implies that Kyoto commitment increases that share by 1.38 percentage points. Instrumentation almost doubles the effect of Kyoto to 2.41, statistically significant at the 1% level. ICC membership and its spatial lag continue to be good instruments: both, the overidentification and the weak instrument tests suggest instrument validity. Columns (A3) to (A4) examine the fossil fuel share in energy use. The OLS estimate of Kyoto is -0.67 and not significant. Overall, it is not very easy to explain changes in the share of fossil fuel, probably due to a lack of time-variance in the dependent and the independent variables. The IV point estimate is negative and statistically significant at the 5% level. It implies that Kyoto commitment results in a 2.46 percentage points reduction in the fossil fuel share. So compared to the counterfactual, Kyoto countries rely less on fossil fuel, and thus CO_2-intensive energy sources.

1.5.2 Electricity mix

Next, we turn to changes in the share of coal and new clean forms of energy (i.e. wind and solar) in electricity production. Figure 1.2 suggests a negative relationship of Kyoto commitment and changes in the coal share. The upper, right-most diagram explores the alternative energy share. Interestingly, there are a couple of non-committed countries, where wind and solar energy expanded substantially (Kenya, El Salvador, Nicaragua), but in the

[29] This energy source comprises solid and liquid biomass, biogas, industrial and municipal waste.

[30] Fossil fuel comprises coal, oil, petroleum, and natural gas products.

[31] To save space, Table 1.5 only reports Kyoto estimates and first- and second-stage diagnostics. Full results are delegated to Table A.3 in Appendix A.

sample of non-committed countries as a whole, the share increased by 0.5 percentage points only. In the sample of committed countries, Spain, Portugal, and Germany have considerably increased their shares of new clean energy sources. The group average is 2 percentage points. The growth difference between the two groups is 1.68 percentage points, significant at the 1% level.

Panel B of Table 1.5 shows regression results. Columns (B1) and (B2) analyze the coal share in electricity production. While the analysis in Figure 1.2 suggests a statistically significant negative relationship, controlling for a host of variables and applying our IV strategy, there is no statistical evidence in favor of a negative effect of Kyoto on the coal share. This is despite the fact that our instruments continue to do fine. Columns (B3) and (B4) study the share of alternative energy sources. The IV regression suggests that Kyoto commitments have increased that share by about 1.66 percentage points. It appears that CO_2-free energy sources have gained ground in Kyoto countries' electricity generation, thus lowering CO_2 emissions ceteris paribus.

1.5.3 Fuel prices

We continue with the fuel price channel. The two lower left panels of Figure 1.2 plot the (absolute) changes in fuel prices, expressed in U.S. dollars per liter, across the groups of committed and non-committed countries. Between the pre- and post-treatment period, the diesel price has increased by about 30 cents in the group of non-committed countries and by 49 in committed countries. The differential increase across the two groups is 18 cents. It is significant at the 1% level. A similar picture emerges when looking at gasoline. The average increase in non-committed countries was 29 cents and in committed countries 45. The difference, 16 cents, is again statistically significant at the 1% level.

Panel (C) in Table 1.5 confirms this pattern. Column (C1) reports the uninstrumented fixed-effects estimator. It suggests that Kyoto status is positively associated to the price of diesel fuel. With an adjusted (within) R^2 of 71%, the specification is surprisingly successful in predicting the diesel price. Instrumenting Kyoto commitment leaves the controls virtually unchanged, but the Kyoto effect more than doubles to 22 cents per liter. The IV strategy turns out to work reasonably well: the weak identification test yields an F-statistic of 20.3, and the overidentification test does not reject instrument validity. The results for the price of gasoline (columns (C3) and (C4)) look very similar. Again, the IV estimation yields a point estimate of Kyoto that is about double the OLS one. In summary, Kyoto commitment has increased fuel prices considerably. This may be indirect evidence for higher fuel taxes in Kyoto countries.

1.5.4 Energy and electricity use

Finally, we are interested in the role of Kyoto commitment for the per capita use of energy and electricity. The unconditional long FE exercise is visualized in the two lower panels on the right in Figure 1.2. Energy use per capita (in kg of oil equivalent) has increased by about 4 percentage points less in the group of committed countries. The effect is statistically significant at the 5% level. Turning to the log electricity per capita consumption (in kWh) in the right-most diagram, cross-country variation in growth rates is wider than for energy use per capita. Electricity use per capita has grown in almost all countries. The average growth rate over the two observed periods is 28% in the sample of non-committed countries and 12% in the group of committed countries. The difference, -16 percentage points, is statistically significant at the 1% level.

Panel D in Table 1.5 reports regression results. Column (D1) suggests that Kyoto commitment reduces per capita energy use by 5%. When instrumenting for Kyoto the negative relationship is unchanged, but estimated with less statistical precision. The evidence is different for electricity consumption per capita. Kyoto commitment has no statistically significant effect without instrumentation, but turns out to reduce energy use per capita by about 8% in the IV model.

1.5.5 Robustness checks

Table 1.6 summarizes robustness checks pertaining to the instrumental variables estimates of Table 1.5. We apply long fixed-effects estimation and vary the sample by excluding transition countries or focusing on rich countries only. In general, these modifications do not qualitatively affect the benchmark results. However, the evidence on less energy and electricity use due to Kyoto commitment is not very robust. The IV strategy continues to work with two exceptions.[32] And, for all regressions of the rich countries only sample the F-statistic of the weak identification test is very low. Using LIML estimation remedies this

[32] The overidentification test signals endogenous instruments in the long FE estimation for diesel prices and the FE model on rich countries only for gasoline prices.

Table 1.6: Robustness checks: Channels – Summary table IV estimates

Method/Sample:	long-FE	w/o EIT	rich only	long-FE	w/o EIT	rich only
	(A) Shares in energy use					
Dep. Var.:	Renewables			Fossil fuel		
	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)
Kyoto (0,1)	3.43***	2.85***	2.05**	-3.62***	-2.16*	-0.58
	(1.14)	(1.10)	(0.85)	(1.37)	(1.17)	(1.13)
Observations	220	1,060	701	220	1,060	701
Over-ID test (p-value)	0.32	0.67	0.72	0.18	0.81	0.58
Weak-ID test (F-stat)	36.49	22.07	8.39	36.49	22.07	8.39
adj. R^2	-0.32	0.21	0.05	-0.58	0.07	0.02
	(B) Shares in electricity production					
Dep. Var.:	Alternative energy			Coal		
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)
Kyoto (0,1)	3.06***	1.93***	1.69***	-0.96	-0.47	-0.74
	(0.58)	(0.53)	(0.54)	(1.41)	(2.46)	(1.30)
Observations	220	1,060	701	220	1,060	701
Over-ID test (p-value)	0.60	0.55	0.30	0.62	0.74	0.92
Weak-ID test (F-stat)	36.49	22.07	8.39	36.49	22.07	8.39
adj. R^2	-0.47	0.06	0.21	-0.74	-0.05	-0.05
	(C) Pump prices					
Dep. Var.:	Diesel fuel			Gasoline		
	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)
Kyoto (0,1)	0.49***	0.16***	0.31***	0.46***	0.22***	0.32***
	(0.06)	(0.06)	(0.08)	(0.06)	(0.07)	(0.09)
Observations	252	554	316	252	554	316
Over-ID test (p-value)	0.04	0.37	0.39	0.47	0.47	0.04
Weak-ID test (F-stat)	39.06	21.99	8.83	39.06	21.99	8.83
adj. R^2	0.47	0.66	0.67	0.40	0.55	0.60
	(D) Per capita use of					
Dep. Var.:	Energy			Electricity		
	(D1)	(D2)	(D3)	(D4)	(D5)	(D6)
Kyoto (0,1)	-0.06	-0.05	-0.06*	-0.02	-0.08*	-0.06
	(0.04)	(0.04)	(0.03)	(0.06)	(0.05)	(0.04)
Observations	220	1,060	701	218	1,049	701
Over-ID test (p-value)	0.67	0.31	0.42	0.92	0.17	0.74
Weak-ID test (F-stat)	36.49	22.07	8.39	36.30	22.14	8.39
adj. R^2	-0.02	0.32	0.43	0.15	0.47	0.65

Note: Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. * p<0.1,** p<0.05, *** p<0.01. Instruments for Kyoto: membership in ICC and its spatial lag. All regressions use year dummies and additional controls as in Table 1.5. Full results in Appendix A, Table A.4.

problem and reduces the maximum LIML size bias to 15% for all specifications, which is not overly large.

Summarizing, using the same instrumental variables strategy as for CO_2 emissions, we find that Kyoto has increased the share of renewables in energy production by about 2-3 percentage points and reduced that of fossil fuels by similar amounts. It also has increased the share of alternative energy in electricity production by about 2 percentage points. Relative to the counterfactual, prices of diesel and gasoline are about 20 US cents higher in Kyoto countries. Finally, Kyoto appears to have lowered per capita electricity and energy use, although this finding is less robust. These results are robust to excluding transition countries. These channels contribute to emission savings and thus support the finding of Kyoto countries' emission reductions.

1.6 Discussion

We provide robust evidence that Kyoto commitment reduced CO_2 emissions. We also identify channels through which Kyoto may have affected emissions. Our paper fits in the larger literature on the empirical evaluation of the effectiveness of international environmental agreements. As pointed out, previous studies on the Helsinki or Oslo Protocol regulating SO_2 emissions do not find effects. These studies acknowledge the self-selection into treaties problem and use Heckman-type self-selection models (Ringquist and Kostadinova, 2005) or random- and fixed-effects estimation (Ringquist and Kostadinova, 2005; Aakvik and Tjøtta, 2011). However, they are conceptually different from our approach in that they do not employ IV estimation. Furthermore, the Kyoto Protocol might be special in at least two respects. First, the issue of global warming is discussed very controversially and trails a huge public debate. So informal enforcement mechanisms could play a more important role. Second, CO_2 emissions are associated with costly inputs: energy and electricity. There is always an economic incentive to save on those. Our paper can certainly not reconcile opposing views on voluntary IEAs' effectiveness. It will be an avenue of future research to assess the general effectiveness of international environmental agreements.

In our study, we only shortly touch the issue of carbon leakage. We saw that lower fuel prices and a change in energy and electricity generation may explain part of the observed emission reductions. Yet, our study is silent on another possible channel: relocation of production to non-Kyoto countries and re-export to Kyoto countries. This would also generate the observed pattern of less emissions in Kyoto countries. Assessing the empirical importance of carbon leakage is an important avenue for future research.

Chapter 2

Kyoto and the Carbon Footprint of Nations*

2.1 Introduction

A country's *carbon footprint* accounts for all carbon dioxide (CO_2) emissions that the country's residents cause by consuming or investing a specific vector of goods. Whether these goods are produced domestically or imported does not matter.[1] However, the carbon inventories drawn up by the UN's Framework Convention on Climate Change (UNFCCC) measure *domestic emissions*, i.e., the amount of carbon embodied in the vector of goods produced on a nation's territory. With international trade in goods, a country's carbon footprint and its domestic CO_2 emissions need not coincide, the difference being the *carbon content of net trade.*

This paper provides the first econometric *ex post* analysis of the Kyoto Protocol, thereby complementing computable general equilibrium (CGE) analyses such as the one by Elliott et al. (2010). For this purpose, it assembles a new *panel* database on the carbon footprint of nations. It uses an instrumental variables (IV) strategy to study the effects of commitments made by some countries under the Kyoto Protocol on countries' CO_2 emissions and carbon

* This chapter is based on joint work with Gabriel Felbermayr. It is based on the article "Kyoto and the Carbon Footprint of Nations", in: *Journal of Environmental Economics and Management*, 2013, 63(3), 336-354. This is a revised version of our working paper that circulated under Ifo Working Paper No. 103, 2011.

[1] This is the flow version of the carbon footprint. The stock version refers to *accumulated* emissions embodied in goods absorbed over a country's existence.

footprints. The key finding is that, on average, Kyoto has caused some domestic emission savings. But it has also caused increased net imports of carbon so that the carbon footprint of countries has not changed. *Carbon leakage* due to the Protocol's incomplete coverage has therefore neutralized emission savings.

The international policy community cares about anthropogenic CO_2 emissions because they are believed to trigger global warming, which can have large negative consequences for global welfare (Stern, 2007). The Kyoto Protocol has been the first multilateral attempt to cap carbon emissions. Many observers think that the design of the Protocol is fundamentally flawed because it exempts emerging and developing countries,[2] and it lacks an enforcement mechanism. Whether it has actually affected countries' emissions, their carbon footprints or the carbon content of net trade is an unsettled empirical question. For any successful future international agreement on climate policies, more needs to be known about the empirical relevance of the leakage phenomenon.

Several difficulties affect the empirical analysis. First, selection of countries into the Kyoto Protocol may be non-random so that estimating treatment effects requires instrumental variables. Second, for statistical inference we need to minimize errors in the measurement of countries' carbon footprints. We use high quality input-output (I-O) tables and sectoral emission coefficients from official sources to calculate footprints. These data are available only for 40 countries and the period 1995-2007. Covering more than 80% of the world's emissions, our data allows using differences-in-differences techniques to measure the impact of Kyoto commitments on domestic CO_2 emissions, carbon footprints and net carbon imports.

We report the following findings. First, carbon emissions embodied in international trade flows are quantitatively important: in 1995, about 16% of emissions were traded; in 2007 this measure is up to 21%. The increase started in 2002, the first year of China's WTO membership and the year in which most countries ratified their Kyoto commitments. Second, there is substantial variation across countries in the levels and growth rates over time of domestic CO_2 emissions and carbon footprints. Third, a naive inspection of the data suggests that growth rates of carbon footprints and growth rates of domestic CO_2 emissions are negatively correlated with Kyoto commitment status. Fourth, we show that countries' ratification of the Rome Statute governing the International Criminal Court (ICC) predicts Kyoto commitment. Our identifying assumption is that a country's stance on the ICC has

[2] The USA has not ratified the treaty, presumably because it "leaves out developing countries such as China and India" (Feenstra and Taylor, 2008, p. 426).

no effect on domestic emissions or footprints so that the ICC membership dummy can be excluded from our second-stage instrumental variables regressions. The same holds true for trading partners' ICC status and lagged population growth. Fifth, we use these instruments in a differences-in-differences setup. We find that Kyoto commitments have reduced domestic CO_2 emissions on average by about 7% (relative to the unobserved counterfactual), but the carbon footprint has not changed. As a consequence, the ratio of CO_2 imports over domestic CO_2 emissions (the carbon imports ratio) has increased on average by about 14 percentage points.

Related Literature. A number of *descriptive* studies present estimates of the carbon footprint of nations. Hertwich and Peters (2009) do so for 87 countries and 2001 data;[3] Davis and Caldeira (2010) update the analysis to 113 countries and 2004 data. These papers make an impressive effort toward a comprehensive view on the carbon footprint of nations by including other major greenhouse gases such as CH_4, N_2O or F-gases, by accounting for agricultural production, land-use change, international transportation, and the non-market sector (heating). Only recently a panel data set for 113 regions has been proposed by Peters et al. (2011). The authors provide detailed estimates for the years 1997, 2001 and 2004 and base their analysis on raw data from the Global Trade Analysis Project (GTAP).[4] In our econometric exercise, to minimize measurement error in the dependent variable, we must restrict the analysis to those 40 countries for which the OECD provides high quality I-O tables and for which there is official data to generate *yearly sectoral* emission coefficients.

Our study relates to a large tradition in empirical economics to analyze the effects of international or domestic institutional arrangements on economic outcomes. In virtually all applications, reverse causation is an issue as the choice of institutions (or policies) and the membership in international organizations is not exogenous. Moreover, membership is measured by simple dummy variables.[5] The public economics literature specifically deals with membership in international environmental agreements and its effects. The theory highlights that free-riding and coordination are particularly important in the case of the Kyoto Protocol and other treaties governing air pollution due to the public goods nature

[3] Their data are very nicely presented on a website `www.carbonfootprintofnations.com/`.

[4] GTAP I-O data may suffer from measurement problems as they are not based on a harmonized data collection and processing approach. Also, yearly sectoral emission and output data is available for only half of those countries.

[5] As examples, see the literature on the effects of IMF (Dreher and Walter, 2010) or WTO membership (Rose, 2004a,b).

of emissions (see e.g. Andreoni and McGuire, 1993; Congleton, 2001; Beron et al., 2003). Murdoch and Sandler (1997) empirically show that CFC cutbacks took place already before ratification of the Montreal Protocol, which regulates the use of ozone depleting substances.[6] Ringquist and Kostadinova (2005) point out that the voluntary nature of international environmental agreements leads to self-selection bias and find significantly reduced SO_2 emissions for the Helsinki Protocol. Aakvik and Tjøtta (2011) also control for the possible endogeneity of the Helsinki and Oslo Protocol by employing a differences-in-differences strategy and find no effect on sulphur dioxide emissions. Our paper is the first to employ an IV strategy to deal with (Kyoto's) endogeneity.

There is a rich theoretical and quantitative literature on the effectiveness of climate policies in the presence of international trade; see Copeland and Taylor (2005) for an important early contribution and de Melo and Mathys (2010) for a survey. An important CGE study by Babiker (2005) uses a model with increasing returns to scale and an Armington demand system and finds carbon leakage in excess of 100% in one scenario. Recent work focuses on border tax adjustments as remedies to the carbon leakage problem. Mattoo et al. (2009) highlight how border tax adjustments could harm developing economies. Elliott et al. (2010) find substantial carbon leakage ranging from 15% at low tax rates to over 25% for the highest tax rate. Our approach complements the ex ante perspective of CGE models by carrying out an ex post evaluation of the Kyoto Protocol's effect on the carbon footprint, emissions, and trade.

The structure of the paper is as follows. In Section 2.2 we describe how we construct our panel database on nations' carbon footprints. Section 2.3 discusses the selection of countries into commitments under the Kyoto Protocol and presents our instrumental variables strategy. Section 2.4 contains our main results and an array of robustness checks.

2.2 Measuring the carbon footprint of nations

2.2.1 Method and data

In the presence of international trade, domestic emissions need not coincide with the CO_2 embodied in domestic consumption and investment, i.e., the country's *carbon footprint.* To

[6] Although Wagner (2009) revisits the evidence and challenges the findings on ground of data limitations.

calculate the carbon footprint, one has to measure the *carbon content of trade*, i.e., the CO_2 emissions embodied in a country's exports and imports.

Country i's carbon footprint, F_{it}, is defined as domestic emissions, E_{it}, plus the emissions embodied in imports minus the emissions embodied in exports. So, the *net emissions embodied in trade*, EET_{it}, are added to E_{it}:

$$F_{it} = E_{it} + EET_{it}. \tag{2.1}$$

For a given year, the sum of the carbon footprints of all countries is equivalent to the sum of domestic emissions of all countries. This implies that the net emissions embodied in trade add up to zero in the world, i.e. $\sum_i EET_{it} = 0$.

Accounting method. To obtain a precise measure of EET_{it}, it is crucial to account for the increasing importance of trade in intermediates and re-exports. Therefore, one has to track each product and its components along the global production chain to the respective country of origin.[7] As an example, let country A's sector h use an intermediate input from country B. Country B might assemble this intermediate from intermediates produced locally or in a country C, D or even A, and so forth. All those upstream emissions (occurring locally or abroad) must be associated to the final consumption of good h.

Recently, Trefler and Zhu (2010) have proposed a *multi-region input-output (MRIO) method* to account for global supply chain linkages. The MRIO I-O table is an extension of the standard I-O table. In contrast to the standard I-O table, the MRIO method distinguishes each intermediate flow not only by sector but also by country of origin. So, each column and row of the MRIO table corresponds to a country-sector combination. The MRIO table collects all bilateral inter-industry demand linkages into a world I-O table $\mathbf{B}$:[8]

$$\mathbf{B} \equiv \begin{pmatrix} \mathbf{B}_{11} & \cdots & \mathbf{B}_{1N} \\ \vdots & \ddots & \vdots \\ \mathbf{B}_{N1} & \cdots & \mathbf{B}_{NN} \end{pmatrix},$$

[7] This is crucial since Metz et al. (2007) document wide cross-country heterogeneity in production structures and sectoral carbon intensities.

[8] To avoid notational clutter, we suppress time indices in the following. Matrices and vectors are in bold face.

where $\mathbf{B}_{ij}$ is the *bilateral I-O table* of intermediates produced in country i and used in country j and N is the total number of countries.[9] Bilateral I-O tables $\mathbf{B}_{ij}$ are derived from reported multilateral tables $\bar{\mathbf{B}}_j$ under the assumption that country i's share of intermediates h in country j's sector g is proportional to its import share in this sector.[10]

Except for the distinction of each sector by country, the MRIO method follows the standard I-O logic to obtain total carbon intensities. Let $\mathbf{I}$ be the identity matrix. The Leontief inverse $(\mathbf{I}-\mathbf{B})^{-1}$ is the matrix of total unit input requirements of each sector in each country for intermediates of each sector in each country.[11] Let $\mathbf{e}_i$ be country i's sectoral CO_2 emission intensities vector, and $\mathbf{e} \equiv \begin{pmatrix}\mathbf{e}_1 & \cdots \mathbf{e}_N\end{pmatrix}$ be the world emission intensity vector. By adding up a country-sector's input requirement times the respective emission intensity over all country-sectors, we get the vector of total carbon intensities of each sector in each country $\mathbf{A} = \mathbf{e}(\mathbf{I}-\mathbf{B})^{-1}$.

The last step is to evaluate the sectoral exports and imports with their respective total carbon intensity to get EET_i, where the emission intensity of imports differs for each trade partner j. Let $\mathbf{X}_i$ be country i's vector of sectoral exports and $\mathbf{M}_{ij}$ its vector of sectoral imports from country j and $\mathbf{T}$ be the world trade matrix

$$\mathbf{T} \equiv \begin{pmatrix} -\mathbf{X}_1 & \mathbf{M}_{21} & \cdots & \mathbf{M}_{N1} \\ \mathbf{M}_{12} & -\mathbf{X}_2 & \ddots & \vdots \\ \vdots & \ddots & \ddots & \vdots \\ \mathbf{M}_{1N} & \cdots & \cdots & -\mathbf{X}_N \end{pmatrix}.$$

The i'th column of this trade matrix corresponds to the exports and bilateral imports of country i.[12] Accordingly, the MRIO net emissions embodied in trade are given by

$$EET_i = \mathbf{A}\mathbf{T}_\mathbf{i}, \tag{2.2}$$

[9] The submatrices $\mathbf{B}_{jj}$ on the diagonal, i.e. for which $i = j$, are the domestic I-O tables.

[10] Country $j's$ use of sector g inputs from country $i's$ sector h is $B_{ij}(h,g) = \theta_{ji}(h)\bar{B}_j(h,g)$, where the import share is $\theta_{ji}(h) \equiv M_{ji}(h)/(Q_j(h) + \sum_k M_{jk}(h) - X_j(h))$ and $Q_j(h)$ is country j's output in sector h; see OECD (2002, p. 12).

[11] The first H columns of the Leontief inverse correspond to the vector of intermediate demand of sectors $h = 1, \ldots, H$ in country 1, the next H columns to the intermediate demand of sectors $h = 1, \ldots, H$ in country 2, and so on.

[12] Note that imports enter the trade matrix positively while exports enter with a minus. This ensures that emissions embodied in imports are added while emissions embodied in exports are substracted when deriving the EET.

where we again see the strong similarity to the standard carbon content of trade calculation. So, to empirically compute EET_{it}, one requires input-output tables, bilateral trade data, and sectoral CO_2 emission coefficients, ideally all for the year t.

Data. Harmonized I-O tables for our 40 sample countries are taken from the OECD Input-Output Tables 2009. They are observed around the years 1995, 2000, and 2005.[13] We aggregate the I-O data to 15 ISIC industries to match the available emissions data. We obtain bilateral goods trade data in f.o.b. values from the UN Comtrade database. We use a concordance table provided by Eurostat[14] to translate the data from the SITC commodity classification into ISIC. Information on the level of sectoral CO_2 emissions from fuel combustion come from the International Energy Agency (IEA).[15] In order to obtain emission coefficients, we divide sectoral emission levels by some measure of sectoral output. Output data is obtained from the OECD Structural Analysis Database, the UNIDO Industrial Statistics Database (INDSTAT2) 2011, the UN System of National Accounts,[16] and OECD I-O tables.[17] A detailed data description follows in the Appendix B. Our database comprises 40 countries over the period 1995 to 2007; countries are listed in Table 2.1. To model the rest of the world (RoW), we argue that countries at a similar stage of economic development have similar production technologies. Therefore, we group countries into three classes according to their level of real GDP per capita, obtained from the Penn World Table 6.3. Each RoW country is assigned a weighted average of emission coefficients and I-O tables of sample countries in the same real GDP class.[18]

[13] We used the I-O tables from 1995 for the years 1995-97, those from 2000 for 1998-2002, and those from 2005 for 2003-2007.

[14] `http://ec.europa.eu/eurostat/ramon`.

[15] They include CO_2 produced during consumption of solid, liquid, and gas fuels and gas flaring as well as the manufacture of cement. Note that other sources of CO_2 emissions such as fugitive emissions, industrial processes or waste are disregarded. However, CO_2 emissions from fuel combustion make up roughly 80% of total CO_2 emissions.

[16] `http://data.un.org/Data.aspx?d=SNA&f=group_code%3a203`.

[17] For some countries and years sectoral output data are missing. We impute missing output data by applying growth rates of output or where those were not available growth rates of real GDP of the respective country and year.

[18] Alternatively, we apply U.S. emission coefficients and I-O tables to RoW. The obtained carbon footprint series are virtually the same.

2.2.2 Descriptive evidence

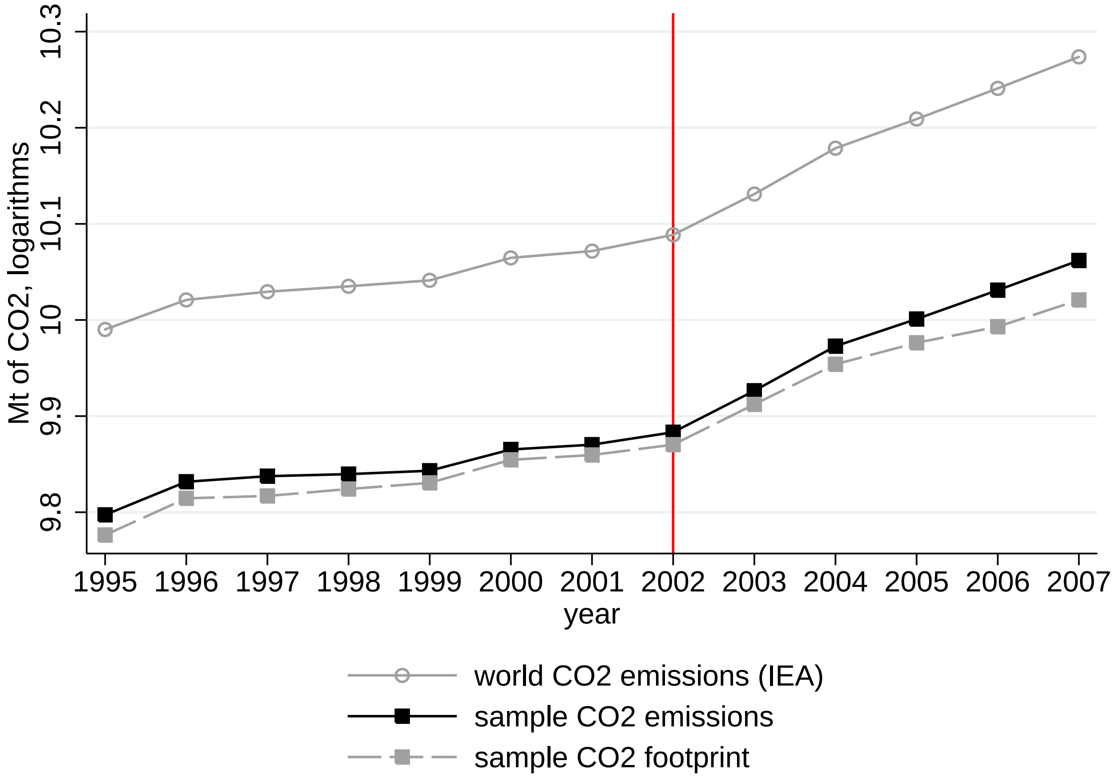
Emissions and carbon footprint. Figure 2.1 tracks CO_2 emission levels in logs for the whole world and for our sample. The upper (gray, solid) curve relates to the entire world and measures CO_2 emissions as reported by the IEA. From 1995 to 2007 emissions have increased by about 33% (an increase of 7.2 gigatons of CO_2); about two thirds of this increase occurred after 2002, the first year of China's WTO membership and most countries' year of Kyoto ratification. The second curve (black, solid) reports emissions for our sample of 40 countries. Over the whole period of 13 years, our sample covers a fairly constant share of about 81.5% of world emissions. The curve closely tracks the behavior of the world total. Finally, the last curve (gray, dashed) shows the carbon footprint of our sample. This measure closely tracks our emission data, but not perfectly. The reason is that we do not force our sample world to be closed; rather, there is trade with the rest of the world. Over the sample period, our sample countries have consistently run a trade surplus in terms of carbon (i.e., carbon emissions in the group exceed the carbon footprint). It started of at around 2% of emissions in 1995, dipped slightly in the late 1990s but has increased since 2002 and stood at about 4% in 2007. The trade surplus as well as its increase stems from net carbon exports of China in particular (in 2007: 600 Mt of CO_2) and also India (in 2007: 100 Mt) to the rest of the world.

Trade in goods and embodied emissions. Figure 2.2 plots the evolution of CO_2 emissions embodied in international trade (black line).[19] Trade in carbon has increased by about 60% between 1995 and 2007, the largest share of the absolute increase (74%) happening after 2002. The gray line in Figure 2.2 tracks the share of carbon trade in total emissions. The share remains fairly constant at around 16 to 17% from 1995 to 2002 but increases drastically from 2003 onwards to reach 21% in 2007. This is explained by a large increase in the trade volume from 2003 onwards and happened even though the carbon intensity of trade fell by roughly 20% in the same period (not shown).

Country level comparisons. Table 2.1 shows detailed information about the countries included in our sample. With five exceptions (Australia, Czech Republic, Romania, Russia and Switzerland), ratification of the Kyoto Protocol has taken place in the year of 2002.

[19] This is the CO_2 embodied in imports, not to be confused with the net CO_2 EET.

Figure 2.1: CO_2 emissions in the world and in the sample



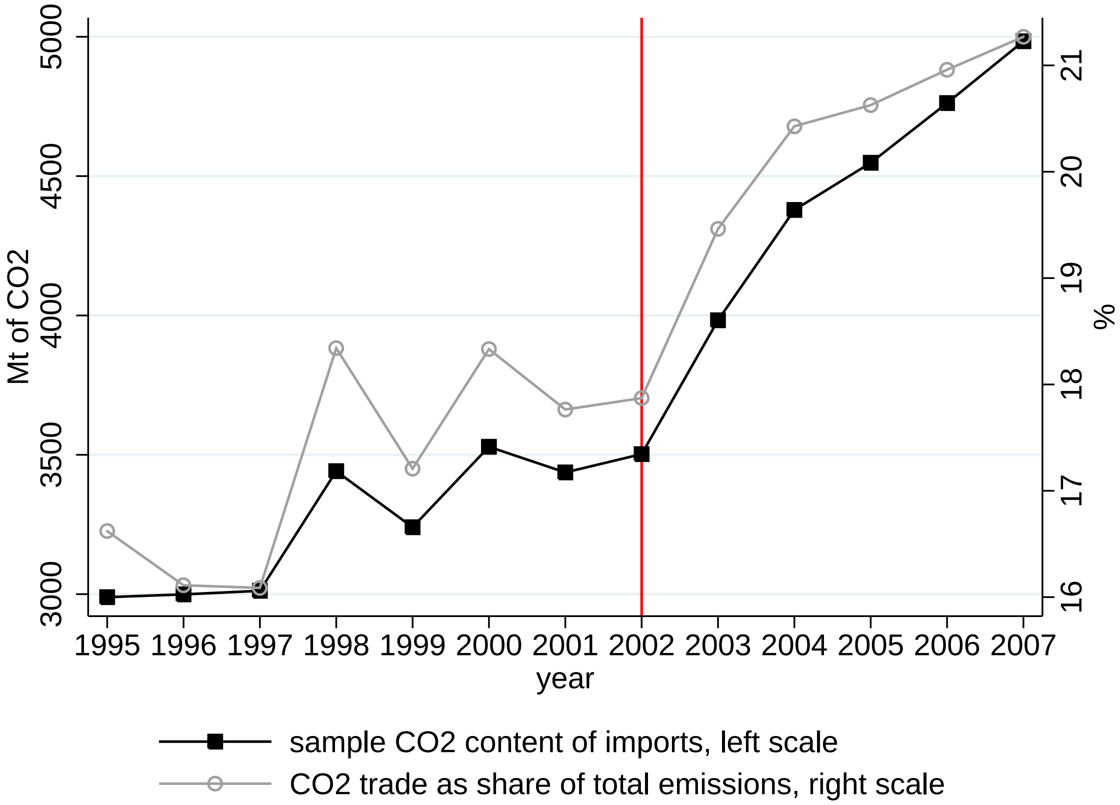
Source: Inventoried emissions data from the IEA; Carbon footprint: own calculations. World corresponds to 187 countries; sample to 40 countries.

In 1995, emissions per capita (in tons of CO_2) vary dramatically across countries. At the lower end, emissions per capita in India or Indonesia are 0.88 and 1.04 tons, while they are 19.65, 16.24, and 16.01 tons per capita at the higher end in the U.S., Australia, and Canada. Average yearly growth rates of per capita emission levels range from 5.14% in China to -1.76% in Sweden.[20] Regressing those growth rates on the logarithm of initial emission levels yields a coefficient of -1.19 (robust t-value -4.72), so that there is a substantial amount of (absolute) convergence.

Turning to carbon footprints, countries with high per capita emissions also have high per capita footprints; the coefficient of correlation is 0.93. There is also evidence for convergence, but the estimated coefficient is smaller (-0.76, t-value -4.04). However, the coefficient of correlation between the growth rates of per capita emissions and footprints is 0.42 only. Finally, the last two columns in Table 2.1 show net CO_2 imports in percent of domestic emissions of the years 1995 and 2007. Somewhat less than two thirds of countries have

[20] Since our starting year is 1995, industrial restructuring in formerly communist economies has mostly come to an end.

Figure 2.2: CO_2 content of trade and share of CO_2 emissions traded



Source: CO_2 content of trade: own calculations. Sample (40 countries).

positive net imports. Net carbon imports can be very substantial: e.g., in 2007, Switzerland imports goods that embody almost 122% of domestic CO_2 emissions. Imports in Ireland, Netherlands, Sweden, Norway, France also exceed 30% of domestic emissions. The share of carbon emissions exported is highest in China (27%) and South Africa (20%), the Czech Republic (18%) and Australia (15%). So being a net carbon exporter is not a phenomenon limited to developing countries.

2.3 Empirical strategy

2.3.1 Differences-in-differences estimation

We are interested in estimating the effect that Kyoto commitment has on countries' carbon dioxide emissions, carbon footprints, and carbon trade. Our working hypothesis is that the Kyoto Protocol's year of ratification in national parliaments marks the point in time from which on Kyoto had an effect on outcomes. Kyoto commitment can be seen as a commitment

Table 2.1: Per capita emission levels and carbon trade: initial levels and rates of change.

Country	Year of ratification	CO_2 emission		CO_2 footprint		Net CO_2 imports (as % of emissions)	
		1995	avg. yearly growth rate	1995	avg. yearly growth rate	1995	2007
India		0.88	2.60%	0.73	3.39%	-17.3	-10.1
Indonesia		1.04	4.05%	1.09	2.54%	4.9	-10.7
Brazil		1.55	1.32%	1.57	1.77%	1.3	6.4
China		2.57	5.34%	2.27	3.50%	-11.7	-27.3
Turkey		2.68	2.57%	2.88	1.46%	7.6	-4.6
Chile		3.30	2.57%	3.60	1.96%	9.1	2.2
Mexico		3.34	1.71%	3.08	2.67%	-7.8	2.3
Argentina		3.65	0.96%	3.82	0.27%	4.7	-3.0
Portugal	2002	4.61	1.07%	5.32	1.56%	15.4	21.6
Romania	2001	5.36	-2.35%	4.11	-0.20%	-23.4	-2.7
Spain	2002	5.60	3.89%	5.92	4.65%	5.7	14.6
Hungary	2002	5.66	-0.40%	5.76	0.30%	1.8	9.9
Switzerland	2003	5.84	-0.41%	11.57	1.38%	98.0	140.9
France	2002	6.14	-0.52%	7.16	0.86%	16.6	35.8
South Africa		6.69	0.61%	5.31	0.60%	-20.6	-20.7
New Zealand	2002	7.06	1.80%	7.95	1.57%	12.6	9.8
Italy	2002	7.08	0.56%	7.09	1.57%	0.1	11.8
Sweden	2002	7.08	-2.91%	7.96	0.10%	12.4	57.3
Greece	2002	7.17	2.23%	7.62	3.14%	6.3	17.1
Slovenia	2002	7.26	0.80%	6.46	1.69%	-11.0	-2.1
Norway	2002	7.65	0.39%	10.14	1.47%	32.7	49.2
Slovakia	2002	7.69	-1.17%	7.78	0.14%	1.2	17.0
Austria	2002	7.78	0.81%	9.52	0.53%	22.4	18.7
Korea		8.64	1.46%	8.48	1.53%	-1.8	-1.0
Israel		8.78	0.65%	10.12	-0.69%	15.3	-0.6
Poland	2002	8.98	-1.14%	7.96	-0.86%	-11.3	-8.4
United Kingdom	2002	9.18	-0.58%	8.89	-0.10%	-3.1	2.2
Japan	2002	9.24	0.44%	10.62	-0.19%	14.9	7.2
Ireland	2002	9.46	1.16%	9.52	1.82%	0.6	8.1
Russia	2004	10.49	0.62%	9.54	1.38%	-9.0	-1.2
Germany	2002	10.97	-1.12%	11.65	-0.81%	6.2	9.9
Netherlands	2002	11.46	-0.38%	14.95	0.73%	30.4	47.3
Estonia	2002	11.84	1.34%	8.38	4.37%	-29.2	-2.2
Finland	2002	12.15	0.11%	9.66	1.28%	-20.5	-9.7
Czech Republic	2001	12.20	-0.20%	9.86	-0.22%	-19.2	-19.4
Belgium-Luxembourg	2002	12.72	-1.13%	12.81	-1.74%	0.8	-5.9
Denmark	2002	13.43	-3.35%	15.10	-2.40%	12.4	25.2
Canada	2002	16.01	0.76%	16.42	0.84%	2.6	3.4
Australia	2007	16.24	1.48%	13.60	1.55%	-16.3	-15.6
United States		19.65	-0.23%	19.97	0.21%	1.6	6.7

Note: The 40 sample countries ordered with respect to their 1995 per capita emission levels. CO_2 emissions and footprints in tons of CO_2 per capita. Domestic CO_2 emissions are from the IEA. Carbon footprints computed using MRIO approach and approximating RoW I-O tables with the GDP per capita matching method described in the text.

device for national policy to overcome the prisoner's dilemma associated with a global public good (see the discussion in the public economics literature; for an overview see e.g. Congleton, 2001). By ratifying the Protocol in national parliament, policy makers tie their hands by an international agreement. Economic subjects adapt their expectations to this and start acting as well. The choice of ratification as the decisive treatment date is in line with other empirical studies investigating the effects of international environmental agreements on air pollutants like CFC or SO_2. Murdoch and Sandler (1997) find that signatories reduced their CFC emissions well before the Montreal Protocol's entry into force. And members to the Helsinki Protocol reduced their sulphur dioxide emissions after ratification (Ringquist and Kostadinova, 2005). Anecdotal evidence also suggests that countries have engaged in a flurry of policy initiatives after Kyoto ratification.[21]

In order to estimate the average treatment effect of Kyoto ratification, we use a differences-in-differences approach. I.e. we control for country-specific time-invariant omitted variables by including country dummies. To avoid reporting spurious results, we follow Bertrand et al. (2004) and work with long averages rather than with yearly data. For this purpose, we define different treatment periods and corresponding pre- and post-treatment periods. Our preferred specification defines the treatment period as the years 2001-2003, when all countries (except Russia and Australia) have ratified the Protocol. The pre- and post-treatment period are symmetric around the treatment period and defined as the four-year intervals 1997-2000 and 2004-2007.[22] Hence, we base our analysis on 80 pre- and post-treatment averages. This strategy deals with country-specific business cycles. First-differencing eliminates any country-specific time-invariant determinants of the relevant outcome variables (e.g., climatic conditions, endowments with different types of energy resources, geographic location, preferences of the representative consumers, etc.) thereby reducing omitted variables bias. Beyond that, the broad treatment window – which means we are dropping data around the treaty's ratification – has the advantage that it alleviates the problem of knowing exactly when countries actually started their emission reduction strategies. Hence, the specification takes the form

$$\begin{aligned} \Delta Outcome_{i,t} &= \delta + \beta \Delta Kyoto_{i,t} + \xi \Delta \mathbf{X}'_{i,t} + \upsilon_{i,t}, \\ Outcome_{i,t} &\in \left\{ \ln E_{i,t},\, \ln F_{i,t},\, \frac{EET_{i,t}}{E_{i,t}} \right\}, \end{aligned} \tag{2.3}$$

[21] See data displayed on www.lowcarboneconomy.com/Low_Carbon_World/Data/View/12.

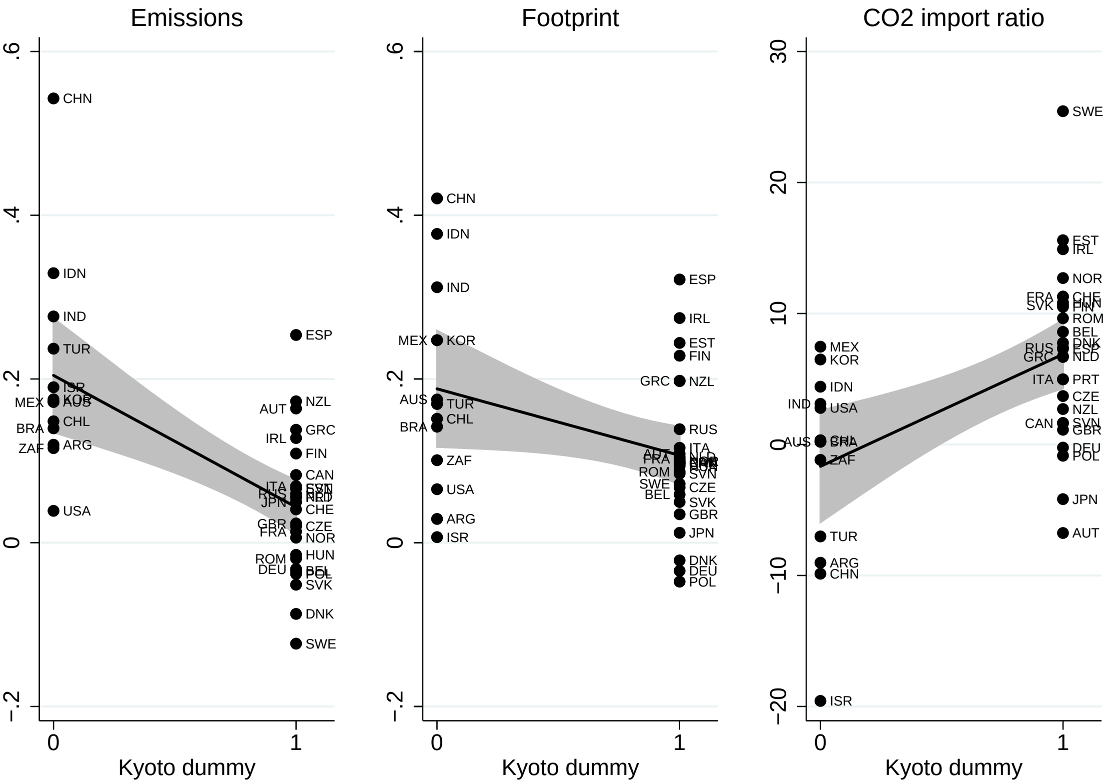
[22] Russia has ratified in 2004 and is counted as treated in our analysis; Australia has ratified in late 2007 and is put into the control group. We present robustness checks pertaining to these choices below.

where Δ denotes the first difference operator, and $t \in \{pre, post\}$ $(T = 2)$. δ is a constant accounting for a common time trend that would affect the treatment and the control groups alike. $E_{i,t}, F_{i,t}, EET_{i,t}$ are defined in (2.1). $\mathbf{X}_{i,t}$ is a vector of controls and includes amongst others the log of population, the log of GDP and an EU dummy. The controls are motivated in more detail when presenting the results. $Kyoto_{it}$ is a dummy taking value 1 if country i has Kyoto commitments in period t. Due to our two-period setup, $\Delta Kyoto_{i,t} = Kyoto_{i,t}$. We correct the variance-covariance matrix for heteroskedasticity. Before addressing the crucial question of instrumenting $Kyoto$, we set $\xi = \mathbf{0}$ in equation (2.3) and show scatter plots and univariate regressions to obtain a first impression on the effect of Kyoto on outcome variables.

Figure 2.3 plots the change between the pre- and post-treatment period average of log CO_2 emissions, log carbon footprints (both in per capita terms) and the carbon import share against the Kyoto commitment dummy. The left-most panel reveals that between the two periods domestic CO_2 emissions have, on average, grown by 20% in the subsample of non-committed countries compared to 4% in the subsample of committed countries. This difference, 16 percentage points, is statistically significant at the 1% level. When looking at the middle panel – footprints – the evidence is less clear-cut. On average, the growth rate of per-capita carbon footprints appears 8 percentage points higher in the subsample of non-committed countries, the difference being statistically significant at the 5% level. Finally, the right-most panel compares the change in the CO_2 import share. That share has increased by 9 percentage points more in the sample of committed countries, the difference being statistically significant at the 1% level.

The evidence displayed in Figure 2.3 is suggestive. It points toward the possibility that Kyoto commitments have indeed reduced domestic CO_2 emissions, but less so the carbon footprint. Kyoto would thus have led to delocation of carbon-intensive production and to an increase in carbon trade, but not to a reduction of committed countries' absorption as captured by the carbon footprints. While Figure 2.3 can deal with constant level differences across countries and common time trends, it cannot address the non-random selection of countries into the Kyoto Protocol. The reported effects may be spurious if countries with beneficial emission projections, for example, might be more willing to commit to an emission target under Kyoto. The next section models countries' selection into Kyoto and identifies variables that explain selection but not emissions.

Figure 2.3: Kyoto commitment and changes in log carbon emissions, log footprint, and CO_2 import share



Note: The graphs show scatter plots of differences between pre- and post-treatment period averages in log CO_2 emissions and footprints per capita and in the share of CO_2 imports over domestic emissions for committed and non-committed countries. The graphs also show fitted linear regression lines with 95% (heteroskedasticity-robust) confidence intervals.
Regression coefficients and robust standard errors (in parentheses). Emissions: -0.16^{***} (0.04); footprint: -0.08^{**} (0.04); carbon import ratio: 8.56^{***} (2.47).

2.3.2 Instrumental variables strategy

The IV approach solves the omitted variables bias by exploiting the exogenous variation in an instrumental variable that is correlated with the endogenous variable of interest. When the IV strategy is valid,[23] it allows causal inference. In this paper, we propose countries' membership at the *International Criminal Court* as an instrument for Kyoto commitment. The ICC, headquartered in The Hague, Netherlands, is a permanent tribunal to prosecute individuals for genocide, crimes against humanity, war crimes, and the crime of aggression. Like the UNFCCC, the ICC is a multilateral policy initiative. The Rome Statute governing the ICC was finally signed in 1998 and ratified, until December 2010, by 114 countries.

[23] The crucial assumptions are that the instrument is neither related with the outcome nor the error term (for example omitted variables), i.e. the instrument is independent of the outcome; but highly correlated with the endogenous variable.

34 countries, including the U.S., India, and China, have decided not to ratify the Statute. Groves (2009) likens the Kyoto Protocol to the Rome Statute and argues that both initiatives threaten U.S. sovereignty.[24] Indeed, countries' preferences for multilateral international policy initiatives, proxied by their involvement in the ICC, turn out to correlate robustly to Kyoto commitment. The maintained assumption is that ICC involvement of a country is not caused by carbon emissions or the footprint and that it does not directly affect these outcome variables, neither.[25]

Our selection equation takes the following form

$$\Delta Kyoto_{i,t} = \alpha_0 + \alpha_1 \Delta ICC_{i,t} + \alpha_2 \Delta W.ICC_{i,t} + \alpha_3 \Delta \ln Pop_{i,t-1} + \zeta \Delta \mathbf{Z}'_{it} + \varepsilon_{it}, \tag{2.4}$$

where ICC_{it} is a dummy taking the value 1 if a country has ratified the Rome Statute. Data on ICC membership stems from the UN Treaty Series database. The variable $W.ICC_{i,t}$ captures ICC membership of other countries and is computed as $\sum_{j \neq i} \frac{Pop_{j,t}}{Dist_{ij}} ICC_{j,t}$, where $Dist_{ij}$ is geographical distance between countries i and j, and $Pop_{j,t}$ is population. $Pop_{j,t}/Dist_{ij}$ is a conventional spatial weight. It ensures that $W.ICC_{i,t}$ increases when other countries ratify the Rome Statute, and does so most when those countries are large and close by. Data on population and GDPs stem from the World Bank World Development Indicators and data on bilateral distance from the CEPII distance database. $\ln Pop_{i,t-1}$ refers to the average log of population as of the 4-year period *before* the pre-treatment period. This instrument is motivated by a study of York (2005) who finds that population growth makes ratification of the Kyoto Protocol less likely. Yet, lagged population growth does not affect contemporaneous emissions. The ICC and lagged population variables are the instruments that we exclude from our second-stage regressions.

The vector $\mathbf{Z}'_{it}$ contains other potential variables that may play a role for the selection of a country into the Kyoto Protocol. They reflect, amongst others, preferences and political institutions. The first variable is EU_{it}; a dummy variable that takes value 1 if country i is an EU member at time t. We assume that EU countries might be more willing to act upon the threat of global warming. The vector also includes $\ln MEA_{i,t}$ which counts the number of

[24] Similarly, Mike Huckabee (2007), former Governor of Arkansas, argues that the Kyoto Protocol "would have given foreign nations the power to impose standards on us." China's stance in the Copenhagen climate change negotiations was similar.

[25] Other multilateral treaties, such as those governing the WTO or international environmental questions cannot be easily excluded since they will affect emissions directly either through "green" preferences of voters and consumers, or through trade policy.

Table 2.2: Summary statistics

Variable	Mean	Std. Dev.	Min	Max	Data source
Log CO_2 emission	0.05	0.11	-0.15	0.50	iea.int
Log CO_2 footprint	0.09	0.10	-0.14	0.37	
Net CO_2 imports (as share of emissions)	0.04	0.08	-0.20	0.25	
Kyoto dummy	0.68	0.47	0.00	1.00	unfccc.int
ICC dummy	0.68	0.40	0.00	1.00	treaties.un.org
ICC dummy, spatial lag	0.52	0.63	0.01	2.65	
Log population, time lag	0.02	0.02	-0.03	0.07	PWT 7.0
Log GDP	0.42	0.13	0.23	0.83	PWT 7.0
EU dummy	0.15	0.36	0.00	1.00	
China dummy	0.04	0.17	0.00	1.00	
Polity	0.44	1.36	-1.00	8.00	www.systemicpeace.org
Log stock of other MEA	0.28	0.14	0.13	0.72	iea.uoregon.edu

Note: The table shows mean, standard deviation, min and max of changes between pre-treatment (1997-2000) and post-treatment (2004-07) period as well as data sources

multilateral environmental agreements (MEAs) other than the Kyoto Protocol that country i has ratified up to period t. We think of it as a proxy for a country's environmental awareness. Data on MEAs are obtained from the International Environmental Agreement Database Project.[26] Another control is $Polity_{i,t}$, which measures country $i's$ political orientation (autocracy / democracy) using the Polity2 index from the Polity IV Data Series 2009. The index ranges from -10 to 10, where higher values indicate a stronger level of democracy. The vector $Z_{i,t}$ also includes the log of GDP, since higher economic activity implies more emissions which might hinder Kyoto ratification. Table 2.2 provides summary statistics of the variables.

The variables in $Z_{i,t}$ cannot be excluded from the second stage because they are related to emissions or footprints or the error term in equation (2.3). For example, environmental awareness might shape preferences (potentially a component of the error term in (2.3)) and thus consumption choices and the carbon footprint. Another example are political

[26] `http://iea.uoregon.edu/page.php?query=list_countries.php`

Table 2.3: Explaining Kyoto commitment

Dep.var.: Kyoto commitment			
	(1)	(2)	(3)
Method:	FD-OLS	FD-OLS	FD-OLS
Excluded instruments:			
ICC (0,1)	0.88***	0.76***	0.43**
	(0.12)	(0.15)	(0.20)
ICC, spatial lag		0.28*	0.19
		(0.14)	(0.12)
Log population, time lag		-4.59**	-11.29***
		(2.17)	(4.10)
Other covariates:			
EU (0,1)			-0.34
			(0.27)
Log stock of other MEA			0.54
			(0.76)
Polity (-10 to 10)			-0.03
			(0.06)
Log GDP			0.88
			(0.54)
No. of observations	80	80	80
Adj. R^2	0.69	0.76	0.83
F-statistic	49.60	45.99	25.15

Note: First-differenced linear probability models. T=2: pre-treatment (1997-2000), post-treatment average (2004-07). Spatial Kyoto lag: size and distance weighted ICC status of other countries. Regression includes a constant. Heteroskedasticity-robust standard errors. * p<0.1,** p<0.05, *** p<0.01.

institutions: they could affect (green) R&D activities / rate of innovation of a country (something left in the error term) and thus affect emissions.

Table 2.3 reports the first-stage results using a first-differenced linear probability model on pre- and post-treatment averages. Column (1) shows that ICC membership and Kyoto commitment are highly correlated. Column (2) adds the spatial ICC lag and lagged log population. Including these variables increases the fraction of explained (within) variance from 69 to 76%. Finally, column (3) corresponds to the first stage regression we use. The coefficient on the ICC dummy is 0.43 and statistically significant at the 5% level. It implies that a country which has ratified the Rome Statute and is therefore a member of the ICC has a 43 percentage points higher likelihood to commit to binding Kyoto commitments. The spatial lag of ICC membership is not statistically significant. Lagged log population turns out to be a strong predictor of Kyoto commitments: economies growing fast between the pre-pre and pre-treatment period have a strongly reduced probability to have commitments. A 1% increase in lagged population growth is associated with an 11 percentage points higher probability of Kyoto commitment. The log stock of other MEAs, i.e., excluding Kyoto, is a proxy for green preferences. It has a positive influence on ratification of the Kyoto Protocol, but the effect is not statistically significant. The coefficient on the Polity index suggests that an increase in the democratic stance of countries lowers the odds for Kyoto commitments, although the effect is not statistically different from zero. This is because in the period under consideration many non-committed countries have strongly improved their Polity ratings (Indonesia by 8 points, Mexico by 1.5 points, and Chile by 1.25 points). Finally, the coefficient on log GDP appears with a positive sign, but is statistically insignificant. GDP growth does not predict the ratification of Kyoto commitments. Overall, the share of explained variance is about 83% and the F-statistic is well above 20. The three excluded instruments are jointly highly significant, with an F-statistic of 12.5 (p-value 0.00).[27]

Our *identification assumption* is the following: Membership to the ICC is not caused by *growth* in carbon emissions or in the carbon footprint of nations. ICC membership does not directly affect growth in carbon emissions or the carbon footprint, neither.[28] Past population growth is another potential instrument, in particular if we work with dependent variables in per capita terms so that the contemporaneous population lag disappears from the left-hand side regressors. Then, it should not affect contemporaneous changes in emissions or the footprint or be caused by those variables. Since we have more than a single instrument, we can compute overidentification tests to verify whether the instruments are indeed uncorrelated with the error term, and are thus rightfully excluded from the estimated equation.

[27] A series of robustness checks (not shown), such as excluding China or transition countries or using random-effects Probit models, all corroborate that the first stage is strong.

[28] ICC membership may be a proxy for a country's overall preference for multilateralism. Our differences-in-differences strategy accounts for that preference as long as it does not change over time. In our second-stage regressions, we include the stock of other multilateral environmental agreements to capture the time-variant component.

2.4 Benchmark results: Kyoto has affected firms but not consumers

Table 2.4 presents our benchmark results. Columns (1) to (6) present OLS estimations, the remaining columns show evidence from IV regressions. In columns (1) and (2) the dependent variables are expressed in absolute terms while columns (4), (5), (7), (8), (10) and (12) express the dependent variables in per capita terms. Net EET are expressed as share of domestic emissions. All regressions are on first-differenced data, where the pre-treatment period is 1997-2000, and the post-treatment period is 2004-2007. Standard errors are heteroskedasticity-robust and finite-sample adjusted.[29] All regressions include a constant (not shown).

2.4.1 OLS estimates

Column (1) of Table 2.4 regresses the log of domestic CO_2 emissions on the Kyoto status dummy variable, the log of population, and the log of GDP. This parsimonious regression explains a surprising 54% of the total variation in emissions. The Kyoto dummy is negative and statistically significant at the 10% level (p-value of 0.06). The estimate implies that Kyoto commitment is associated with a decrease in domestic emissions by about 8%. The estimated elasticity of emissions with respect to population size is 1.13 and statistically significant at the 1% level. The elasticity is statistically identical to unity (the F-test on unity cannot reject with a p-value of 0.72). Hence, population growth translates one-to-one into emission growth. The unitary elasticity of CO_2 emissions with respect to population is a fairly robust finding.[30] The elasticity of emissions with respect to GDP is about 0.38 and statistically significant, thereby replicating the stylized fact that – holding population constant – economic growth reduces the carbon intensity of economies. Squared GDP or population terms do not turn out statistically significant and are therefore excluded.[31] Column (2) turns to the carbon footprint. Here, the estimated effect of Kyoto is zero, and statistically insignificant. The elasticity of population size is again statistically identical to unity (p-value of 0.88), and the elasticity on GDP is about 0.47. Squared terms are

[29] The employed STATA routine is described in Schaffer (2005).

[30] See Cole and Neumayer (2004), who have worked with a larger sample and longer time coverage.

[31] The literature on the carbon Kuznets curve has mixed results so far, see e.g. Dinda (2004) and Stern (2004).

Table 2.4: The effects of Kyoto on carbon emissions, footprint and net imports – Benchmark results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep.var.:	Emissions	Footprint	Imports	Emissions	Footprint	Imports	Emissions	Footprint	Imports	Emissions	Footprint	Imports
Units:	log *level*		share[a]	log *per capita* value		share[a]	log *per capita* value		share[a]	log *per capita* value		share[a]
Method:	FD-OLS	FD-OLS	FD-OLS	FD-OLS	FD-OLS	FD-OLS	FD-IV	FD-IV	FD-IV	FD-IV	FD-IV	FD-IV
Kyoto (0,1)	-0.08*	-0.00	0.08**	-0.08***	-0.00	0.09***	-0.08**	0.03	0.11***	-0.07**	0.06	0.14***
	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)
Log pop	1.13***	0.94**	-0.11									
	(0.36)	(0.43)	(0.36)									
Log GDP	0.38**	0.47***	0.08	0.37**	0.47***	0.09	0.37**	0.49***	0.10	0.13	0.36***	0.21*
	(0.16)	(0.09)	(0.10)	(0.16)	(0.09)	(0.10)	(0.16)	(0.10)	(0.09)	(0.09)	(0.12)	(0.11)
China (0,1)										0.30***	0.18***	-0.10
										(0.08)	(0.05)	(0.07)
Polity (-10 to 10)										0.02*	0.03***	0.01**
										(0.01)	(0.01)	(0.00)
Log stock of other MEAs										0.10	0.05	-0.10
										(0.11)	(0.10)	(0.12)
EU (0,1)										-0.07*	-0.08	-0.01
										(0.04)	(0.05)	(0.05)
2nd stage diagnostics												
adj. R^2	0.54	0.39	0.22	0.37	0.33	0.24	0.37	0.31	0.22	0.55	0.45	0.20
F-stat	14.88	11.99	4.38	5.29	14.34	6.48	4.70	13.32	5.40	10.96	46.94	2.76
RMSE	0.08	0.09	0.07	0.08	0.08	0.07	0.08	0.08	0.07	0.07	0.08	0.07
1st stage diagnostics												
Over-ID test (Hansen J, p-value)							0.19	0.27	0.16	0.20	0.20	0.18
Weak-ID test (F-stat)							36.23	36.23	36.23	34.39	34.39	34.39
Max IV F-test size bias[b]							10%	10%	10%	10%	10%	10%
Max IV bias rel. to OLS[b]							5%	5%	5%	5%	5%	5%

Note: First-differenced (FD) models. N=40 countries, T=2: pre-treatment average (1997-2000), post-treatment average (2004-2007). All regressions include a constant (not shown). Standard errors and 1st stage diagnostics are heteroskedasticity-robust and finite-sample adjusted. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Excluded instruments for Kyoto variable: ratification status of ICC treaty, spatial lag thereof, and lagged growth rate of population. IV: instrumental variables regression. Robust underidentification test (not reported) satisfied in every regression.
[a]Net carbon imports as share of domestic carbon emissions. [b]Stock and Yogo (2005); critical values are for Cragg-Donalds F-statistic and i.i.d. errors.

again statistically not different from zero and are therefore dropped. So, while GDP or population exert very similar effects on domestic emissions and on the carbon footprint, our results suggest that Kyoto did not have a measurable effect on the carbon footprint of nations. Since emissions apparently did go down, Kyoto commitment increased the CO_2 content of imports. Column (3) verifies this conjecture by regressing net carbon imports as a share of domestic carbon emissions on the Kyoto commitment dummy. The point estimate is positive, statistically significant at the 5% level. It implies that Kyoto commitment increases the net CO_2 import share by about 8 percentage points. GDP has a positive and population a negative sign but both are statistically insignificant. The regression is less successful than the preceeding ones in explaining outcome variance (adjusted R^2 is 0.22). Dropping the insignificant variables leaves the point estimate of Kyoto and its statistical significance unchanged.

Since the effect on log population in columns (1) to (2) is statistically identical to unity, it is useful to substract the log of population on both sides. This is equivalent to expressing the dependent variable in per capita terms and dropping the log of population from the list of covariates. This saves valuable degrees of freedom. Columns (4) to (5) show that this transformation has only a marginal effect on the point estimates of Kyoto commitment, and improves the accuracy of estimates. It still holds that Kyoto reduces domestic emissions, increases carbon imports but has not affected the carbon footprint. In most of the remaining analysis, we therefore work with emissions and footprints defined in per capita terms.

2.4.2 IV estimates

Regressions (1) to (6) in Table 2.4 assume that the error term is uncorrelated with the Kyoto dummy. As explained before, this is unlikely to be true: countries expecting a downward trend on their emissions may be particularly willing to commit to Kyoto targets so that OLS estimates will be biased away from zero. Other omitted variables like, for example, technological progress or environmental awareness are time-variant and therefore not differenced out with country-fixed effects. Hence, an IV approach is needed. Column (7) to (12) instrument the Kyoto dummy with the ratification status of the ICC treaty, the spatial lag thereof, and the lagged growth rate of population. In all specifications, we report a battery of diagnostics to check the validity of our IV strategy. In particular, we report the p-value associated to the Hansen test of overidentifying restrictions. The joint null hypothesis is that the instruments are valid, i.e., uncorrelated with the error term, and that the excluded instruments are correctly excluded from the estimated equation. The reported J-statistic is consistent in the presence of heteroskedasticity.[32] We also show the heteroskedasticity-robust Kleibergen-Paap Wald F-statistic on the excluded instruments. Following Stock and Yogo (2005), we report the maximum bias of IV estimation due to weak instruments. The first maximum bias relates to the actual (i.e., different from the undistorted F) maximal size of the Wald test; the second defines weak instruments in terms of the maximum bias of the candidate IV estimator relative to the squared bias of the OLS estimator. The idea is to compare the first-stage F-statistic matrix to a critical value. The critical value is determined by the IV estimator in use, the number of instruments, the number of included endogenous regressors, and how much bias or size distortion the researcher is willing to tolerate. Stock et al. (2002) suggest that an instrument is "weak" if IV *relative bias* exceeds 10% or the actual *size* of the nominal 5% IV t-test exceeds 15%.

In regressions (7) to (9), the F-statistic on the excluded instruments is 36.23, well beyond the canonical 10% and implying the lowest possible maximum biases as of Stock and Yogo (2005). The overidentification test cannot reject the null of instrument validity, while the underidentification test does reject, signalling instrument relevance. Accordingly, it appears that our IV strategy is valid. Compared to the OLS estimates presented in columns (4) to (6), the IV estimates of columns (7) to (9) yield very comparable results. Kyoto decreases domestic emissions by about 8%, does not affect the carbon footprint, and drives up net carbon imports by about 11 percentage points.

Columns (10) to (12) implement specifications with additional controls. These more complete regressions are our preferred specifications. Due to its massive economic catching-up coupled with huge emission increases (China's major energy source is coal which is comparatively CO_2-intensive), China plays an important and special role in the climate policy debate. To capture this, we include a China dummy. In both the emissions and the footprint equation that dummy is positive and statistically significant at the 1% level. *Ceteris paribus*, China's emissions are 30% and its carbon footprint 18% higher than the rest of the world average. Including a control for the degree of democracy of a country's political system (Polity) appears to affect emissions, footprints and net carbon imports positively,

[32] We also compute an underidentification test of whether the equation is identified, i.e., that the excluded instruments are relevant (correlated with the endogenous regressors). The null hypothesis is that the equation is underidentified. The Kleibergen and Paap (2006) statistic is heteroskedasticity-robust. That test always rejects with p-values lower than 0.01, so that we do not report it to save space.

carbon footprints being most strongly affected. Reforms that increase the democratic stature of countries often also spur growth. The log of the stock of other (than Kyoto) multilateral environmental agreements is meant to proxy for countries' environmental awareness. It does not exert a measurable effect, but it is important to note that there is very little time variation in this variable. Finally, we control for EU membership. Domestic emissions are affected negatively, the carbon footprint and net carbon trade are not affected. These additional controls change the estimated Kyoto effects only very slightly: Kyoto decreases domestic emissions by about 7%, the point estimate on the CO_2 footprint increases to 0.06, but the effect is still statistically insignificant (p-value 0.20). The production effect minus the absorption effect approximates the effect on net imports which is now at 14 percentage points.[33]

Summarizing, our benchmark IV regressions suggest that Kyoto commitments have a measurable negative effect on CO_2 emissions, but leave the CO_2 footprint unchanged relative to the counterfactual situation. Kyoto has affected firms – who have reduced emissions, possibly by outsourcing production to non-committed countries – but not consumers – who have not changed their consumption habits.

2.5 Robustness checks

The remaining analysis in this paper discusses a wide array of robustness checks ranging from using different country samples to applying alternative IV strategies and treatment windows. Results always compare to columns (10) to (12) of our benchmark Table 2.4. The thrust of our argument continues to hold: Kyoto has led to increased net carbon imports in committed countries but has not reduced carbon footprints. Results are summarized in Table 2.5; full regression output is available in Appendix B and at JEEM's online repository of supplemental material, which can be accessed from the website `http://aere.org/journals`.

[33] Expressing net carbon imports relative to domestic carbon footprints or refraining from any normalization leads to similar signs and levels of statistical significance.

2.5.1 Alternative samples

Excluding China. Panel A of Table 2.5 varies the sample of countries that underly the regressions. In columns (A1) to (A3), we drop China from the sample.[34] One could easily imagine that China's special situation, also due to its entry into the WTO in 2002, drives the pattern discovered in our benchmark regressions. However, this is not what we observe. While the positive effect of Kyoto commitment on domestic emissions becomes less pronounced (now standing at about 5%, measured only at the 10% level of significance (p-value of 0.08)), the carbon footprint of countries now turns out to be positive and larger than without China in the sample but still statistically insignificant (now at about 7% with a p-value of 0.14). As a consequence, Kyoto pushes net imports of carbon up by 13 percentage points. As shown by the overidentification and the weak instruments test, our IV strategy remains valid.

Excluding transition countries. Columns (A4) to (A6) exclude Germany, Romania, Poland, and Slovakia from the sample. These countries have inherited a substantial industrial production base from formerly centrally planned economies and have also reduced domestic emissions by at least one percent per year (see Table 2.1). It is often argued that the small overall success of the group of committed countries is an artifact of those transition countries' industrial restructuring, as heavily polluting old plants were replaced by more efficient ones. However, this does not seem to drive our results. Note that our IV strategy identifies the effect of Kyoto against the counterfactual of no Kyoto and not against any specific business-as-usual trajectory. Excluding those transition countries largely confirms our benchmark results: Kyoto has lowered domestic emissions by about 7% (p-value 0.04), leaves the carbon footprint unchanged (with an estimate of 6% and a p-value of 0.19), and increased net carbon imports by about 14 percentage points (p-value 0.00). In all regressions, the F-statistic on excluded instruments remains high (39.07), and the other first-stage diagnostics signal validity of our strategy.

Excluding all ex-communist countries. Finally, columns (A7) to (A9) exclude all eight ex-communist countries from the sample. This decreases the sample size quite a bit and makes inference harder. Also the quality of our instruments is affected. The F-statistic

[34] Note that we drop China from the regression sample, but we do not recalculate the carbon footprints with China in the ROW for this robustness check.

Table 2.5: Robustness checks – Summary table

Panel A: Alternative samples									
Sample:	China excluded			DEU,ROU,POL,SVK excl.			Ex-communist countries excl.		
Dep. var.:	emission	footprint	imports[a]	emissions	footprint	imports[a]	emissions	footprint	imports[a]
	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)	(A7)	(A8)	(A9)
Kyoto (0,1)	-0.05*	0.07	0.13***	-0.07**	0.06	0.14***	0.01	0.12**	0.13**
	(0.03)	(0.05)	(0.04)	(0.03)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)
Over-ID (p)	0.23	0.17	0.25	0.41	0.47	0.13	0.25	0.07	0.14
Weak-ID (F)	35.55	35.55	35.55	39.07	39.07	39.07	13.80	13.80	13.80
Panel B: Alternative IV strategies									
IV strategy:	ICC variable only (LIML)			Lagged population growth			Selection probability		
Dep. var.:	emission	footprint	imports[a]	emissions	footprint	imports[a]	emissions	footprint	imports[a]
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)	(B7)	(B8)	(B9)
Kyoto (0,1)	-0.14**	0.02	0.21**	-0.06*	0.06	0.13***	-0.08**	0.04	0.13***
	(0.07)	(0.06)	(0.10)	(0.03)	(0.05)	(0.04)	(0.03)	(0.04)	(0.03)
Over-ID (p)	0.64	0.33	0.19						
Weak-ID (F)	4.33	4.33	4.33	95.57	95.57	95.57	54.73	54.73	54.73
Panel C: Alternative dependent variables									
Definition:	variables per unit of GDP			Fixed I-O tables (2000)			US I-O table for ROW		
Dep. var.:	emission	footprint	imports[a]	footprint	footprint[b]	imports[a]	footprint	footprint[b]	imports[a]
	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)	(C7)	(C8)	(C9)
Kyoto (0,1)	-0.07**	0.03	0.10***	0.04	0.01	0.11**	0.06	0.03	0.14***
	(0.03)	(0.04)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
Over-ID (p)	0.30	0.41	0.32	0.16	0.32	0.18	0.22	0.43	0.18
Weak-ID (F)	36.00	36.00	36.00	34.39	36.00	34.39	34.39	36.00	34.39
Panel D: Alternative treatment windows									
Window:	narrow: 2002 only			broad: 2001-04			treatment at start of 2005		
Dep. var.:	emission	footprint	imports[a]	emission	footprint	imports[a]	emission	footprint	imports[a]
	(D1)	(D2)	(D3)	(D4)	(D5)	(D6)	(D7)	(D8)	(D9)
Kyoto (0,1)	-0.05*	0.05	0.11***	-0.08**	0.06	0.15***	-0.08**	0.03	0.12**
	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)
Over-ID (p)	0.15	0.25	0.12	0.18	0.23	0.12	0.62	0.50	0.22
Weak-ID (F)	33.66	33.66	33.66	33.73	33.73	33.73	8.70	8.70	8.70

Note: N=40 countries, T=2. First-differenced IV regression. Default specification: pre-treatment average (1997-2000), post-treatment average (2004-07); excluded instruments for Kyoto variable: ratification status of ICC treaty, spatial lag thereof, and lagged growth rate of population; All regressions include full list of covariates and a constant (not shown). Full regression output in Tables B.3 to B.6 in Appendix B. Standard errors and first-stage diagnostics are heteroskedasticity-robust and finite-sample adjusted. * $p<0.1$,** $p<0.05$, *** $p<0.01$.
[a] Net carbon imports as a share of domestic carbon emissions. [b] Footprint per GDP.

on excluded instruments falls to 13.80, which is, however, still above the alert level of 10. Compared to our benchmark regressions, the lower F-statistic implies that the maximum IV bias relative to the OLS endogeneity bias is now 10% rather than 5% according to the Stock and Yogo (2005) critical values. In terms of results, Kyoto no longer has a measurable impact on domestic CO_2 emissions. However, Kyoto now increases the footprint by about 12%, statistically significant at the 5% level (p-value 0.02). The increase in the carbon footprint is now solely driven by an increase in net carbon imports of 13 percentage points. The results obtained by excluding the ex-communist countries yield the most pessimistic picture possible: Kyoto appears to have triggered delocation of production to dirtier countries without giving rise to emission savings in committed countries.

2.5.2 Alternative IV strategies

Using ICC instruments only. In our benchmark regressions, we have used three instruments for the Kyoto dummy: ICC membership, its spatial lag, and lagged population growth. In Panel B of Table 2.5, we assess whether this choice of instruments influences the results. Columns (B1) to (B3) use only the ICC variables as instruments. Stock and Yogo (2005) propose to use *limited information maximum likelihood* (LIML) instead of IV to reduce a possible bias due to weak instruments. The first-stage diagnostics show that the overidentification (and underidentification test, not reported) yield satisfactory results. The F-statistic on excluded instruments, however, is now only 4.33. This is lower than the Staiger and Stock (1997) 2SLS rule of thumb which requires a minimum value of 10 for a strong instrument.[35] Stock and Yogo (2005) show that this rule is too conservative with LIML estimation. Their tabulations imply that the true power of the F-test is 25%, which is still large.[36] This IV strategy biases the absolute value of Kyoto estimates upwards. The pattern discovered in our benchmark table, however, remains intact: Kyoto reduces domestic emissions, increases carbon imports, and has no effect on the carbon footprint.

Lagged population growth as only instrument. Next, we use a single instrument only, namely lagged population growth (columns (B4) to (B6)). The F-statistic on the excluded instrument is large, so that the instrument appears strong. The idea of the instrument is that lagged population growth correlates with countries' willingness to commit to climate goals, but not to current emission growth. The effect of current population growth on emission increases is captured by expressing the dependent variables in per capita terms. This IV strategy yields estimates of the Kyoto effect close to the benchmark IV estimates.

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[35] The maximum relative bias test cannot be performed in these regressions since the equations are not "sufficiently" overidentified, see Stock and Yogo (2005).

[36] Though, dropping variables that are insignificant in the first and second stage reduces the maximal LIML bias to 10-15%, while the sign pattern and coefficients are comparable to the IV benchmark.

Wooldridge two-step procedure. Finally, columns (B7) to (B9) apply a procedure proposed by Wooldridge (2002, p. 623 f.). It consists in estimating the binary response model (3) by maximum likelihood (Probit),[37] and obtain the fitted probabilities $\hat{\Pi}$. The variable $\hat{\Pi}$ is then used as an instrument in a standard IV approach. The F-statistic on the excluded instrument is 54.73, so that the instrumental variable appears strong. We find again that Kyoto commitment reduces domestic emissions (by about 8%), has no effect on the carbon footprint, and increases net imports of CO_2.

2.5.3 Alternative definitions of the dependent variables

Carbon intensities. Panel C of Table 2.5 varies the definition of the dependent variables. In columns (C1) and (C2), emissions are relative to GDP. As shown by the first-stage diagnostics, this modification keeps the IV strategy intact. Compared to the benchmark regressions, the sign pattern of coefficients is fairly similar. Instead of including GDP, we use GDP per capita whenever the dependent variable is in per GDP terms. It turns out that higher GDP per capita has a strong negative influence on emission intensities. Richer countries have higher emissions per capita, but lower emission intensities (see also Cole and Neumayer, 2004). The effect of Kyoto commitment on the CO_2 intensity of production is negative and statistically significant at the 5% level (p-value 0.03): Kyoto reduces that intensity by 7%. In line with our results on emissions per capita, Kyoto has no measurable effect on the CO_2 intensity of absorption; the point estimate is positive but statistically insignificant (p-value 0.45). Net carbon imports are again positively affected.

Computing carbon footprints holding I-O tables fixed. Columns (C4) to (C6) apply a different method in calculating the carbon footprint. Rather than using new I-O tables when they are available, they are now held fixed to the year 2000. This modification has no importance for measured domestic CO_2 emissions, but affects the calculation of the carbon footprint and net carbon imports. In column (C4), the carbon footprint is expressed in per capita terms; in column (C5) it is expressed in CO_2 intensity terms. In both cases, the estimated effect of Kyoto is positive but statistically zero (p-values of 0.48 and 0.82, respectively). Coefficients on controls do not change much relative to the benchmark

[37] Using a linear model yields comparable results, but the obtained instrument is somewhat less powerful. The choice of a non-linear selection model helps with identification of the Kyoto effects.

regressions. Column (C6) shows that Kyoto still exerts a positive, statistically significant effect on net carbon imports (p-value of 0.02), comparable in size to the benchmark estimates.

Alternative treatment of rest-of-the-world. In the benchmark regressions, we treat the technology matrix of the RoW aggregate as an average over observed countries.[38] In the robustness checks presented in columns (C7) to (C9), we instead assume that the RoW has the U.S. technology matrix. Assuming U.S. technology has some tradition in the empirical factor content of trade literature (see Feenstra (2004) for a survey) and often has important implications for results. In the present context, however, this assumption makes little difference to the interesting coefficients: Kyoto has neither an effect on countries' per-capita carbon footprint (column C7) nor a measurable effect on absorption per GDP (column C8). Net carbon imports (column C9) are still affected positively.

2.5.4 Alternative treatment windows

In the benchmark regressions, we defined the treatment window to comprise the years 2001-03. In this window, most countries have, if at all, ratified the Kyoto Protocol. In Panel D of Table 2.5 we perform robustness checks pertaining to this choice. We keep pre- and post-treatment windows of similar length.[39]

Narrow treatment window. We start by looking at the results when we define the pre-treatment period to be 1997-2001 and the post-treatment period to be 2003-07. The treatment window, i.e., the period over which the Kyoto dummy switches from zero to unity is then confined to the year of 2002.[40] Columns (D1) to (D3) of Panel D show that the Kyoto effects on emissions, footprints, and net imports are very much in line with the benchmark results; also the coefficients on covariates vary only a bit.

Broad treatment window. Next, we define the pre-treatment period to be 1997-2000 and the post-treatment period to be 2005-07. The resulting wide treatment window now

[38] See Section 2.2 for details.

[39] In principle, we could define the pre-treatment window always as starting in 1995. We have tried this in additional robustness checks: results do not change. However, we prefer to compute averages over symmetrically defined periods.

[40] Switzerland, Romania and Russia are still coded as treated.

comprises all ratifications (except that of Australia). The sign pattern obtained from regressions presented in columns (D4) to (D6) compares well to the benchmark results.

Treatment at start of year 2005. Finally, we assume that treatment started in the beginning of the year 2005 when the Kyoto Protocol formally entered into force.[41] The pre-treatment period then is 1997-04 while the post-treatment period is 2005-07.[42] With this definition and using LIML, the observed pattern remains. Note that with an F-statistic of 8.70, the maximal LIML bias is still 10%.

2.5.5 Additional robustness checks

We have also experimented with a balanced panel of yearly observations for 1997-2007. Results are reported in Table B.7 in Appendix B. The first 6 columns use the within transformation to control for unobserved time-invariant country-specific determinants of emissions. Columns (1) to (3) present OLS estimates, while columns (4) to (6) apply our benchmark IV strategy to this setup. Not surprisingly, with dramatically increased degrees of freedom (we now have 440 observations), it is possible to calculate Kyoto effects at higher statistical precision. The OLS estimates suggest that Kyoto has decreased domestic CO_2 emissions by about 3%, left the carbon footprint unchanged, and led to higher net carbon imports by 6 percentage points. The signs of the covariates are sensible; note that our proxy for environmental awareness (the number of MEAs other than Kyoto ratified by a country) now reduces the carbon footprint. Turning to IV estimates in columns (4) to (6), the sign pattern of Kyoto coefficients is preserved. The point estimates increase in absolute size. The most notable difference to the long diff-in-diff benchmark is the coefficient on footprints. It is now estimated to be 0.05, statistically significant at the 5% level (p-value 0.02). Instrumenting does not alter the estimated coefficients on covariates much. The IV strategy appears valid for emissions and footprints, with the over- and underidentification tests yielding good results and the F-statistic on excluded instruments at 82.01. The

[41] The Protocol became legally binding after Russia's ratification pushed the share of world emissions as of 1990 covered by Kyoto over the 55% threshold. The EU, Japan, and Canada and other countries had declared earlier on that they would treat the emission reduction targets as binding even in the absence of Russia's ratification.

[42] Our instruments appear to be too weak in this case when working with symmetric periods (i.e. a pre-treatment period of 2002-04).

overidentification test signals endogenous instruments in the case of the net CO_2 import share.[43]

Summarizing, we conducted several robustness checks to make sure that no particular assumption, instrumentation strategy or country group drives our results. Neither excluding China, varying the set of instruments, the assumption of when treatment starts or the applied estimator changes the empirical pattern we find: Kyoto has led to increased net carbon imports in committed countries but has not reduced carbon footprints.

2.6 Conclusion

We have estimated the effect of Kyoto commitments on domestic CO_2 emissions, carbon footprints and net carbon imports. We have done so by exploiting a newly constructed panel data set of yearly observations from 1995-2007 for 40 countries. Our inference is based on the differences between committed and non-committed countries over two time periods: a pre-treatment period of 1997-2000 and a post-treatment period of 2004-2007. This differences-in-differences approach is demanding as it is effectively based on a cross-section of only 40 rates of change. We use an IV strategy that exploits correlation between countries' commitment to Kyoto and that to the International Criminal Court, as well as lagged population growth. We find a robust pattern in the data: On average, Kyoto commitment has reduced domestic emissions by about 7%. It has not consistently affected the carbon footprint. The difference between production and absorption being made up by international trade, Kyoto commitment has increased the ratio of net carbon imports over domestic emissions by about 14 percentage points.

Our results imply that the Kyoto Protocol has given rise to substantial relocation of production (carbon leakage). Committed countries have reduced their emissions relative to the counterfactual of no Kyoto, but they have not reduced their carbon footprints. It follows that the Kyoto Protocol, due to its incomplete coverage, has been ineffective or possibly even harmful for the global climate. It has imposed substantial costs on firms and consumers in committed countries, but the return of all these efforts – lower global carbon emissions – has been statistically indistinguishable from zero. Our results lend empirical support to the

[43] Estimation based on first-differenced yearly data is less successful. The OLS model does not reveal any impact of Kyoto commitment on outcome variables. The IV model resurrects the sign pattern that we have seen throughout the tables of this paper (domestic emissions down, footprint unchanged, net imports up), but instruments appear too weak in the context of yearly differenced data.

case that non-global climate policy efforts like the Kyoto Protocol bear very little promise. Either future global climate deals have to cover all major economies, or committed countries could apply border tax adjustments to target footprints and thus contain the carbon leakage problem. Though finding the correct carbon tariff rate – which guarantees that a unit of CO_2 has the same price wherever it is emitted – will be very challenging. Bearing the MRIO methodology in mind, it implies huge informational requirements since countries are very heterogeneous with respect to their industrial structures and emission intensities.

Chapter 3

Estimating the Effects of Kyoto on Bilateral Trade Flows Using Matching Econometrics*

3.1 Introduction

The Kyoto Protocol – signed in 1997, entered into force in 2005 – assigns emission ceilings to industrial countries relative to their 1990 emission levels in the period 2008-12. Yet, it covers less than half of anthropogenic greenhouse gas (GHG) emissions because developing countries including major polluters like China and India are exempt *en bloc* and the U.S. did not ratify the treaty. As a result Kyoto countries' politicians fear for the competitiveness of their (energy-intensive) industries. They argue that increased costs of GHG emissions due to Kyoto would put Kyoto countries' industries at a comparative disadvantage. This was indeed the reasoning given by the U.S. for not ratifying the Kyoto Protocol. And it is the reason why Canada recently pulled out of the treaty. Classical trade theory suggests that, in a globalized world, (GHG-intensive) industries should increasingly produce in non-Kyoto countries and export their products to emission-constrained Kyoto countries.[1] So the

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* This chapter is based on joint work with Gabriel Felbermayr. It is based on the article "Estimating the Effects of Kyoto on Bilateral Trade Flows Using Matching Econometrics", in: *The World Economy*, 2013, 36(3), 303-330. This is a revised version of our working paper that circulated under Ifo Working Paper No. 119, 2011.

[1] This entails potentially detrimental effects for the environment. Emission savings in Kyoto countries are at least partially offset, when the possibility to trade leads to the relocation of production (and thus emissions) to non-Kyoto countries due to Kyoto commitment ("carbon leakage").

question arises whether the Kyoto Protocol actually had an impact on trade patterns. We will address the issue by investigating bilateral export flows.

The analysis of competitiveness issues seems crucial for the design of future climate agreements. At the moment it seems politically infeasible to reach a global deal. Potential Kyoto follow-ups would only apply to a sub-group of countries. If it turns out that trade flows react to differentials in climate policy, policymakers should think of ways to address the issue. One instrument to level the playing field currently debated, and for example advocated by French president Sarkozy, is the use of carbon-related border tax adjustments (BTA).

Related literature. The *ex-ante* analysis of competitiveness effects of unilateral climate policy is typically addressed in computable general equilibrium (CGE) models. Babiker (2005) uses a model with increasing returns to scale and an Armington demand system. He finds competitiveness effects for an OECD emission cap, but the extent of locational effects depends on the assumed market structure. Manders and Veenendaal (2008) use a different model and find only modest competitiveness effects from a policy to reduce emissions in the European Union in 2020 to 20 percent below 1990 levels when accompanied by a BTA. In contrast, Babiker and Rutherford (2005) model the Kyoto Protocol in a CGE framework and find more substantial competitiveness effects. Recent work focuses on border tax adjustments as remedies to the competitiveness and carbon leakage problem. Mattoo et al. (2009) highlight how carbon-related BTAs could harm developing economies. The most recent paper, by Elliott et al. (2010), investigates trade in carbon and finds substantial carbon leakage ranging from 15 percent at low tax rates to over 25 percent for the highest tax rate. *Ex-post* analyses of trade effects of environmental policy mostly embed a measure of environmental stringency in the gravity framework (see, e.g. Jaffe et al., 1995; Ederington et al., 2005; Levinson and Taylor, 2008). Studies on climate policy are, however, scant. A study by the World Bank (2008) finds no significant competitiveness effects of carbon taxes on trade flows of energy-intensive goods. Aichele and Felbermayr (2011a) derive a gravity equation for the carbon content of trade. Their study suggests that Kyoto commitment on average leads to increased carbon imports in committed countries, thereby leading to leakage. Based on aggregate data and on a different way to deal with self-selection of countries into the Protocol, Aichele and Felbermayr (2012) confirm these findings.

We contribute to the literature in the following ways. First, we use a different empirical methodology that combines differences-in-differences estimation with matching techniques to account for the endogeneity of Kyoto commitment. Second, beyond assessing the average treatment effect (ATE) of Kyoto commitment, we provide an estimate of the average treatment effect on the treated (ATT). From a policy perspective, this is the relevant estimate since it informs about how *Kyoto countries'* exports – and not an average country's exports – have reacted to their Kyoto commitments so far. And finally, conducting a sectoral analysis of Kyoto's effect on exports allows identifying which sectors' trade flows are affected by the Kyoto Protocol and which are not.

Our empirical approach is motivated by theoretical and empirical work on the economic fundamentals driving international environmental agreement (IEA) and particularly Kyoto membership. Since ratification of the Kyoto Protocol is a political process, it is certainly not random. The empirical literature typically distinguishes economic, political and environmental determinants of IEAs (see Murdoch and Sandler, 1997; Beron et al., 2003; Egger et al., 2011a, for examples). GDP or GDP per capita are important variables. York (2005) stresses demographic change as predictor of Kyoto ratification. And also free-riding on other's efforts might matter (Murdoch and Sandler, 1997; Carraro and Siniscalco, 1998). Egger et al. (2011a) show that a country's trade openness affects its probability to sign IEAs. Finally, the Kyoto status of important trade partners might matter, as in the U.S.-China case. This is the basis for our empirical model to estimate the likelihood of self-selection into Kyoto. The same fundamentals that determine selection into the Kyoto Protocol also drive trade patterns (see Bergstrand, 1989; Eaton and Kortum, 2002; Anderson and van Wincoop, 2003, for seminal contributions in the gravity literature). In this case, matching techniques are well suited to get an unbiased estimate of the ATT. Although matching is typically used to study effects of, for example, job training programs on labor market outcomes, several studies apply matching in the gravity context (see Persson, 2001; Chintrakarn, 2008; Egger et al., 2008; Baier and Bergstrand, 2009b, for examples).[2] Fewer studies use matching techniques to estimate the effect of environmental policies. List et al. (2003) employ a differences-in-differences matching estimator to analyze the effects of environmental air

[2] Matching is a promising strategy in the gravity context, because it allows matching on relative measures. The sheer number of country pair observations makes it likely to find an appropriate clone (in terms of joint GDP and distance etc.) for a country pair. This is certainly easier and more credible than performing matching for countries. Arguably, it is impossible to find a clone, say, for the U.S.

quality regulation on plant birth within New York state counties. Millimet and List (2004) extend the study by analyzing heterogeneity in the ATTs for county characteristics.

For our sample of 117 exporters – of which 34 have Kyoto commitments – the ATE estimates suggest bilateral exports are reduced by 15-20% due to Kyoto commitment. The ATT estimates imply the average treatment effect for a *Kyoto* country was 13-14% only. So our results highlight that not accounting for self-selection overstates the negative effect of Kyoto commitment. We report heterogeneity of Kyoto's treatment effects across sectors. Some sectors, e.g. iron and steel, organic and inorganic chemicals, plastics and also machinery and equipment exhibit substantial negative trade effects; while Kyoto countries even expanded exports in some sectors, e.g. travel goods and handbags or footwear. For about half of the products (27 out of 51 SITC product classes) we cannot identify significant effects, however. Consistent with theory, energy-intensive industries and sectors producing homogeneous goods are more strongly affected by negative competitiveness effects.

The rest of the paper proceeds as follows. Section 3.2 discusses our empirical strategy and data. Section 3.3 presents our results and robustness checks. Section 3.4 contains an analysis of competitiveness effects on the sectoral level. Section 3.5 concludes.

3.2 Empirical strategy and data

We are interested in how Kyoto commitment – i.e. the commitment to an emission cap under the Kyoto Protocol – affects the exports of Kyoto countries. The unit of analysis is a country pair, i.e. an exporter-importer dyad (possibly at the industry level), indexed by $i = 1, \dots, N$. Let $D_{it} \in \{0, 1\}$ be a treatment dummy that takes on the value of one if country pair i's exporter has a Kyoto commitment in period t and zero else. Working with a Kyoto dummy is certainly a crude assumption because the intensity of Kyoto commitment might differ across countries. Nevertheless, this approach is common in the treatment evaluation literature, see e.g. the literature on treatment effects of free trade agreements (FTAs) (Baier and Bergstrand, 2007), currency unions (Baldwin and Taglioni, 2007) or other international environmental agreements (Ringquist and Kostadinova, 2005; Aakvik and Tjøtta, 2011). We assume treatment starts with ratification of Kyoto commitment in national parliaments. The notion is that once ratification takes place, governments adjust their policies and economic subjects adjust their expectations. This assumption is also common in the evaluation of other international environmental agreements such as the Helsinki Protocol regulating sulfur dioxide emissions (Ringquist and Kostadinova, 2005). In a robustness check, we use the Kyoto Protocol's entry into force in 2005 as an alternative treatment date.

Let Y_{it} denote the outcome variable of interest: country pair i's value of bilateral exports in period t in logs (default sample). In a reduced sample, we restrict attention to exports to non-Kyoto countries. This amounts to evaluating the effect of *differential status* in trade partners' Kyoto commitments. Y_{it} is determined by Kyoto status and a vector of standard gravity covariates $\mathbf{X}_{it}$ including the log of GDPs, the log of bilateral trade costs proxied by joint FTA, WTO and EU membership, and multilateral resistance terms. Bilateral export flows could also be affected by unobservable influences. These might include differences in endowments, geographic location or climatic conditions, culture and also preferences. Let u_i be country-pair specific, time-invariant determinants of exports. The log gravity equation can then be written as

$$Y_{it} = \gamma D_{it} + \mathbf{X}_{it}{}'\beta + u_i + \alpha_t + \epsilon_{it} \tag{3.1}$$

where α_t is a common time trend and ϵ_{it} is an i.i.d. error term. The coefficient of interest is Kyoto's treatment effect γ.

3.2.1 Self-Selection into Kyoto Commitments: Problems and Cures

A complication arising in the estimation of γ is self-selection into treatment. Kyoto membership is the outcome of a political process and therefore not random. When selection is on time-invariant unobservables like differences in climatic conditions or endowments in fossil fuels, differences-in-differences (DID) estimation eliminates u_i from equation (3.1) and recovers Kyoto's treatment effect.[3] Yet, the likelihood of Kyoto commitment is influenced by economic fundamentals also affecting bilateral trade flows. Economic size and economic growth are important determinants, as well as GDP per capita. York (2005) stresses the importance of demographic factors for Kyoto ratification. Rose and Spiegel (2009) document that signing bilateral environmental agreements positively influences bilateral cross asset holding. The reasoning is that commitment under an environmental treaty reveals a country's time preference. So commitment in the environmental arena signals trustworthiness and furthers cooperation in other international forums. And Egger et al. (2011a) show that trade openness positively affects the number of international environmental agreements a

[3] See a similar discussion for self-selection into FTAs in Baier and Bergstrand (2007).

country signs. These arguments suggest that treated and untreated country pairs may differ with respect to their economic fundamentals. Thus, they might differ in their willingness to commit to Kyoto and be differently affected by Kyoto commitment. It implies that the treatment effect for an average country differs from the average treatment effect on the treated. As argued above, the ATT is the relevant indicator of how Kyoto commitment has affected Kyoto countries' exports.

Selection on observable covariates suggests the use of matching econometrics.[4] The basic idea of the matching method is to find untreated units that are similar to treated ones in terms of their covariates (also called matching variables) except for treatment status, and thus establish experimental conditions. For a survey, see e.g. Blundell and Costa Dias (2009) or Imbens and Wooldridge (2009). In the matching language, each unit has two potential outcomes $Y_i(D_i)$ depending on treatment status. The average treatment effect (ATE) is the average difference between treated and untreated outcome, and the ATT is the average difference between treated and untreated outcome conditional on treatment

$$\begin{aligned} ATE &= \mathbb{E}\left[Y_i(1) - Y_i(0)\right], \\ ATT &= \mathbb{E}\left[Y_i(1) - Y_i(0) \mid D_i = 1\right], \end{aligned} \tag{3.2}$$

where $\mathbb{E}$ is the expectation operator. In actual data however, we can only observe one of the potential outcomes. Either a unit is treated or it receives no treatment. Matching econometrics infers the missing counterfactual by the outcome of country pairs j in the properly constructed control group. The critical assumptions are that for every treated observation with $X_i = x$ there has to be at least one untreated observation with $X_j = x$ (*overlap assumption*) and once we control for covariates X treatment is randomly assigned (*ignorability assumption* or *selection on observables*). A simple estimator of the ATT in a very general form is

$$\hat{ATT} = \frac{1}{NT} \sum_{i \in D_i = 1} \left[Y_i - \sum_{j \in D_j = 0} w_{ij} Y_j \right], \tag{3.3}$$

[4] Several studies use cross-section matching techniques in a gravity context. Baier and Bergstrand (2009b) find that matching econometrics helps to get economically plausible and more stable estimates of FTAs' effects on trade flows. In a similar vein, Persson (2001) and Chintrakarn (2008) use propensity-score matching to estimate the trade effects of currency unions.

where w_{ij} is the weight assigned to country pair j in the control group being matched with country pair i and NT is the number of treated country pairs.[5]

One way to construct the control group and respective weights is based on the *Mahalanobis metric* (one-to-one matching, k nearest neighbors). The Mahalanobis metric exploits the Euclidean distance in matching variables between i and j, $\|X_i - X_j\|$. With one-to-one matching the untreated country pair j for which the Mahalanobis metric is smallest (i's nearest "neighbor") is chosen as control and receives a weight of one; for all other untreated pairs the weight is zero. Accordingly, in the case of k-nearest neighbor matching, the k closest neighbors are chosen as control group with $w_{ij} = 1/k$.[6] An alternative matching approach dates back to Rosenbaum and Rubin (1983) and matches on the *propensity score* (one-to-one, k nearest neighbor, kernel, radius matching). Treatment selection is modeled with a Probit or Logit model. We use the Probit specification as default but apply a Logit selection model as a robustness check. Country pairs are matched according to their probability of exporter's Kyoto commitment. Nearest neighbor matching again uses the k nearest neighbors, but now in terms of the propensity score. With kernel density matching, the control group comprises all untreated pairs with propensity scores in the neighborhood of i (defined by the bandwidth), where j receives a higher weight, the closer its propensity score is to i's. Finally, radius matching uses all untreated pairs with propensity score differences smaller than the specified radius.

The simple matching estimator is confounded in the presence of unobserved heterogeneity. However, the framework is easily extended to a DID setup with time-invariant unobservables (see e.g. Heckman et al., 1997; Blundell and Costa Dias, 2009). In its simplest version, there are two time periods: a pre-treatment period $t-1$ and a post-treatment period t. For a country pair receiving treatment, matches in the untreated group are found on the basis of pre-treatment period covariates $X_{i,t-1}$.[7] The ATT compares the differences between post- and pre-treatment outcomes between treated and control country pairs. Let lower letters

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5 The same logic applies to retrieve an estimate for ATE. The summation then is over all country pairs $i = 1, \ldots, N$ and the counterfactual outcome is recovered by matching. In the following, our representation focuses on ATTs but the respective ATEs can be estimated in a similar fashion.

6 With continuous matching variables, the ATT will have a conditional bias depending on sample size and number of covariates. Abadie and Imbens (2006) provide a bias-adjusted version that renders the estimator $N^{1/2}$-consistent and asymptotically normal.

7 Note that the basic DID matching estimator only allows for a common time trend in X_i changes, such that the pre- and post-treatment distribution of covariates remain unchanged.

indicate the difference, e.g. $y_{it} \equiv Y_{it} - Y_{i,t-1}$. So the DID matching estimator is

$$\hat{ATT}_t^{MDID} = \frac{1}{NT_t} \sum_{i \in d_{it}=1} \left[y_{it} - \sum_{j \in d_{jt}=0} w_{ij,t-1} y_{jt} \right]. \tag{3.4}$$

For example, Egger et al. (2008) apply the DID matching estimator to estimate the effect of regional trade agreements on trade structure and volume.

The DID matching estimator assumes that changes in the covariates $\mathbf{X}_i$ follow a common time trend. This assumption is not likely to hold in our context, thus creating a bias due to discrepancies in covariates. For example, non-Kyoto countries are predominantly developing countries experiencing higher growth rates in GDP and GDP per capita than Kyoto countries. *Regression-adjusted matching estimators* deal with this bias by correcting for changes in covariates, see Robins and Ritov (1997), Heckman et al. (1998), Imbens (2004) or Imbens and Wooldridge (2009) and Heckman et al. (1997) for a DID version. The correction typically is linear in covariates (Heckman et al., 1997) which results in the regression-adjusted DID matching estimator:

$$\hat{ATT}_t^{RMDID} = \frac{1}{NT_t} \sum_{i \in d_{it}=1} \left[(y_{it} - x_{it}{}' \beta_{t-1}) - \sum_{j \in d_{jt}=0} w_{ij,t-1} (y_{jt} - x_{jt}{}' \beta_{t-1}) \right], \tag{3.5}$$

where β_{t-1} stems from a regression of Y on X for the untreated in the pre-treatment period. This is equivalent to performing a DID estimation on equation (3.1) with weighted least squares (see e.g. Blundell and Costa Dias, 2009). The weights stem from propensity score or Mahalanobis matching on pre-treatment covariates as described above. The combination of matching and DID estimation has the advantage of generating a quasi-experimental data set and will take us a long way in reducing selection bias.

A last issue meriting attention is that countries' ratification of the Kyoto Protocol took place in different years. The first committed countries to ratify the Protocol were the Czech Republic and Romania in 2001. The bulk of Kyoto countries followed in 2002 and 2003 and late ratifiers include Australia and Croatia in 2007. We deal with this by analyzing averages of a pre- and post-treatment period.[8] Define a treatment period from 2001 to 2003 in which most countries ratified Kyoto. Pre- and post-treatment period are chosen to be symmetric 4-year windows around the treatment period, i.e. 1997-2000 and 2004-2007

[8] This is also the approach taken in Egger et al. (2008).

respectively. Note that using differences in average outcomes before and after treatment has the additional advantage of overcoming problems of autocorrelation in the data (see Bertrand et al., 2004).

3.2.2 The Choice of Matching Variables

Matching relies on the *ignorability assumption.* This assumption ensures that once we control for covariates treatment is random. Put differently, it reestablishes a data set as if from an experimental setup. So successful matching crucially hinges on the choice of matching variables. The appropriate matching variables are those that influence both the decision to select into treatment and the outcome of interest. However, there exists no test equivalent to a goodness-of-fit test for model selection in the matching context. Thus, we use theoretical insights from the public economics and gravity literature to guide our choice. We bilateralize all covariates. That is, we search for clones that are similar, e.g., in their joint GDP.

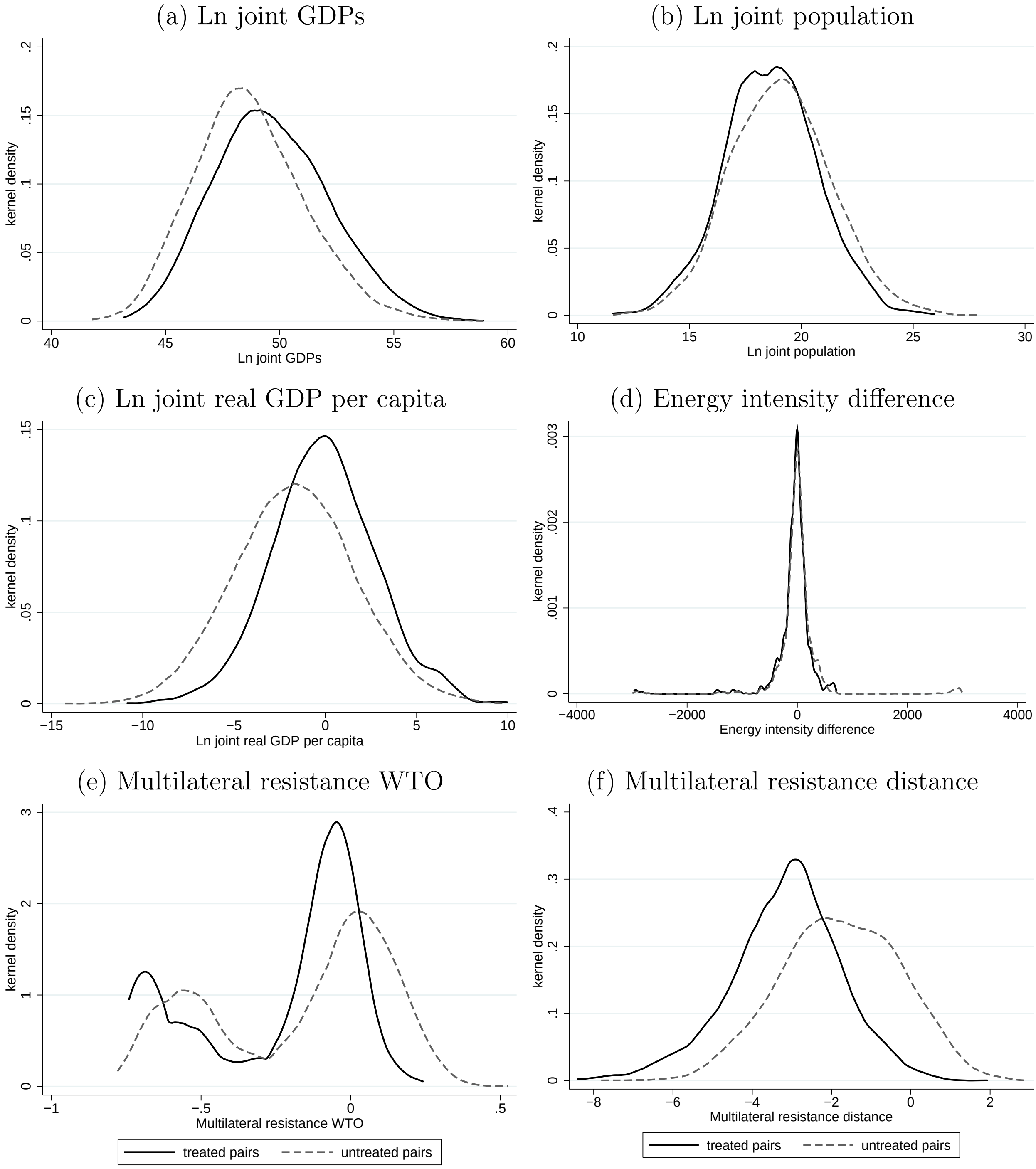
Bilateral exports are determined by market size of exporter and importer, carbon taxes, bilateral trade costs, price indices and production technology (see Anderson and van Wincoop, 2003; Aichele and Felbermayr, 2011a). Market size is measured by joint GDP and joint population size. GDP and population growth are also typical determinants of IEA membership (see e.g. Murdoch and Sandler, 1997; Beron et al., 2003; York, 2005). We capture technological differences in a country pair by the product of real GDP per capita (the growth literature shows that GDP per capita and technology are closely related) and emission intensity differences in a pair. These variables also matter for Kyoto selection. Advanced countries with a high GDP per capita might care more for environmental problems. Emission intensity on the other hand represents reliance on fossil fuels which reduces the likelihood for Kyoto commitment. Also trade openness matters for IEA membership (Egger et al., 2011a). Multilateral resistance (MR) is related to openness and captures how close a country pair is to all other trade partners in terms of distance and other trade cost measures such as joint WTO membership. So MR terms bear information on how easy it is to find other trade partners which is linked to competitiveness effects. Therefore, we include MR terms for FTA, joint EU and WTO membership and bilateral distance, contiguity and common language. We compute multilateral resistance terms as linear approximations to price indices as suggested by Baier and Bergstrand (2009a). They take the form $MR^V_{mx} = \sum_{k=1}^K \theta_k V_{mk} + \sum_{l=1}^K \theta_l V_{lx} - \sum_{l=1}^K \sum_{k=1}^K \theta_k \theta_l V_{kl}$ where m, x index the importer and exporter respectively, k and l are country indices, and θ_k is country k's share in world GDP. V comprises the log of bilateral distance and dummies for common language, contiguity, joint FTA, WTO and EU membership. In a robustness check, we will also add political controls to the matching variables (see Subsection 3.3.3 for details). A country's political institutions might influence how easy it is to ratify an international treaty in national parliament. And the political orientation might influence trade patterns.

There is no direct test whether the ignorability assumption holds. However, a *balancing test* proposed by Rosenbaum and Rubin (1985) is used to ensure that the distribution of covariates is the same for treated and control pairs. The test checks whether the differences in the mean of each covariate between treated and matched control country pairs is too large. The STATA routine also provides a measure of bias reduction (based on the differences in the mean of covariates between treated and untreated pairs). As pointed out in subsection 3.2.1, an additional prerequisite in matching is the overlap assumption. Since we have about 12,000 country pairs the overlap assumption is most likely fulfilled. Additionally, with propensity score matching, we drop observations outside the common support – i.e. treated country pairs with a propensity score higher than the maximum or lower than the minimal propensity score of untreated pairs.

Summarizing, our matching variables are log of joint GDP, log of joint population, log of joint real GDP per capita, the exporter's energy intensity minus the importer's energy intensity, and the six multilateral resistance terms. The list of covariates captures a broad spectrum of determinants of bilateral trade flows which are related to IEA membership. We hope this ensures that no variable is omitted that could confound the estimates.

Figure 3.1 shows that treated and untreated country pairs differ with respect to our matching variables. In Panel (a), the kernel density function of the log of the product of GDPs in a treated country pair (black solid line) is to the right of the untreated country pairs' kernel (gray dashed line). This indicates that treated country pairs jointly have larger markets. Panel (b) shows that treated country pairs are jointly smaller in population size than untreated ones, although the difference is not very distinctive. In Panel (c) the log of joint real GDP per capita is to the right of the one of untreated pairs. So treated pairs are jointly more advanced countries. The distribution of energy intensity differences does not differ much except for some outliers to the right (Panel (d)). Treated country pairs also differ with respect to how close they are to other WTO countries (Panel (e)) and they also tend to be geographically closer to other trade partners (Panel (f)). Additionally, a t-test rejects

Figure 3.1: Kernel densities before matching (pre-treatment period)



Note: The graph shows Epanechnikov kernel density functions of the matching variables for treated country pairs (i.e. the exporter is a Kyoto country in the post-treatment period) and untreated country pairs (i.e. the exporter is no Kyoto country in the post-treatment period) for the pre-treatment period 1997-2000.

the null of identical means between treated and untreated country pairs for all matching variables except the multilateral resistance term of contiguity (see Table 3.2).

3.2.3 Data Description

Bilateral export flows for the years 1990-2009 stem from the UN Comtrade database. We use total as well as sectoral export data. Sectoral bilateral exports comprise the 52 non-agricultural 2-digits SITC Rev. 3 commodities.[9] A country's nominal GDP, population and energy intensity are obtained from the World Development Indicator (WDI) 2010 database. Real GDP per capita is taken from the Penn World Tables (PWT) 7.0. Geographical variables and bilateral distance measures are taken from CEPII. Joint FTA membership comes from the WTO. The EU and WTO dummy are constructed from the homepage of the EU and WTO, respectively. Information on the Kyoto status of countries stems from the UNFCCC. A country is coded as Kyoto country when it has ratified the Kyoto Protocol and is listed in the Annex B to the Kyoto Protocol. So only countries that committed to an emission ceiling under the Protocol are Kyoto countries.

Our benchmark period is 1997-2007.[10] The data set comprises 117 exporters and 128 importers, 34 of which are Kyoto countries.[11] This gives a total of 12,139 country pairs or roughly 24,000 observations. 4,210 country pairs, i.e. about 35%, have a Kyoto exporter. In the reduced sample, we focus on exports into non-Kyoto countries. Here, we have roughly 17,000 observations. Of the 8,573 country pairs again around 36% of the exporters have Kyoto obligations. Table 3.1 provides summary statistics for the default sample.

Figure 3.2 shows sectoral differences between post- and pre-treatment period averages in the log of treated pair's real exports minus the log of untreated pair's real exports, i.e. the difference in the average real trade growth trend in treated versus untreated country pairs between these periods. Export flows are deflated with the exporter's GDP deflator taken from WDI 2010.[12] The dashed line indicates the overall trend. Kyoto countries' real exports on average grew by 44% between the pre- and post-treatment period. The respective growth for non-Kyoto countries was 35%. Hence, Kyoto countries' exports grew by roughly 9 percentage points more. Albeit the positive overall trend difference, 15 out of the 51 goods categories experienced less export growth if the exporter was a Kyoto country. The variation in sectoral trends is quite substantial. Iron and steel (goods category 67) displays the largest negative

[9] See Table 3.5 for a list with sector descriptions.

[10] We also run a robustness check on 1995-2009 data, but caution that the financial crisis starting in 2008 could bias the results if Kyoto and non-Kyoto countries were hit differently.

[11] Liechtenstein is not in our data set due to data availability. Australia and Croatia are coded as non-Kyoto countries because they ratified Kyoto at the end of our benchmark period, in late 2007.

[12] Using nominal instead of real export flows does not change the ordering of the goods categories.

Table 3.1: Summary statistics

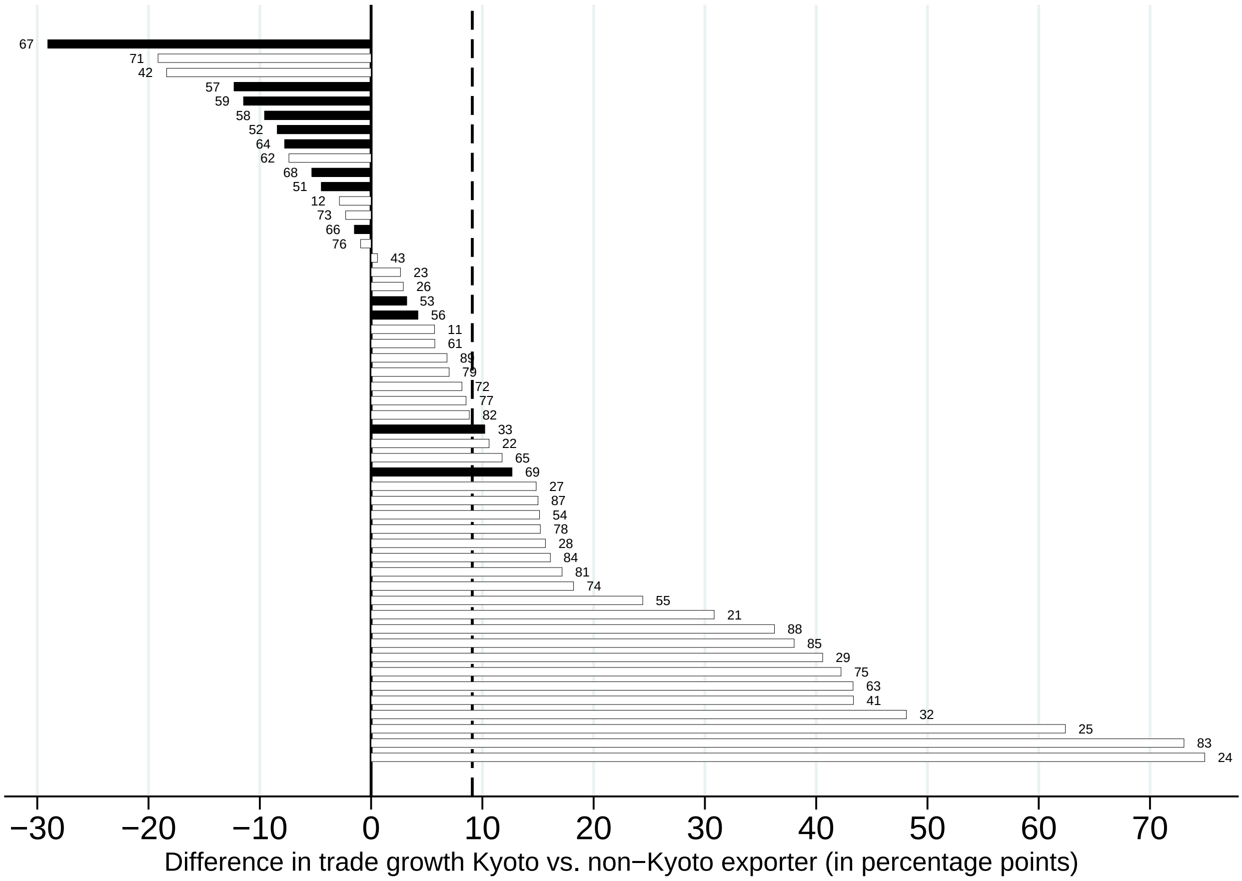
Period:		Pre-treatment		Post-treatment	
Variable	Obs.	Mean	Std. Dev.	Mean	Std. Dev.
Ln exports	12,139	15.53	3.40	16.39	3.42
Kyoto (0,1)	12,139	0.00	0.00	0.35	0.48
Gravity variables					
Ln GDP exporter	12,139	24.62	1.87	25.24	1.82
Ln GDP importer	12,139	24.38	1.94	25.01	1.87
Ln real GDP per capita exporter	12,139	-0.49	2.28	-0.35	2.29
Ln real GDP per capita exporter	12,139	-0.64	2.30	-0.52	2.30
FTA (0,1)	12,139	0.23	0.42	0.29	0.45
WTO (0,1)	12,139	0.63	0.47	0.77	0.41
EU (0,1)	12,139	0.02	0.13	0.05	0.21
MR FTA	12,139	-0.27	0.09	-0.35	0.11
MR WTO	12,139	-0.82	0.24	-1.08	0.24
MR EU	12,139	-0.02	0.03	-0.06	0.06
Emission intensity difference	12,139	-10.42	424.05	-30.22	541.08
Matching variables					
Ln joint GDP	12,139	49.00	2.51	50.25	2.43
Ln joint population	12,139	18.93	2.21	19.09	2.21
Ln joint real GDP per capita	12,139	-1.13	3.24	-0.87	3.24

Note: The table shows summary statistics for averages of the dependent, treatment, gravity control and matching variables for the periods before (1997-2000) and after (2004-2007) treatment in the default sample.

growth difference. Here, exports grew by 30 percentage points less for Kyoto exporters. Other energy-intensive goods categories (black bars) are also amongst the sectors affected most negatively by the exporter's Kyoto commitment.[13] For example, plastics in primary form (goods category 57) with -12 percentage points or chemical materials and products (goods category 59) with -11 percentage points less growth. Most of the energy-intensive goods categories experienced a below average growth trend. Other goods categories like cork and wood (goods category 24), travel goods, handbags and similar containers (goods category 83) or pulp and waste paper (goods category 25) had substantially more growth if the exporter committed to Kyoto. So Figure 3.2 suggests quite substantial effects of Kyoto commitment on a sectoral level, where energy-intensive goods categories are affected

[13] We follow the EU Commission and the U.S. Department of Energy (`http://www1.eere.energy.gov/industry/industries_technologies/index.html`) in classifying goods as energy-intensive.

Figure 3.2: Differences in pre- to post-treatment period sectoral real trade growth



Note: The graph shows the difference in average pre- to post-treatment real trade growth between country pairs with and without Kyoto exporter for all non-agricultural 2 digit SITC Rev. 3 goods categories. Black bars indicate energy-intensive goods. The dashed line at 9.09 denotes the average aggregate difference in trade growth, i.e. Kyoto exporters experienced about 9 percentage points more real trade growth than non-Kyoto exporters.

negatively. In Section 3.4 we will look into sectoral effects in more detail, but first we analyze overall trends in the following section.

3.3 Estimates of Kyoto's effect on exports

3.3.1 Balancing property

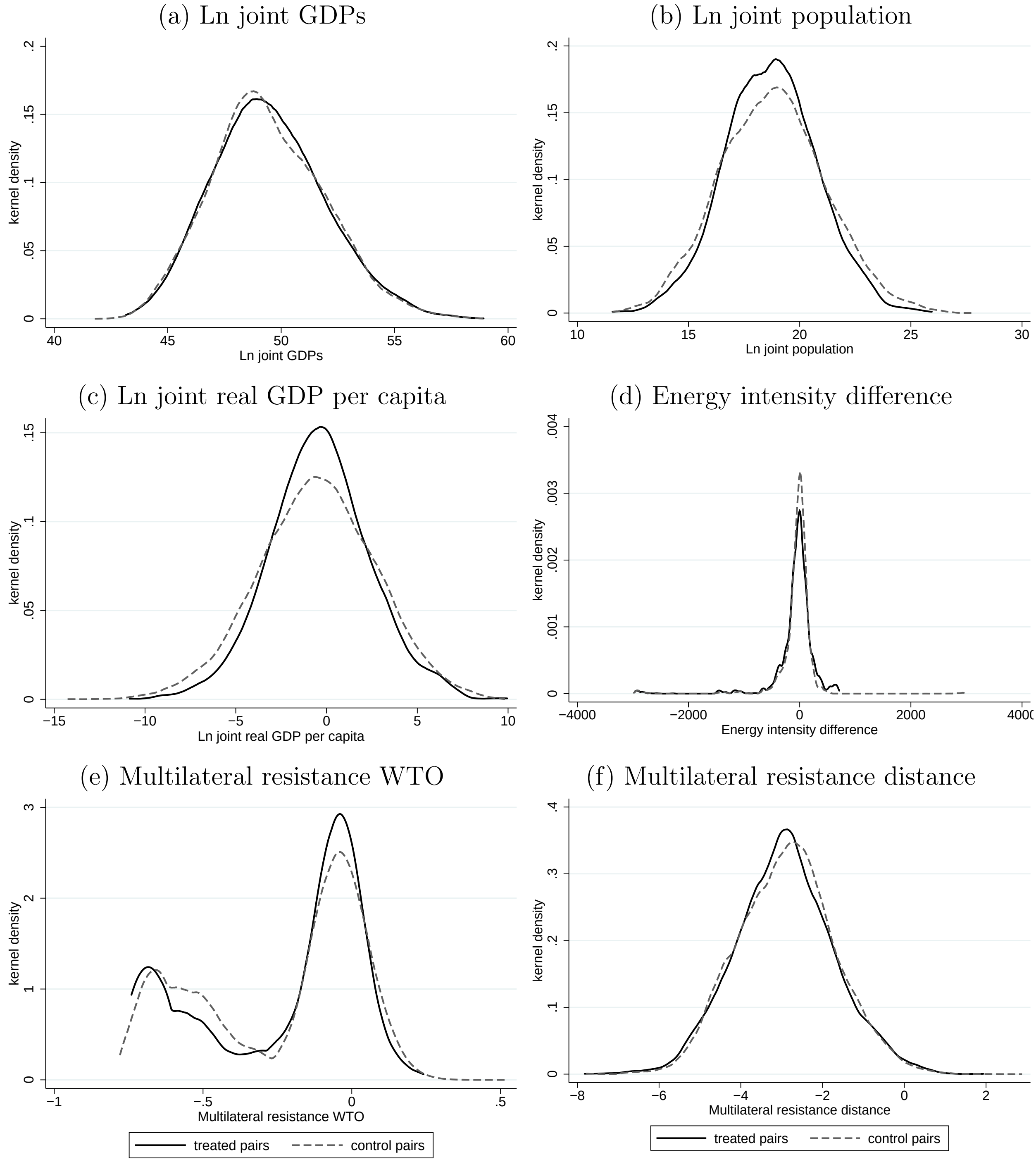
Before turning to our results, we will revisit the distribution of covariates. After the matching procedure, although not perfectly identical the kernel density distributions for treated country pairs (black solid line) and the control group (gray dashed line) are a lot more similar (see Figure 3.3). Balancing tests confirm this finding.[14] Table 3.2 shows matching variable means in the treated and control group. The table also provides the t-statistic of a balancing test of identical means in both groups. Keep in mind that before matching there exist significant differences between the groups in terms of all covariates except MR contiguity. Ideally, this source of bias vanishes completely with matching. After the matching procedure, the null of identical means in both groups is no longer rejected for most variables and the achieved bias reductions are large. Most striking is the energy intensity difference case. Before matching, the exporter in a treated pair on average needs around 58.35 kg of oil equivalents less per 1,000 PPP-adjusted US dollars of GDP than the importer while in an untreated pair the difference is -2.22 only. The null of identical means is strongly rejected (p-value of 0.00). After matching, the sample means are -59.40 for the treated country pairs and -56.44 for the control pairs. The balancing property is no longer rejected at conventional significance levels (p-value 0.72). The balancing property also holds for the WTO MR term, the log of joint GDP, the MR term of bilateral distance, and the log of joint population. The balancing test is a little less convincing for the log of joint real GDP per capita (p-value of 0.04) but the null of identical means is rejected at the 1% significance level. For the MR terms of FTAs, language and EU the balancing property is violated. But even in these cases, matching helps to reduce the bias in the observables distribution for the treated and control group. In light of the ignorability assumption this is reassuring. Though for a few matching variables balancing is rejected, matching brings us closer to a quasi-experimental data set.

3.3.2 Baseline Results

We apply the variants of the regression-adjusted DID matching estimator outlined in Section 3.2 (Mahalanobis matching and propensity score matching with nearest neighbors, kernel or radius) to estimate the ATT of Kyoto commitment on bilateral exports. The baseline results for the default sample including all country pairs are reported in Table 3.3. Column (1) shows estimates obtained by a differences-in-differences gravity estimation as benchmark. The gravity controls other than Kyoto commitment are log GDP of the importer and exporter, log real GDP per capita of the importer and exporter, dummies for FTA as well as joint WTO and EU membership, multilateral resistance terms for FTA, joint EU and

[14] The results described here stem from kernel propensity score matching in the default sample. A similar picture emerges for the other matching variants. In the reduced sample, the balancing properties are better still.

Figure 3.3: Kernel densities after matching (pre-treatment period)



Note: The graph shows Epanechnikov kernel density functions of the matching variables for treated country pairs (i.e. the exporter is a Kyoto country in the post-treatment period) and control country pairs (i.e. the exporter is no Kyoto country in the post-treatment period) for the pre-treatment period 1997-2000. Matches are based on Epanechnikov kernel propensity score matching with bandwidth 0.06.

joint WTO membership, the energy intensity difference, a period dummy and a constant. The adjusted R^2 is 0.293. So around one fourth of the within variation in the log of bilateral exports can be explained with our model. The coefficient on the log of the importer's GDP

Table 3.2: Balancing property

Variable	Mean		Bias	t-statistic
	Treated	Control	reduction	
Before matching:				
MR contiguity	-0.01	-0.00		**-0.94**
Energy intensity difference	-58.35	-2.22		-7.36
MR WTO	-0.24	-0.11		-27.42
Ln joint GDP	49.65	48.89		17.63
MR distance	-3.17	-1.29		-70.40
Ln joint population	18.63	19.03		-10.60
Ln joint real GDP per capita	-0.10	-1.44		24.87
MR FTA	-0.04	-0.02		-3.12
MR language	8.17	8.42		-16.58
MR EU	0.01	-0.01		10.12
After matching:				
MR contiguity	-0.01	-0.01	63%	**0.30**
Energy intensity difference	-59.40	-56.44	94.7%	**-0.36**
MR WTO	-0.24	-0.24	97.7%	**0.43**
Ln joint GDP	49.46	49.43	95.8%	**0.55**
MR distance	-2.93	-2.89	98%	**-1.39**
Ln joint population	18.72	18.79	80.8%	**-1.49**
Ln joint real GDP per capita	-0.38	-0.53	89.2%	**2.09**
MR FTA	-0.08	-0.10	-12.2%	2.83
MR language	8.29	8.38	67%	-4.22
MR EU	-0.03	-0.02	46.5%	-8.61

Note: The table shows means in the treated and control group for all matching variables before and after matching. The table also provides t-statistics of a two-sided t-test of mean comparisons between treated and untreated/control group. T-statistics printed in bold face indicate the test statistic does not reject the null of same means at the 1% significance level for the respective matching variable. Matches are based on Epanechnikov kernel propensity score matching with a bandwidth of 0.06. MR refers to multilateral resistance term computed as suggested by Baier and Bergstrand (2009a).

is 0.740 and statistically significant at the 1% level. This implies that a one percent increase in the importer's GDP increases bilateral exports by about 0.74%. The effect of an increase in exporter's GDP is not statistically different from zero. The coefficient on the log of the exporter's real GDP per capita is 0.605 and highly statistically significant. This suggests that more economically advanced exporters trade more. The effect of the importer's real GDP per capita on the other hand is insignificant. Joint WTO membership reduces exports by roughly 30%. Probably, this is because in our sample period new WTO members typically are less developed countries. FTA and joint EU membership increase bilateral exports by 17% and 30% respectively. Energy intensity differences are not significant. Finally, the average treatment effect of the exporter's Kyoto commitment is -0.082 and statistically significant at the 10% level. This implies that exports are reduced by about 8% due to the exporter's Kyoto commitment.

The next three columns show results on ATEs from regression-adjusted DID matching. Column (2) applies 5 nearest-neighbor propensity score matching, column (3) uses Epanechnikov kernel propensity score matching with a bandwidth of 0.06 and column (4) applies matching on the Mahalanobis metric with 5 nearest neighbors. Compared with column (1), the magnitude and significance of covariates vary only a bit. Most notably, the exporter's market size now matters for export volumes and the exporter's real GDP per capita turns insignificant. The models in columns (2)-(4) can explain roughly 40% of the within variation of bilateral exports. Note also that the number of observations reports country pairs used in the regression but does not take into account the weighting procedure. 5 nearest-neighbor propensity score matching (column 2) gives an estimate of Kyoto's ATE of -0.164, statistically significant at the 5% level. This suggests that bilateral exports are by 16.4% lower if the exporter has a Kyoto commitment. The estimate is larger than under the standard gravity benchmark. Kernel propensity score matching confirms this result. The estimated coefficient is -0.197, statistically significant at the 1% level. With Mahalanobis matching with 5 nearest neighbors, Kyoto's ATE is -0.152, significant at the 5% level. Summarizing, our results suggest an average country's exports are lowered by 15-20% due to the exporter's Kyoto commitment. And Kyoto's ATE is larger in the quasi-experimental data set obtained from matching.

Columns (5)-(7) show Kyoto's ATT from regression-adjusted DID matching using the same matching variants as in columns (2)-(4). The estimated effects lie in the range of -0.13 to -0.14. So, our results suggest a *Kyoto* country's exports are lowered by 13-14% due to the Kyoto commitment. Also, the ATTs are smaller in absolute terms than the respective ATEs.

Our findings can be summarized as follows. The estimates suggest that bilateral exports are reduced by 15-20% when the exporter has a Kyoto commitment. Our results also highlight that not accounting for self-selection on observables overstates the negative effect of Kyoto commitment: the ATEs are larger in absolute terms than the ATTs. Kyoto countries' competitiveness is less affected by Kyoto commitment than an average country's competitiveness. The ATT is around 13%. It follows, that comparing treated country pairs

Table 3.3: Treatment effects on export flows – Baseline results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	ATE: DID	ATE: reg.-adj. DID matching			ATT: regression-adjusted DID matching		
Method:	FE-OLS	PS 5 N-N	PS kernel	Maha 5 N-N	PS 5 N-N	PS kernel	Maha 5 N-N
Kyoto (0,1)	-0.082*	-0.164**	-0.197***	-0.152**	-0.142***	-0.139***	-0.133***
	(0.042)	(0.068)	(0.065)	(0.076)	(0.043)	(0.039)	(0.049)
Ln GDP exporter	0.131	0.334**	0.329**	0.522***	0.483***	0.458***	0.569***
	(0.090)	(0.168)	(0.152)	(0.139)	(0.095)	(0.082)	(0.111)
Ln GDP importer	0.740***	0.607***	0.644***	0.888***	0.830***	0.817***	0.842***
	(0.075)	(0.153)	(0.151)	(0.157)	(0.071)	(0.066)	(0.083)
Ln real GDP per capita exporter	0.605***	0.207	0.189	-0.100	0.347**	0.390***	0.016
	(0.145)	(0.350)	(0.319)	(0.281)	(0.157)	(0.140)	(0.186)
Ln real GDP per capita importer	0.084	0.375	0.446	0.294	0.124	0.111	0.147
	(0.120)	(0.294)	(0.297)	(0.235)	(0.116)	(0.107)	(0.130)
FTA (0,1)	0.173**	0.074	0.107	0.187	0.067	0.105	0.178*
	(0.086)	(0.149)	(0.119)	(0.142)	(0.093)	(0.087)	(0.105)
Joint WTO (0,1)	-0.329***	-0.114	0.118	-0.130	-0.127	-0.152	-0.380**
	(0.123)	(0.343)	(0.281)	(0.247)	(0.130)	(0.122)	(0.150)
Joint EU (0,1)	0.305***	0.212	0.134	-0.063	0.173*	0.190**	0.105
	(0.089)	(0.172)	(0.155)	(0.170)	(0.102)	(0.096)	(0.106)
Energy intensity difference	-0.000	-0.001**	-0.001*	-0.001	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Multilateral resistance terms[a]	YES	YES	YES	YES	YES	YES	YES
Period fixed-effects	YES	YES	YES	YES	YES	YES	YES
Observations	24,278	16,846	21,790	13,578	17,094	23,374	15,352
Adj. R^2	0.293	0.401	0.382	0.428	0.438	0.437	0.457

Note: Dependent variable is the log of bilateral exports. Country-pair fixed-effects estimation on pre- and post-treatment averages, i.e. 1997-2000 and 2004-2007, respectively. Period fixed-effects, MR terms and constant not shown. Heteroskedasticity-robust standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01. PS: Propensity score matching. Bandwidth of Epanechnikov kernel matching is 0.06. Maha: Matching on Mahalanobis metric. N-N: nearest-neighbor matching. [a] Construction of multilateral resistance terms for FTA, joint WTO and joint EU membership, see Baier and Bergstrand (2009a).

with a properly constructed control group alters results. In our context, it proofs important to use matching techniques to get closer to an experimental data set.

3.3.3 Robustness Checks

In this subsection, we report robustness checks pertaining to the selection model used, the assumption about when treatment occurs, the sample composition, the investigated time horizon, the choice of matching variables and a placebo treatment. Table 3.4 summarizes the obtained ATTs of Kyoto commitment from regression-adjusted DID matching. Column (1) replicates the benchmark ATTs from various propensity score and Mahalanobis metric matching variants to simplify comparison.

Logit selection model. Using a Logit instead of a Probit selection model for propensity score matching does not affect the obtained ATTs, see column (2). The estimated coefficients again suggest a 13-14% drop in bilateral exports due to the exporter's Kyoto commitment.

Treatment period. The choice of the treatment period from 2001-03 might influence our results. As a robustness check, we vary the definition of the time window of treatment. First, we include 2004 in the treatment period. Russia and the Ukraine ratified the Kyoto Protocol in 2004. So by including 2004 in the treatment window, all ratifications except the one of Belarus (in 2005) and Australia and Croatia (both in 2007, they are therefore assigned to the untreated group) fall into the treatment period. Pre- and post-treatment period are again chosen to be symmetric periods around the treatment window: 1998-2000 and 2005-2007. Column (3) shows that the ATTs from propensity score matching are a bit smaller but still significant using this alternative broader treatment period. The results for matching on the Mahalanobis metric are weaker. Overall, the results are robust to this alternative assumption on the treatment window.

Another question is whether treatment occurs with ratification in national parliament or with entry into force of the Protocol in 2005. We use entry into force as treatment date in a second robustness check. Then, the relevant pre- and post-treatment periods are 1997-2004 and 2005-2007, respectively. Interestingly, the ATTs are again statistically significant in most propensity score models but now lie in the range of -5 to -9%, see column (4). So the ATT from ratification is larger than the one from entry into force. Since both models have

Table 3.4: Sensitivity analysis on Kyoto's ATT

	(1) Baseline	(2) Logit	(3) Broad	(4) Entry force	(5) Reduced	(6) w/o China	(7) w/o EIT[a]	(8) Long	(9) Policy	(10) Placebo
ATT, propensity score matching										
One-to-one	-0.135***	-0.143***	-0.147***	-0.088**	-0.149	-0.204***	-0.151***	-0.208***	-0.166**	-0.054
	(0.050)	(0.052)	(0.054)	(0.041)	(0.097)	(0.061)	(0.055)	(0.054)	(0.070)	(0.063)
3 nearest-	-0.157***	-0.130***	-0.100**	-0.054	-0.123	-0.167***	-0.123***	-0.207***	-0.150**	-0.057
neighbor	(0.047)	(0.046)	(0.046)	(0.039)	(0.084)	(0.050)	(0.047)	(0.048)	(0.074)	(0.058)
5 nearest-	-0.142***	-0.144***	-0.096**	-0.066*	-0.142*	-0.153***	-0.122***	-0.217***	-0.162**	-0.041
neighbor	(0.043)	(0.045)	(0.045)	(0.036)	(0.076)	(0.045)	(0.044)	(0.045)	(0.072)	(0.054)
Kernel	-0.139***	-0.138***	-0.102**	-0.072**	-0.117*	-0.136***	-0.125***	-0.219***	-0.154**	-0.044
	(0.039)	(0.039)	(0.041)	(0.033)	(0.067)	(0.040)	(0.041)	(0.040)	(0.069)	(0.048)
Radius	-0.143***	-0.141***	-0.110***	-0.074**	-0.116*	-0.138***	-0.131***	-0.222***	-0.150**	-0.041
(radius=0.1)	(0.039)	(0.039)	(0.041)	(0.033)	(0.067)	(0.039)	(0.040)	(0.040)	(0.068)	(0.047)
ATT, Mahalanobis metric										
One-to-one	-0.120**		-0.090*	-0.004	-0.156	-0.119**	-0.149**	-0.174***	-0.105	0.001
	(0.053)		(0.054)	(0.057)	(0.097)	(0.056)	(0.062)	(0.059)	(0.075)	(0.059)
3 nearest-	-0.125**		-0.084	-0.022	-0.179*	-0.115**	-0.111*	-0.175***	-0.107	0.005
neighbor	(0.054)		(0.055)	(0.047)	(0.093)	(0.055)	(0.059)	(0.056)	(0.072)	(0.057)
5 nearest-	-0.133***		-0.092*	-0.038	-0.186**	-0.124**	-0.119**	-0.201***	-0.113*	0.022
neighbor	(0.049)		(0.051)	(0.042)	(0.084)	(0.050)	(0.055)	(0.053)	(0.066)	(0.056)

Note: Dependent variable is the log of bilateral exports. Treatment: exporter ratifies Kyoto commitment. Each cell represents the ATT from regression-adjusted DID matching estimation on pre- and post-treatment averages. The default is 1997-2000 and 2004-2007, respectively; Broad: 1998-2000 and 2005-2007; Entry force: 1997-2004 and 2005-2007; Long: 1995-2000 and 2004-2009. Placebo: 1993-1996 and 1997-2000. Covariates as in Table 3.3 (not shown). Logit uses a logit selection model. The reduced sample (8,573 country pairs) contains only exports to non-Kyoto importers. w/o China specification excludes China from sample. [a] Economies in transition (EIT) from Central and Eastern Europe and the Baltics (i.e. ALB, BGR, CZE, HUN, HRV, MKD, POL, ROU, SVK, SVK, and EST, LVA, LTU) excluded from sample. Policy also includes political variables. Placebo specification gives treatment effect of hypothetical treatment in 1997. Heteroskedasticity-robust standard errors in parentheses. Significance at 1%, 5% and 10% indicated by ***, ** and * respectively.

basically the same post-treatment period, it seems that only part of the observed negative competitiveness effects originates from entry into force. This points to anticipation effects after ratification.

Sample composition. So far, we have analyzed Kyoto countries' exports to all other countries irrespective of their Kyoto status. In a next step, we limit attention to exports into non-Kyoto countries. Column (5) reports results. Comparing the obtained ATTs in column (1) and (5), the ATTs approximately lie in the same range but the effects are less significant. Turning back to the default sample, the estimated effects might be due to special trends in China or economies in transition (EIT). Columns (6) and (7) show that results are not sensitive to excluding China or EITs from Central and Eastern Europe and the Baltic states from the sample.

Time horizon. In a further robustness check, we extend the time horizon to cover 2009 – the latest year with data on all variables. Pre- and post-treatment period are again chosen as symmetric windows around the default treatment period, i.e. 1995-2000 and 2004-09. Results are reported in column (8) of Table 3.4. We find highly significant ATTs of around 20% in most specifications. These effects are larger than in the baseline suggesting that either effects are larger when taking into account the Kyoto phase 2008-09 or Kyoto countries were hit more by the financial crisis.

Political variables. So far, we have omitted country's political conditions. Whether a country is e.g. politically stable or the government is left- or right-wing will influence its probability to self-select into Kyoto. The trade literature also discusses whether political conditions influence bilateral trade flows (see e.g. Mansfield et al., 2000). We check whether results are sensitive to including the durability index for political stability from the Polity IV Project and political variables from the World Bank Database on Political Institutions (DPI). The latter variables are FRAC: a country's fractionalization, SYSTEM: the political system, CHECKS: checks and balances which measures the number of veto players, YRSOFF: the years the government has been in office and GOV1RLC: an index of the government's political orientation, right-left-center. To bilateralize these variables, we take the maximum and minimum values in a country pair (see also Egger et al., 2008). The only exception being SYSTEM where we use the similarity in systems by taking differences and GOV1RLC where we create four dummy variables for whether one or both governments in a country pair are left or right wing. Column (9) shows the results. Results are robust to including political variables in the matching process, although the Mahalanobis matching seems less successful in finding a treatment effect. Given that the number of matching variables is increased quite a bit, this might be related to the curse of dimensionality.

Placebo treatment. In a last sensitivity check, we assign a placebo treatment to Kyoto exporter pairs in 1997. As in the baseline results, we look at the four year period before and after treatment. If we were to find a statistically significant Kyoto effect for the placebo experiment this would indicate we merely pick up structural changes in the trade flows between (mostly developed) Kyoto countries and (mostly developing) non-Kyoto countries that were already going on before Kyoto ratification. On the other hand, if we do not find a treatment effect, it supports our interpretation of a causal effect of Kyoto commitment on exports. All estimated ATTs in column (10) are statistically indistinguishable from zero. This indicates we are picking up a treatment effect and not a trend in our sample that had been there before Kyoto ratification.

3.4 Industry-level heterogeneity

Goods categories differ in terms of their average energy intensity, the degree of product differentiation and tradeability, and also in terms of the degree of regulation they are subjected to. This can lead to heterogeneity in trade reactions to Kyoto commitment. This is also reflected in the political debate which focuses especially on effects on energy-intensive sectors. So, studying aggregate bilateral exports might lead to aggregation bias. This leads us to a sector-by-sector analysis.

3.4.1 Results on Sectoral ATTs

We estimate the ATT for each of the 51 non-agricultural 2-digits SITC goods categories separately. The matching weight is also obtained separately. We choose regression-adjusted DID kernel propensity score matching as default. Table 3.5 presents our results. 17 categories display partly substantial negative effects in the range of 13-58%. Most of these sectors fall into the category chemicals, non-metallic mineral and metal products and machinery and

Table 3.5: Sectoral ATTs of Kyoto commitment

SITC	Sector label	ATT	SITC	Sector label	ATT
11	Beverages	0.077 (0.110)	61	Leather, leather manufactures, n.e.s., and dressed furskins	-0.279* (0.167)
12	Tobacco and tobacco manufactures	-0.562 (0.506)	62	Rubber manufactures, n.e.s.	-0.216** (0.096)
21	Hides, skins and furskins, raw	-0.055 (0.317)	63	Cork and wood manufactures	0.097 (0.105)
22	Oil-seeds and oleaginous fruits	-0.471 (0.442)	64[a]	Paper, paperboard and articles of paper pulp, of paper or of paperboard	-0.125 (0.094)
23	Crude rubber	-0.143 (0.182)	65	Textile yarn, fabrics, made-up articles, n.e.s., and related products	0.135* (0.075)
24	Cork and wood	0.217 (0.236)	66[a]	Non-metallic mineral manufactures, n.e.s.	-0.199** (0.079)
25	Pulp and waste paper	0.700*** (0.257)	67[a]	Iron and steel	-0.508*** (0.115)
26	Textile fibres and their wastes	-0.008 (0.123)	68[a]	Non-ferrous metals	-0.282** (0.113)
27	Crude fertilizers, and crude minerals	0.018 (0.136)	69[a]	Manufactures of metals, n.e.s.	-0.179** (0.071)
28	Metalliferous ores and metal scrap	-0.155 (0.184)	71	Power-generating machinery and equipment	-0.566*** (0.094)
29	Crude animal and vegetable materials, n.e.s.	0.225** (0.094)	72	Machinery specialized for particular industries	-0.263*** (0.075)
32	Coal, coke and briquettes	0.658 (0.547)	73	Metalworking machinery	-0.223** (0.103)
33[a]	Petroleum, petroleum products and related materials	0.023 (0.248)	74	General industrial machinery, n.e.s., machine parts, n.e.s.	-0.243*** (0.077)
41	Animal oils and fats	0.437* (0.257)	75	Office machines and automatic data-processing machines	0.162* (0.087)
42	Fixed vegetable fats and oils, crude, refined or fractionated	-0.243 (0.201)	76	Telecommunications and sound-recording and reproducing equip.	-0.577*** (0.091)
43	Animal or vegetable fats and oils, processed; waxes	-0.221 (0.189)	77	Electrical machinery, apparatus and appliances, n.e.s.	-0.283*** (0.073)
51[a]	Organic chemicals	-0.239** (0.104)	78	Road vehicles (including air-cushion vehicles)	-0.078 (0.094)
52[a]	Inorganic chemicals	-0.184* (0.110)	79	Other transport equipment	-0.303** (0.147)
53[a]	Dyeing, tanning and coloring materials	-0.021 (0.094)	81	Prefabricated buildings; sanitary, plumbing, heating, lighting fixtures	-0.149 (0.111)
54	Medicinal and pharmaceutical products materials, n.e.s.	-0.144 (0.087)	82	Furniture, and parts thereof	-0.009 (0.087)
55	Essential oils, resinoids, perfume materials; toilet, cleansing preparations	-0.089 (0.087)	83	Travel goods, handbags and similar containers	0.426*** (0.107)
56[a]	Fertilizers	0.101 (0.249)	84	Articles of apparel and clothing accessories	-0.070 (0.085)
57[a]	Plastics in primary forms	-0.189* (0.103)	85	Footwear	0.250** (0.117)
58[a]	Plastics in non-primary forms	-0.075 (0.107)	87	Professional, scientific and controlling instruments and apparatus, n.e.s.	-0.218*** (0.077)
59[a]	Chemical materials and products, n.e.s.	-0.065 (0.087)	88	Photographic apparatus, optical goods, n.e.s.; watches and clocks	0.175* (0.098)
			89	Miscellaneous manufactured articles, n.e.s	-0.072 (0.065)

Note: The table displays sector-specific ATTs from regression-adjusted DID kernel propensity score matching (Epanechnikov kernel, bandwidth 0.06) in default sample. Weights obtained sector-by-sector. Dependent variable is log of bilateral sectoral exports. Controls as in baseline (not shown). Heteroskedasticity-robust standard errors in parentheses. Significance at 1%, 5% and 10% indicated by ***, ** and * respectively. [a] Goods category considered to be energy-intensive.

equipment. For example, Kyoto commitment led to a reduction of iron and steel (category 67) exports of roughly 51%. With an ATT of -28% non-ferrous metals (category 68) are also substantially affected. And both non-metallic mineral manufactures (66) and manufactures

Table 3.6: Robustness checks sectoral ATTs

SITC	Sector label	(1) Baseline	(2) Logit	(3) w/o China	(4) Policy
51[a]	Organic chemicals	-0.239**	-0.237**	-0.230*	-0.415*
		(0.104)	(0.104)	(0.118)	(0.234)
52[a]	Inorganic chemicals	-0.184*	-0.178	-0.183	-0.231
		(0.110)	(0.110)	(0.118)	(0.203)
57[a]	Plastics (in primary form)	-0.189*	-0.194*	-0.103	-0.601***
		(0.103)	(0.103)	(0.114)	(0.200)
61	Leather, leather manufactures, n.e.s., and dressed furskins	-0.279*	-0.283*	-0.337*	-0.519**
		(0.167)	(0.168)	(0.178)	(0.259)
62	Rubber manufactures, n.e.s.	-0.216**	-0.216**	-0.229**	-0.629***
		(0.096)	(0.096)	(0.098)	(0.181)
66[a]	Non-metallic mineral manufactures	-0.199**	-0.195**	-0.183**	-0.136
		(0.079)	(0.080)	(0.087)	(0.164)
67[a]	Iron and steel	-0.508***	-0.505***	-0.397***	-0.813***
		(0.115)	(0.116)	(0.122)	(0.309)
68[a]	Non-ferrous metals	-0.282**	-0.282**	-0.212*	-0.483***
		(0.113)	(0.114)	(0.122)	(0.186)
69[a]	Manufactures of metals, n.e.s.	-0.179**	-0.178**	-0.192**	-0.426***
		(0.071)	(0.071)	(0.076)	(0.135)
71	Power-generating machinery and equipment	-0.566***	-0.564***	-0.501***	-0.449
		(0.094)	(0.094)	(0.104)	(0.307)
72	Machinery specialized for particular industries	-0.263***	-0.261***	-0.238***	-0.356***
		(0.075)	(0.074)	(0.079)	(0.136)
73	Metalworking machinery	-0.223**	-0.224**	-0.232**	-0.645***
		(0.103)	(0.104)	(0.112)	(0.182)
74	General industrial machinery, n.e.s., machine parts, n.e.s.	-0.243***	-0.240***	-0.228***	-0.261
		(0.077)	(0.077)	(0.081)	(0.163)
76	Telecommunications equipment	-0.577***	-0.576***	-0.579***	-0.623***
		(0.091)	(0.091)	(0.101)	(0.191)
77	Electrical machinery, apparatus and appliances, n.e.s.	-0.283***	-0.282***	-0.274***	-0.491***
		(0.073)	(0.073)	(0.079)	(0.125)
79	Other transport equipment	-0.303**	-0.296**	-0.416***	-0.012
		(0.147)	(0.147)	(0.155)	(0.377)
87	Professional, scientific and controlling instruments and	-0.218***	-0.219***	-0.206**	-0.029
	apparatus, n.e.s.	(0.077)	(0.078)	(0.089)	(0.132)
25	Pulp and waste paper	0.700***	0.743***	0.656**	1.673***
		(0.257)	(0.254)	(0.280)	(0.555)
29	Crude animal and vegetable materials, n.e.s.	0.225**	0.225**	0.232**	0.141
		(0.094)	(0.095)	(0.097)	(0.180)
41	Animal oils and fats	0.437*	0.439*	0.521*	0.313
		(0.257)	(0.255)	(0.297)	(0.325)
65	Textile yarn, fabrics, made-up articles, n.e.s.,	0.135*	0.138*	0.105	0.067
	and related products	(0.075)	(0.075)	(0.082)	(0.140)
75	Office machines and automatic data-processing machines	0.162*	0.165*	0.172*	0.301
		(0.087)	(0.087)	(0.098)	(0.183)
83	Travel goods, handbags	0.426***	0.420***	0.397***	0.324
		(0.107)	(0.107)	(0.120)	(0.236)
85	Footwear	0.250**	0.245**	0.288**	0.327
		(0.117)	(0.117)	(0.132)	(0.313)
89	Miscellaneous manufactured articles, n.e.s.	0.175*	0.178*	0.104	0.178
		(0.098)	(0.099)	(0.107)	(0.174)

Note: The table displays sector-specific ATTs from regression-adjusted DID kernel matching (Epanechnikov kernel, bandwidth 0.06) in default sample. Weights obtained sector-by-sector. Dependent variable is log of bilateral sectoral exports. Heteroskedasticity-robust standard errors in parentheses. Significance at 1%, 5% and 10% indicated by ***, ** and * respectively. Results only shown for sectors with significant effects in Table 3.5. Logit uses a Logit selection model. w/o China excludes China from sample. Policy includes political variables. [a] Goods category considered to be energy-intensive.

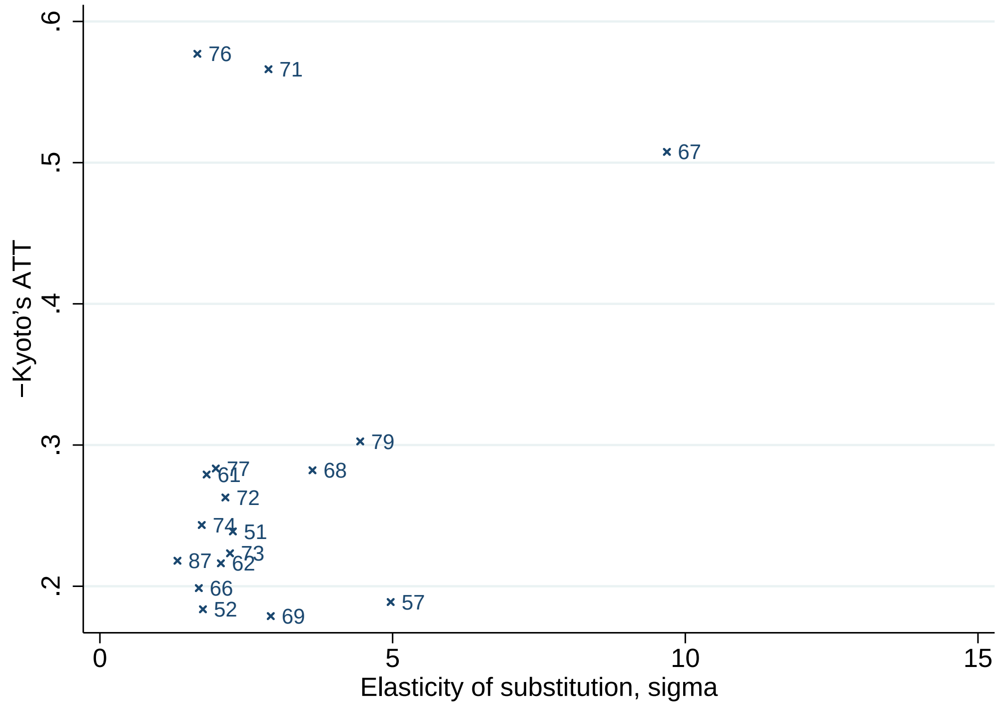
of metal exports (69) are reduced due to Kyoto commitment by little below 20%. In the chemicals category, the affected sectors are organic chemicals (category 51, ATT of -24%), inorganic chemicals (category 52, ATT of -18%) and plastics in primary forms (category 57, ATT of -19%). In the machinery and equipment category the negative ATTs lie in the range of 20-58%. The categories with the largest effects in absolute terms are power-generating machinery and equipment (category 71) and telecommunications equipment (category 76). Interestingly, we find a total of seven positive and significant estimates. Examples are pulp and waste paper (category 25), travel goods, handbags and similar containers (category 83) and footwear (category 85).

We perform several robustness checks on the sectoral ATTs. Table 3.6 reports the results for all sectors with significant effects in the baseline. Column (1) provides the baseline for easier comparison. Column (2) uses a Logit selection model. Column (3) drops China from the sample. And column (4) adds a host of policy variables to the matching variables. The robustness checks confirm the estimates on sectoral ATTs both in terms of magnitude and significance. In Appendix C, we also provide results on sectoral ATTs in the reduced sample (Tables C.1 and C.2). Here, we find less sectors with significant effects. Yet, the results are consistent for the categories chemicals, non-metallic mineral and metal products and partly also machinery and equipment.

3.4.2 Interpretation

Our estimates suggest large heterogeneity of the Kyoto Protocol's effects on sectoral trade. To understand these differences, one has to turn to a more structural interpretation of the underlying gravity equation. Aichele and Felbermayr (2011a) use a well-specified theoretical model to derive such an equation. A decomposition of the overall ATT is beyond the scope of the present paper, but the analysis in Aichele and Felbermayr (2011a) shows that the ATT confounds four elements: sectoral energy intensity, the elasticity of trade flows with respect to cross-country cost differences (essentially the elasticity of substitution in a CES demand system), the effect of Kyoto commitment on production costs, and industry-level transportation costs. Industries differ strongly along these dimensions. The absolute value of the size of the estimated ATT is increased by the cost effect of Kyoto (which is larger the more energy-intensive an industry is) or by the elasticity of substitution (which measures the strength at which exports react to cost differences). It is decreased by the importance of iceberg trade costs in the sector.

Figure 3.4: Sectoral ATTs and elasticity of substitution



Note: The graph shows a scatter plot of sectoral ATTs and average sectoral elasticity of substitution taken from Broda and Weinstein (2006). The graph only displays sectors with a negative and significant effect from regression-adjusted DID propensity score matching.

We draw the following broad conclusions. First, for some industries we find that the ATT is statistically identical to zero. Then, Kyoto cannot have had any effect on the cost structure. In industries where we find negative ATTs, i.e. negative export elasticities of Kyoto commitment, Kyoto affected sectoral costs. Second, among the goods categories with negative ATTs, many are indeed deemed to be energy-intensive. Examples are iron and steel, non-ferrous metals (like aluminium), non-metallic mineral manufactures (like cement or clay) and manufactures of metal, organic and inorganic chemicals and plastics in primary forms. Third, Figure 3.4 plots the sectoral ATTs against the average sectoral elasticity of substitution taken from Broda and Weinstein (2006). There seems to be a positive relationship. This finding suggests that sectoral trade flows react stronger to Kyoto commitment, the larger the elasticity of substitution. For example, iron and steel (67) is a rather homogeneous goods category with a high elasticity of substitution of about 10. And we also observe a high ATT of roughly 50%. Similarly, non-ferrous metals (68) has an elasticity of substitution of about 4 and an ATT of about 30%. However, there are two outliers – goods categories 71 (power-generating machinery) and 76 (telecommunications equipment). And the effects are also large in other machinery and equipment categories, which are differentiated goods according to the Rauch classification. However, these may be goods categories with relatively low *ad valorem* transportation costs.

Moreover, there is substantial heterogeneity in how Kyoto may have affected sectoral costs. Some sectors are exempt from regulation (this is for example the case under the EU Emissions Trading Scheme), some are more generously covered by subsidies. Many sectors might not be affected directly, but rather indirectly because they use energy-intensive intermediate products. Despite these complications, our results are broadly in line with theoretical arguments.

3.5 Conclusions

The international policy community is still on the search for a solution to the threat of global warming. Greenhouse gas emissions have detrimental effects on climate change irrespective of where they take place. If not all countries subject themselves to a world-wide climate deal, unilateral climate policy entails competitiveness effects leading to carbon leakage. This may undermine individual countries' efforts to curb emissions. Relocation effects may even result in an increase of the global level of emissions. The economics literature contributes to this debate by (1) discussing whether international environmental agreements are successful in achieving their goals and (2) by determining the effects of environmental regulation on trade patterns. This paper contributes to the second thread.

The present paper sheds light on the effects of Kyoto commitment on trade patterns. We use regression-adjusted DID matching to account for the possible endogeneity of commitment to the Kyoto Protocol. Our estimates suggest that an average country faces a reduction of exports of around 15-20% due to Kyoto commitment. The average treatment effect for a Kyoto country is smaller and in the range of 10-13%. So our results highlight that not accounting for self-selection overstates Kyoto's negative effect on exports. However, the effect is still large. Moreover, there is large sectoral heterogeneity of Kyoto's ATT. We identify sectors that are affected by competitiveness issues. These are typically energy-intensive industries like iron and steel, non-ferrous metals and chemicals but also machinery and equipment goods. So our message is: Kyoto has had an impact, at least on some sector's trade patterns.

This implies that unilateral climate policy like the Kyoto Protocol in and off itself might not be able to bring down GHG emissions. Some emissions might relocate to other countries. Thus, unilateral climate policy should be accompanied, for example, by carbon-related border tax adjustments. These adjustments should be designed such that they do not lead to green protectionism but that they help in restoring the effectiveness of unilateral climate policy. An industry-by-industry approach may be sensible, as our sector-level results suggest. Targeting the most energy-intensive and easily tradeable goods by BTA may suffice to restore the overall effectiveness of unilateral climate policy.

Chapter 4

Kyoto and Carbon Leakage: An Empirical Analysis of the Carbon Content of Bilateral Trade*

4.1 Introduction

Global warming caused by anthropogenic CO_2 emissions is a major public concern around the world. Because countries' greenhouse gas emissions have global effects, decentralized national regulation is inefficient. The Kyoto Protocol, an international agreement which sets binding emission targets for 37 industrialized countries and the European Union (EU), has met major criticism from its beginnings in 1997 onwards. Its *principle of common but differentiated responsibilities* excepts emerging and developing countries *en bloc* and sets widely different targets even for the 37 committed nations.[1] Countries like China or India face no binding constraints. The U.S. did not ratify the Protocol because it did not include the "meaningful" participation of all developing as well as industrialized countries, arguing that ratification would unfairly put the U.S. at a competitive disadvantage.

Related to this policy concern, economists have long pointed to the possibility of *carbon leakage*: regulation in some countries could change relative goods prices and hence shift

* This chapter is based on joint work with Gabriel Felbermayr. It is a revised version of CESifo Working Paper No. 3661, 2011.

[1] The commitments range from a *reduction* of emissions with respect to the base year 1990 of 21% by Germany and Denmark to an *increase* in emissions of 15% by Spain.

production of CO_2-intensive goods to places that are exempt from such regulation (see, e.g. Copeland and Taylor, 2005).[2] This sort of regulatory arbitrage is particularly important if trade costs are low and falling. Carbon leakage may offset domestic emission savings achieved through stricter climate policy. It can even lead to a global increase in emissions if non-committed countries rely on out-dated carbon-intensive technologies and energy sources.

The potential competitiveness loss and carbon leakage have sparked a debate in the U.S. and Europe about border tax adjustment (BTA) measures against countries that do not take actions to prevent climate change within the current multilateral agreement. Proposed American legislation[3] contains such a provision and former French president Nicolas Sarkozy has made similar proposals for the EU. Carbon-related BTAs always have the air of green protectionism and could be costly if non-committed countries resort to retaliation. So, it is important to assess the empirical relevance of trade-induced carbon leakage.

A vast computable general equilibrium (CGE) literature tries to assess *ex ante* the amount of carbon leakage resulting from unilateral climate policy initiatives such as the Kyoto Protocol. Carbon leakage is typically measured as the emission increase in non-Kyoto countries relative to the emission reduction in Kyoto countries. The results of the CGE simulations differ depending on parametrization and modeling assumptions. They range from moderate leakage rates of 5-40% (for example Felder and Rutherford, 1993; Bernstein et al., 1999; Burniaux and Oliveira Martins, 2012) to up to 130% (Babiker, 2005). A recent important contribution by Elliott et al. (2010) finds substantial carbon leakage ranging from 15 to 25% depending on the tax rate.

Ex post empirical evidence on carbon leakage, on the other hand, is scant. In essence, carbon leakage is a special case of the pollution haven effect, i.e. the trade effect of environmental regulation.[4] The general insight of the pollution haven literature is that environmental regulation indeed affects trade flows and the location choice of firms. To our knowledge, the only studies investigating carbon policy are World Bank (2008) and Aichele and Felbermayr (2013). World Bank (2008) employ a gravity framework to test for the effects of carbon taxes on bilateral trade in goods. They conclude there is no evidence for

[2] Note that stricter regulation can also depress the world price of energy and thus increase the energy demand in non-regulating countries (supply-side leakage). In our study we focus on the demand side channel of carbon leakage.

[3] E.g., the Clean Energy and Security Act (also called Waxman-Markey Bill).

[4] There is quite a body of literature looking on pollution haven effects. See, e.g. Ederington and Minier (2003); Ederington et al. (2005); Frankel and Rose (2005); Levinson and Taylor (2008); Grether et al. (2012); and the survey article by Brunnermeier and Levinson (2004).

carbon leakage. However, there are potential problems. First, estimates might be biased due to non-random selection of countries into the Protocol. E.g., a country's green preferences could be related to both its carbon policy and its trade flows. Second, different sectors might be differently prone to carbon leakage as they differ with respect to the carbon intensity of production. Analyzing bilateral trade flows might suffer from a sectoral aggregation bias. Aichele and Felbermayr (2013) address these concerns in a study on the Kyoto Protocol's competitiveness effects. Also in the gravity framework, they take self-selection into account with a matching econometrics approach. The authors find that Kyoto countries' exports are hampered by Kyoto commitment. On the sectoral level, they show that exports are particularly deterred in energy-intensive sectors but not necessarily in all product categories. However, both studies are not able to judge carbon leakage for the following reasons. A focus on *trade flows* (rather than on the carbon content) does not allow to infer on the reallocation of emissions across countries since emission intensities vary across sectors and countries. Last, investigating trade flows could underestimate carbon leakage because it disregards adjustments on the intermediate stages of production. When firms move production to another country, this might have very different effects on emissions relocation depending on whether (energy-intensive) intermediates are sourced domestically or imported. Levinson (2009) makes this point in a study of the U.S. emission savings due to trade.

Last, Aichele and Felbermayr (2012) document that the Kyoto Protocol has led to an increase in the spatial disconnect of countries' territorial CO_2 emissions and CO_2 footprints. The gap is explained by increasing (multilateral) net CO_2 imports into Kyoto countries. This suggests carbon leakage. The authors look at aggregate country data and use an instrumental variables approach to deal with Kyoto ratification endogeneity. The instrumental variable is membership to the International Criminal Court in Den Haag. In contrast to their country-based approach, we explicitly investigate the bilateral trade dimension of the trade phenomenon carbon leakage.

In this study, we propose a novel way to test for carbon leakage. We suggest to investigate the *carbon content of trade* to understand whether and by what extent commitments made under the Kyoto Protocol affect carbon leakage. The carbon content of trade measures all upstream CO_2 emissions associated with a trade flow along the production chain. Hence, climate policy induced changes in the carbon content of trade reflect the full (direct and upstream) extent of carbon leakage due to relocation of production. Grether et al. (2012)

use a similar logic to study the pollution haven effect. They show that differences in environmental stringency lead to more embodied pollution imports.

Computation of the carbon embodied in trade requires knowledge of the input-output and sectoral emission structure of all investigated countries. Several studies estimate the carbon content of trade for a cross section of countries (see e.g. Ahmad and Wyckoff, 2003; Peters and Hertwich, 2008; Nakano et al., 2009). Only very recently, Peters et al. (2011) have provided an estimation for 113 regions for the years 1990-2008 based on the Global Trade Analysis Project (GTAP) 7. GTAP 7 only provides emission coefficients and input-output data for its base years (1997, 2000, and 2004). The estimates for the years in between are interpolations. While Grether et al. (2012) recently provided first empirical work on SO_2, amongst other pollutants, none of the CO_2 studies works with bilateral data and employs econometric techniques. Therefore, we construct our own database with the bilateral carbon content of trade for 40 countries, 12 industries, and the years 1995-2007. Our database mainly builds on OECD and UN data and, contrary to the study by Peters et al. (2011), has yearly data on emission coefficients.

We motivate our empirical approach by a gravity model for CO_2. The carbon content of trade is determined by a gravity-type equation that features climate policy. We show that in a two-country case (e.g. a Kyoto and non-Kyoto block) a unilateral carbon price increase in a country leads to increased imports of carbon from the country without such regulation. Put differently, the trade partner increases its emissions in reaction to the country's tighter climate policy. In the empirical part, we study the effects of a quasi-experiment – the Kyoto Protocol. We conduct an econometric ex-post evaluation of the leakage effects triggered by the Kyoto Protocol. The maintained assumption in our analysis is that committed countries have indeed stricter climate policies. Note that this assumption is hard to put to a rigorous empirical test because of the plethora of different policies adopted by countries: emission reductions could be achieved with carbon taxes, an emission cap-and-trade system, regulatory requirements, feed-in tariffs for alternative energies, subsidies to research in carbon-free technologies etc. Yet, we provide some illustrative evidence that the number of policy initiatives has gone up after Kyoto ratification. And, anecdotal evidence hints at policy action after ratification.[5] Estimating our carbon content gravity model raises several econometric issues. Most importantly, selection into the Kyoto Protocol is most likely not

[5] See Aichele and Felbermayr (2011b) and data displayed on www.lowcarboneconomy.com/Low_Carbon_World/Data/View/12.

random. Here the structure of our data helps. While the observational units in our analysis are country-pairs (dyads), selection into a multilateral agreement such as Kyoto is done by single countries (monads) based on that country's position relative to all trading partners.[6] So, the extensive use of country $\times$ year dummies effectively accounts for *all* reasons why a country may commit at a certain point in time to pollution targets under the Kyoto Protocol.

This gravity approach bears several advantages compared to the country-based approach in Aichele and Felbermayr (2012). First, we do not need an instrumental variables strategy because of the nature of our data structure. Second, with our sectoral approach we can identify sectors that are specifically prone to leakage. A further feature of our approach is that we can disentangle scale from technique effects. This allows to judge whether carbon leakage is a volume effect and to what extent it is an effect of importing more CO_2-intensive goods.

Our within estimations imply that sectoral carbon imports of a committed country from an uncommitted exporter are about 8% higher than if the country had no commitments. The carbon intensity of those imports is about 3% higher. The empirical evidence also hints at technological clean-up in Kyoto countries. Sector by sector, we find robust evidence for carbon leakage for at least five out of twelve sectors. The affected sectors include such likely candidates as basic metals, other non-metallic mineral products or paper and pulp. Wood and wood products or textiles, on the other hand, seem unaffected by leakage. The findings are robust, in particular to using a model in long first-differences. They highlight the importance of subjecting all countries of the world to binding emission targets. The results also imply that countries' domestic emissions are poor measures of their overall impact on climate change.

The paper is organized as follows: Section 4.2 lays out the gravity model for CO_2. Section 4.3 describes our data. Section 4.4 discusses our empirical strategy and Section 4.5 presents the results and robustness checks. Section 4.6 concludes.

4.2 Gravity for CO_2

This section develops a simple partial equilibrium model of indirect bilateral trade in CO_2 emissions. The objective is to propose a simple theoretical framework which (i) delivers

[6] See Copeland and Taylor (2005) for a theoretical argument on the optimal choice of carbon policies.

a gravity equation for the carbon content of bilateral trade, and (ii) provides guidance in the theory-consistent accounting for embodied CO_2 emissions. To meet these aims, the model must allow for domestic and imported goods to be used as intermediate inputs and for technology differences across sectors and countries. The starting point is a Krugman (1980)-type gravity model with monopolistic competition and increasing returns to scale. The gravity equation can also be rationalized with an Armington trade model, i.e. goods are assumed to be homogeneous and differentiated according to their country of origin. Both models lead to an isomorphic gravity equation (see Arkolakis et al., 2012, for further details). Although the Armington assumption fits trade in energy-intensive homogeneous goods such as energy products and base metals well, we prefer the more general Krugman (1980) version because (i) we are not solely interested in trade in energy-intensive products, (ii) the huge bulk of trade is in differentiated manufacturing goods and (iii) these goods (while often not directly energy-intensive) strongly use energy-intensive intermediates like steel. This is captured in a good's carbon content of trade. The model can also be used to carry out simple comparative statics and a decomposition of carbon policy effects into scale, technique and composition effects.[7]

4.2.1 Consumers

There are K countries, indexed $i, j, k = 1, ..., K$, which are structurally similar, but may differ with respect to climate policy or country size. Each country consumes a manufacturing good M_i and a homogeneous good H_i, where ω denotes the expenditure share of manufacturing. The manufacturing good is a Cobb-Douglas composite of goods M_i^s from S sectors, indexed $s = 1, \ldots, S$. μ^s denotes the expenditure share of sector s in manufacturing, with $\sum \mu^s = 1$. The differentiated varieties within each sector s are home-made as well as imported. N_j^s denotes the number of symmetric varieties produced in country j. Agents have CES preferences over quantities of varieties q_{ij}^s. Overall utility is given by:

$$U_i = (H_i)^{1-\omega}(M_i)^{\omega}, \quad \text{with } M_i = \prod_{s=1}^{S} (M_i^s)^{\mu^s},\ M_i^s = \left(\sum_{j=1}^{K} N_j^s (q_{ij}^s)^{\frac{\sigma^s-1}{\sigma^s}}\right)^{\frac{\sigma^s}{\sigma^s-1}}, \tag{4.1}$$

[7] The distinction of scale, technique and composition effects of changes in trade flows goes back to seminal work by Grossman and Krueger (1993) on the environmental effects of NAFTA.

where $\sigma^s > 1$ is the sectoral elasticity of substitution.[8]

The price index dual to M_i is denoted by $\Pi_i = \prod_{s=1}^{S} (P_i^s)^{\mu^s}$, where the sectoral price index is $P_i^s = \left(\sum_{j=1}^{K} N_j^s (p_{ij}^s)^{1-\sigma^s} \right)^{1/(1-\sigma^s)}$. Prices of sector-$s$ varieties delivered from country j to i have the c.i.f. price $p_{ij}^s = \tau_{ij}^s p_j^s$, where $\tau_{ij}^s \geq 1$ is the usual iceberg trade cost factor and p_j^s is the mill (ex-factory) price of a differentiated variety in country j.

4.2.2 Firms

Output of the homogeneous good sector is freely tradable and acts as numeraire. It is produced under perfect competition from labor with a constant marginal productivity of one. The homogeneous good can be consumed. It can also be used as an input, namely fuel. Using the homogeneous good as the numeraire and assuming that it is produced in every country, wage rates are equalized to unity so that $w_i = 1$.[9]

The S differentiated goods sectors feature monopolistic competition and increasing returns to scale. Each sector is populated by an endogenously determined mass of symmetric firms, which each produces a distinct variety. The minimum unit cost function of a firm is $c_i^s [\Pi_i, b_i, w_i]$, where the function $c_i^s [\cdot]$ has the usual properties.[10] It reports the cost-minimizing combination of three inputs – the manufacturing good, fossil fuel, and labor, whose prices are given by $\Pi_i, b_i,$ and w_i respectively. Carbon prices $b_i = t_i$ differ across countries because of (ad valorem) carbon taxes or emission certificate prices $t_i \geq 1$, which are wasteful.[11] Alternatively, one could think of t_i as regulation to control CO_2 intensity that uses up resources. Total costs of a generic firm consist of a variable and fixed part and are given by

$$C_i^s = c_i^s [\Pi_i, t_i, 1] \, y_i^s + f^s, \tag{4.2}$$

[8] We could add a separable emission externality due to the emissions arising from manufacturing. Since we focus on the positive and not the normative aspects of the relationship between exogenous climate policies and firm location, the negative externality would play no role in the subsequent analysis and is left our for simplicity.

[9] The use of a numeraire sector has a long tradition, see Behrens et al. (2009) for a recent related example and some discussion. Since the present paper is interested in the empirical relationship between trade in goods and climate policies, it appears natural to take the prices of fuel, and (essentially) labor as exogenous. The econometric strategy will be able to accommodate differences in fuel prices over time and countries.

[10] It is homogeneous of degree 1, as well as increasing and strictly convex in all arguments.

[11] We assume that tax income is not rebated, e.g. in a lump-sum fashion, so that income of the representative consumer is exogenous and depends on country size only. This simplifies comparative statics results. The assumption is not unusual in a multi-country setting with taxes, see e.g. Ossa (2011a).

where y_i^s is the output level of the firm and f^s denotes its fixed labor requirement.

Profits of a generic firm in country i are given by $(p_i^s - c_i^s[\cdot])y_i^s - f^s$. Profit maximization yields the optimal price $p_i^s = c_i^s\sigma^s/(\sigma^s - 1)$. Substituting this into profits and recognizing that free entry forces profits to zero, the size of the firm in equilibrium is given by $\bar{y}_i^s = (\sigma^s - 1)f^s/c_i^s$.

4.2.3 International trade flows

In what follows, importer countries will be denoted by the index m, and exporters by x. To avoid notational clutter, the sectoral index is suppressed whenever possible. Maximizing (4.1) subject to the appropriate budget constraint, country m consumer demand for varieties produced in country x is

$$d_{mx} = N_x \frac{\mu\omega L_m}{P_m}\left(\frac{p_{mx}}{P_m}\right)^{-\sigma}, \tag{4.3}$$

where L_m is country m's income which is equal to its labor force,[12] $\mu\omega L_m/P_m$ denotes real expenditure allocated to the sector and p_{mx}/P_m is the price of varieties from country x relative to the average of all consumed varieties in m.

Differentiated goods are also required as inputs for production. Since firms demand the same composite manufacturing good as consumers, they have the same demand structure. With the additional assumption that $c_i[\Pi_i, t_i, 1] = \Pi_i^\alpha t_i^\beta$ is Cobb-Douglas, where $\alpha, \beta \in (0, 1)$ are the cost share of intermediates and fuel respectively, the following theoretical gravity equation for goods can be stated:

Result 1. *The quantity of country m's total sectoral imports Q_{mx} from country x is given by*

$$Q_{mx} = Z \cdot (1 + g_m) \cdot L_m \cdot (P_m)^{\sigma-1} \cdot N_x \cdot (c_x[\cdot])^{-\sigma} \cdot (\tau_{mx})^{-\sigma}, \tag{4.4}$$

*where $Z \equiv \mu\omega(\frac{\sigma-1}{\sigma})^\sigma$ is a constant, $g_m > 0$ is a multiplier for intermediate trade, $L_m(P_m)^{\sigma-1}$ describes country m's **market capacity** in a sector, and $N_x(c_x[\cdot])^{-\sigma}$ is country x's **supply capacity**.*

Proof. See Appendix D.

Note that (4.4) differs from the usual gravity equation (discussed, e.g., in Redding and Venables (2004, p. 57 f.), from where we borrow the terms market and supply capacity)

[12] This includes income generated in the extraction of fossil fuel.

in two ways: first, Q_{mx} is not the value but the quantity of bilateral trade (so that the exponent on trade costs is $-\sigma$ and not $1-\sigma$); second, the trade multiplier g_m accounts for trade in intermediate goods. g_m increases in the cost share of the final good. Moreover, g_m also reflects comparative advantage in dirty versus clean goods production. When a country has a comparative advantage in the homogeneous clean good (e.g. due to a stricter climate policy) the country produces and exports relatively more of the homogeneous good and differentiated goods trade is dampened. Climate policies will affect Q_{mx} through their effects on market and supply capacities of the trading partners.

The model is closed with S goods market clearing conditions in each country i :

$$\bar{y}_i = \frac{(\sigma-1)f}{c_i} = \sum_{m=1}^{K}(1+g_m)\frac{\mu\omega L_m}{P_m}\left(\frac{p_{mi}}{P_m}\right)^{-\sigma}\tau_{mi}, \qquad i=1,\ldots,K. \tag{4.5}$$

Condition (4.5) states that the supply of a variety has to equal its demand inclusive of trade costs from all importing countries.

4.2.4 The carbon content of bilateral trade

In the present paper, the objective is to empirically analyze whether carbon policies have affected the location of emissions through international trade of goods. Sectors are linked via input-output (I-O) linkages. Empirically, emissions by upstream sectors often dwarf direct ones and relevant upstream sectors produce carbon-intensive inputs that are scarcely traded internationally (e.g., electricity). Hence, we need to understand the carbon content of trade, i.e., the quantity of CO_2 that is embodied in a country's trade flows.

From Shepard's lemma, a sector's direct CO_2 unit requirement is given by $e_i^s = \beta^s c_i^s/t_i$. To compute the total embodied CO_2 it is useful to distinguish between two accounting methods. The first accounts only for upstream emissions of domestic suppliers; we refer to this concept as *single-region I-O* (SRIO) method. The second method additionally accounts for foreign upstream emissions caused by imports of intermediates; in line with the literature, we refer to this as *multi-region I-O* (MRIO) method. The SRIO approach uses the exporter's I-O table $\mathbf{B}_x$ (with dimensionality $S\times S$) and computes the matrix of input requirements according to the Leontief inverse $\mathbf{A}_x = (\mathbf{I}-\mathbf{B}_x)^{-1}$. As shown by Trefler and Zhu (2010) in the context of a more standard factor content of trade study, the MRIO approach differs from the SRIO approach simply by using a multi-regional input-output table $\mathbf{B}$, i.e., a $KS\times KS$ matrix whose elements are bilateral I-O matrices, denoted $\mathbf{B}_{ji}$, which record country $j's$ usage of intermediate goods supplied from country i. Computationally, the SRIO and MRIO methods differ only with respect to the dimensionality of the inputs requirement matrix.

Result 2. *The CO_2 content of imports of m from x is given by*

$$E^s_{mx} = \eta^s_x Q^s_{mx}, \tag{4.6}$$

where

$$\eta^s_x \equiv \begin{cases} \mathbf{e}_x \mathbf{A}^s_x & \text{for SRIO} \\ \mathbf{e}\tilde{\mathbf{A}}^s_x & \text{for MRIO} \end{cases}.$$

The row vector $\mathbf{e}_x$ collects only the exporters' sectoral emission coefficients e^s_x while $\mathbf{e}^s$ is the collection of all those vectors world-wide. The column vector $\mathbf{A}^s_x$ reports the exporter's sectoral input coefficients (column s in the domestic Leontief-inverse), while $\tilde{\mathbf{A}}^s_x$ is the vector of world-wide input requirements of sector s in country x (column $S(x-1)+s$ in the multi-region Leontief inverse).

Proof. See Appendix D.

Substituting equation (4.4) into (4.6), one obtains a *gravity equation for CO_2*. Climate policy affects the carbon content of trade through emission intensities η_x, η_m, market and supply capacities and the intermediates multiplier.

The MRIO method is required when one is interested in a country's total carbon footprint. However, for the purpose of the present empirical analysis, the SRIO approach is preferable. The reason is that changes in the SRIO CO_2 content of trade mirror changes in the *trade partner's* CO_2 emissions only. So, the SRIO model allows inference on the amount of emissions (direct and upstream) relocated to a trade partner when country i strengthens its climate policy — i.e. carbon leakage. The major drawback of the MRIO method is that effects in the trade partner cannot easily be disentangled from effects in third countries. The SRIO method, on the other hand, ignores the fact that country i's stricter climate policy may affect from where the trade partner and other countries purchase their inputs – which in turn affects the location of emissions, too. For these reasons, we report results based on both the MRIO and the SRIO approaches.

An alternative concept building on the carbon content of trade is the *CO_2 terms of trade* (CTT).[13] It relates the CO_2 content of one dollar of exports to the CO_2 content of one dollar of imports. Antweiler (1996) proposed the measure as a country-specific index (i.e. summing over all trade partners, respectively) for aggregate trade. However, in our context it makes sense to break the CTT down on the bilateral sectoral level. For the SRIO method, for example, country i's CTT with country j in a sector s are:

$$CTT_{ij}^s = \frac{E_{ji}^s/Q_{ji}^s}{E_{ij}^s/Q_{ij}^s} = \frac{\eta_i^s}{\eta_j^s} = \frac{t_j}{t_i}\frac{\mathbf{c}_i\mathbf{b}\mathbf{A}_i^s}{\mathbf{c}_j\mathbf{b}\mathbf{A}_j^s}, \tag{4.7}$$

where $\mathbf{c}_i$ collects sectoral unit costs into a row vector and $\mathbf{b}$ is a diagonal matrix collecting the sectoral energy cost shares. A country's bilateral CTT are inversely related to its relative carbon price with the trade partner. Carbon prices also affect unit costs and input choices. Likewise, the aggregate CTT of i with j is

$$CTT_{ij} = \frac{\sum_s E_{ji}^s/\sum_s Q_{ji}^s}{\sum_s E_{ij}^s/\sum_s Q_{ij}^s} = \frac{t_j}{t_i}\frac{\sum_s \mathbf{c}_i\mathbf{b}\mathbf{A}_i^s Q_{ji}^s/\sum_s Q_{ji}^s}{\sum_s \mathbf{c}_j\mathbf{b}\mathbf{A}_j^s Q_{ij}^s/\sum_s Q_{ij}^s}, \tag{4.8}$$

which is again inversely related to relative carbon prices. If country i's CTT is larger than one, it signifies its (sectoral) exports are more carbon-intensive than its (sectoral) imports. Regardless of trade imbalances, the country loses from trade in terms of CO_2 emissions. Conversely, if country i's CTT is smaller than one it gains in terms of CO_2 emissions. The products it imports are more CO_2-intensive than the ones it exports; which implies lower domestic emissions.

4.2.5 Climate policy and the CO_2 content of bilateral trade

Before we turn towards the empirical analysis of gravity equation (4.6), we characterize the comparative statics of carbon policies in a simple special case of the model. As is customary in the theoretical literature (Antweiler et al., 2001), we can decompose the sectoral effect of environmental policy in the presence of international trade into two terms: a *technique effect* that relates to the substitution away from energy towards other factors of production, and a *scale effect* which is driven by the change in the cost of production relative to other countries and therefore to the volume of sectoral imports.[14] In principle, the importer's technique and

[13] See Antweiler (1996) for a more general formulation of pollution terms of trade with several pollutants.

[14] On the aggregate bilateral trade level, there is a composition effect in addition which is driven by the change in the mix of traded goods.

scale effect are both affected by own carbon prices as well as by the ones of foreign countries. Neglecting third country effects, and using $\hat{x} = dx/x$, linearizing $E_{mx} = \eta_x Q_{mx}$ yields

$$\hat{E}_{mx} = \underbrace{\kappa_{\eta,m}\hat{t}_m + \kappa_{\eta,x}\hat{t}_x}_{\text{technique effects}} + \underbrace{\kappa_{Q,m}\hat{t}_m + \kappa_{Q,x}\hat{t}_x}_{\text{scale effects}}, \tag{4.9}$$

where $\kappa_{\xi,j}$ denotes the elasticity of a variable ξ with respect to j's carbon price, with $\xi \in \{\eta, Q\}, j \in \{m, x\}$.

In a model featuring I-O linkages, there is no closed form solution for the elasticities in equation (4.9) because one would have to solve for the number of varieties N_i in (4.5). However, the special case of *no intermediates trade* (i.e. the cost share α of the intermediate is zero) lends itself to analytical expressions for the scale and technique effects. A firm's unit cost function then only depends on its own country's climate policy and is given by $c_x[\cdot] = t_x^{\beta}$. Applying Shephard's Lemma, the emission intensity is $\eta_x = e_x = \beta(t_x)^{\beta-1}$. Solving for N_x[15] and using N_x and c_x in equation (4.4), the bilateral trade volume is

$$Q_{mx} = (\frac{\sigma-1}{\sigma})^{\sigma+1}\mu\omega L_m(\tau_{mx})^{-\sigma}(t_x)^{-\beta}(\sum_j \phi_{mj}\frac{F_j}{F_x})^{-1}, \tag{4.10}$$

where $\phi_{ij} \equiv (\tau_{ij})^{1-\sigma}$ and F_j denotes a trade-cost weighted measure of j's market potential. It is given by $F_j \equiv \sum_k \frac{\varphi_{jk}L_k}{\varphi_k}$. φ_{jk} is an entry of the inverse trade cost matrix and $\varphi_k \equiv \sum_{i=1}^{K}\varphi_{ki}(t_i)^{\beta(\sigma-1)}$ is a cost-weighted measure of country k's inverse centrality (proximity to trade partners). Note that the trade volume directly depends on the exporter's carbon price t_x with an exponent of $-\beta$. Climate policy of all countries enters through the last term. It is a measure of the exporter's (size-, distance- and cost-weighted) centrality relative to all countries.

[15] Details are provided in Appendix D.

Result 3. *The technique and scale elasticities of carbon prices in the importer and the exporter countries are given by*

$$\begin{aligned}
\kappa_{\eta,m} &= 0; \\
\kappa_{Q,m} &= \frac{\beta(\sigma-1)F_m - \sum_j \phi_{mj} F_j \kappa_{\lambda,m}}{\sum_j \phi_{mj} F_j} \\
\kappa_{\eta,x} &= -(1-\beta) < 0; \\
\kappa_{Q,x} &= -\sigma\beta - \frac{\sum_j \phi_{mj} F_j \kappa_{\lambda,x}}{\sum_j \phi_{mj} F_j},
\end{aligned}$$

where $\lambda \equiv \lambda_j/\lambda_x$ is the ratio of some country j's share in varieties relative to the exporter's share in world varieties, and $\kappa_{\lambda,i}$ is the elasticity of λ with respect to i's carbon price, $i \in \{m, x\}$.

Proof. See Appendix D.

If the importer country m imposes a carbon price while the exporter country x remains inactive (i.e., $i = m$; $\hat{t}_m > 0$, $\hat{t}_x = 0$), there is no technique effect, since in our special case country m's climate policy does not have any price effect in country x, so $\kappa_{\eta,m} = 0$.[16] The sign of the scale effect $\kappa_{Q,m}$ depends on the effect of increased costs in m, and on x's relative proximity, which in turn depends on all bilateral trade costs. While generally ambiguous, in the two-country case country x increases the share of varieties it produces (i.e. its competitiveness in manufacturing) so that imports Q_{mx} increase and, hence, $\kappa_{Q,m} > 0$.[17]

If the exporter country x uses climate policy and raises its carbon price while the importer m remains inactive (i.e., $i = x$, $\hat{t}_x > 0$, $\hat{t}_m = 0$), country m's carbon imports are decreased by the technique effect, since country x lowers its carbon intensity with an elasticity of $\kappa_{\eta,x} = -(1-\beta) < 0$. The sign of the scale effect $\kappa_{Q,x}$ is, again, ambiguous. The direct effect of $\hat{t}_x > 0$ is to lower the trade volume with an elasticity of $-\sigma\beta$. In a two-country world, $\kappa_{Q,x} < 0$ as country x loses competitiveness so that the volume of its sales falls unambiguously.

Summarizing, we expect the carbon content of imports to rise if the importer is committed under the Kyoto Protocol and to fall if the exporter is committed. The higher the elasticity of substitution σ and the higher the carbon intensity of a sector β, the stronger is the reaction

[16] This would be different if the world-wide price of carbon fuels were endogenous. Note, however, that the reduced world fuel price would only strengthen the effects. So we can disregard it without loss of generality.

[17] Detailed expressions for the elasticities are provided in Appendix D

of the carbon content of trade to climate policy. However, those predictions are derived under strong assumptions. It will be left to the empirical exercise to determine the sign and magnitude of scale, technique and the overall effect of the Kyoto Protocol.

4.3 Data

4.3.1 Data sources

In this section, we use Result 2 to construct a novel dataset of the CO_2 content of bilateral trade flows for the period 1995 to 2007. Three types of data are required: input-output tables, sectoral CO_2 emission coefficients, and bilateral trade data.

Input-output tables. I-O tables allow accounting for upstream emissions of CO_2. The OECD provides harmonized I-O tables for a total of 40 countries (its members plus other countries including Brazil, Russia, India and China). A key feature and a novelty of that data is the presence of a time dimension for the major share of countries. I-O tables are observed around the years 1995, 2000, and 2005; for 37 out of 40 countries, we have at least two tables per country.[18] The OECD input-output tables contain 48 industries (2 digit ISIC). Unfortunately, we have to aggregate the I-O data to 15 industries to match the available emissions data.[19] When taking other countries' upstream CO_2 emissions into account (in the MRIO model), we would need to know from which country each sector sources its imported inputs. In other words, we need *bilateral* I-O tables. Such data are not available. However, we observe θ^s_{ij}, the share of imports from country j in country $i's$ total absorption of sector s inputs. Following Trefler and Zhu (2010), we assume that this share applies equally in all sectors in country i that make use of input s. For example, if the U.S. imports 20% of its steel absorption from China and a sector uses steel as intermediate input, then we assume 20% of this steel was sourced in China. Consequently, the bilateral input-output table of country i with country j is

$$\boldsymbol{B_{ji}} = \boldsymbol{\theta_{ji}} \cdot \boldsymbol{\bar{B}_j}, \tag{4.11}$$

[18] We used the I-O tables from 1995 for the years 1995-97, those from 2000 for 1998-2002, and those from 2005 for 2003-2007.

[19] Table B.1 in Appendix B shows the sectoral breakdown. There is an obvious trade-off between country coverage and the level of sectoral disaggregation. Our focus on international trade links forces us to include as many countries as possible, possibly at the risk of some aggregation bias.

where $\mathbf{\bar{B}_j}$ is the reported I-O table of country j and $\boldsymbol{\theta_{ij}}$ is a column vector containing the shares θ_{ij}^s.

Trade data. We obtain bilateral trade data (f.o.b.) from the UN Commodity Trade (COMTRADE) database.[20] We use a concordance table provided by Eurostat to translate the data from the SITC commodity classification into ISIC. Prior to 1999, bilateral trade data for Belgium and Luxembourg are reported jointly. Therefore trade, output and emissions data of both countries are aggregated.

Sectoral $\mathbf{CO_2}$ emission coefficients. We use information on the level of sectoral CO_2 emissions from fuel combustion reported by IEA.[21] In order to obtain emission coefficients, we need to divide sectoral emission levels by some measure of sectoral output. Whenever possible, output data come from the OECD's Structural Analysis Database (STAN). When data is missing, we use the Industrial Statistics Database (INDSTAT4) of the UNIDO and the UN System of National Accounts. For some countries and years, however, sectoral output data are missing altogether so that our data set is unbalanced. This is no major problem for our econometric analysis. A detailed data description is relegated to Appendix B and D.

Other covariates. GDP per capita (in constant 2000 US $) stems from the World Development Indicators (WDI) 2010 database. Bilateral distance measures and dummies for contiguity and common language are taken from the CEPII distances database. Information on joint FTA membership comes from the WTO. The EU and WTO dummy are constructed from the homepage of the EU and WTO, respectively. Information on the Kyoto status of countries is obtained from the UNFCCC homepage. For summary statistics of covariates, see Table 4.1.

4.3.2 Descriptive statistics

We start by looking at trends in country pairs' carbon content of trade. To visualize the data, we divide the sample into a pre-treatment period (1997-2000) and a post-treatment period (2004-07). All "Kyoto countries" (i.e., countries with binding Kyoto commitments)

[20] We do not have information on bilateral service trade.

[21] Other sources of carbon dioxide emissions such as fugitive emissions, industrial processes or waste are disregarded. However, CO_2 emissions from fuel combustion make up 80% of total CO_2 emissions.

Table 4.1: Summary statistics of covariates

Variable	Observations	Mean	Std.dev	Min	Max
FTA	223,499	0.49	0.50	0.00	1.00
Joint WTO	223,499	0.92	0.28	0.00	1.00
Joint EU	223,499	0.17	0.38	0.00	1.00
Ln GDP$_m$	223,499	9.21	1.12	5.92	10.65
Ln GDP$_x$	223,499	9.20	1.13	5.92	10.65
Ln distance	223,499	8.26	1.14	4.09	9.88
Contiguity	223,499	0.06	0.23	0.00	1.00
Common language	223,499	0.10	0.30	0.00	1.00

Note: The table displays summary statistics of covariates for the sample period 1995-2007.

except Russia have ratified the Protocol between 2001-03,[22] and we choose the pre- and post-treatment periods to be symmetric around this treatment window. The black bars in Figure 4.1 show the difference between *average sectoral bilateral* imports, carbon intensity of bilateral imports and the CO_2 content of imports (measured using the SRIO method) for an average country pair in three different groups: country pairs where only the exporter is a Kyoto country but not the importer (-1), country pairs where both or no country is a Kyoto country (0), and pairs where only the importer has a Kyoto commitment but not the exporter (1).

The left panel in Figure 4.1 shows bilateral imports. It is evident that trade has increased significantly in all groups. With an average of 57%, imports of non-Kyoto countries from Kyoto trade partners have risen the least. For pairs with no or two Kyoto countries, imports have increased most: by about 75%. The black line shows the linear fit and the shaded area the 95% confidence interval of a first-differenced regression of $\ln Q_{ij,t}$ on $Kyoto_{i,t} - Kyoto_{j,t}$ where t refers to the pre- or post-treatment periods. The resulting coefficient is 0.08. It is statistically significant at the 1% level. So, Kyoto importers have a higher increase in import volumes from non-committed countries.

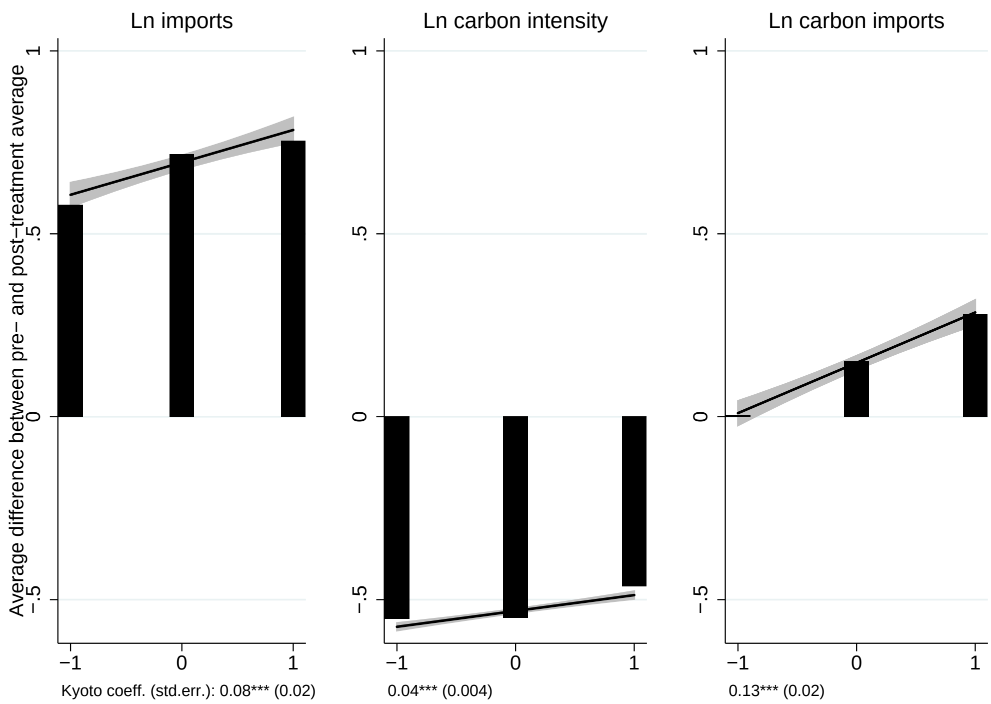
The results on the carbon intensity of bilateral imports in the middle panel are striking. The carbon intensity of imports has dropped dramatically, reflecting fuel-saving technological progress and/or a shift towards greener varieties. For instance, it has fallen by half for the group with two or no Kyoto countries. But country pairs with a differential Kyoto commitment of 1 (importer but not exporter committed) have seen the smallest decrease in

[22] Russia, which ratified in 2004, is treated as Kyoto country. Since Australia ratified in late 2007, it is treated as non-Kyoto country.

the carbon intensity of their imports. The regression yields a coefficient of differential Kyoto commitment of 0.04, statistically significant at the 1% level.

Finally, the right panel in Figure 4.1 repeats the same exercise for the carbon content of imports. It shows that, for a pair with only a committed importer, the carbon content of imports grew by about 28%, on average. For country pairs, where either both countries or neither of them have Kyoto commitments, the carbon content of trade has risen, on average, by 15% and for country pairs with only a Kyoto exporter by roughly 1%. The first-differenced regression of $\ln EM_{ij,t}$ on $(Kyoto_{i,t} - Kyoto_{j,t})$ gives a coefficient of 0.13 (statistically significant at the 1% level). This suggests that a Kyoto country increases its carbon content of imports after treatment by more than a similar non-Kyoto country.

Figure 4.1: Differential Kyoto commitment and imports, carbon intensity and carbon content of trade



Note: The bars show the average difference between pre-treatment (1997-2000) and post-treatment (2004-07) averages for country pairs where only the exporter has a commitment under Kyoto (-1), both countries or none of the two have Kyoto commitments (0), or only the importer has Kyoto commitments (1). The black line shows a linear fit and the shaded area the 95% confidence interval of a regression of the respective average sectoral bilateral variable on differential Kyoto commitment.

The results displayed in Figure 4.1 are suggestive and turn out to qualitatively confirm evidence based on more elaborate econometrics.[23] They are, however, plagued by a number of

[23] Figure 4.1 does not deflate import values. Using the importer's GDP deflator leaves the direction of the effects unchanged. Results are less robust when working with the exporter's GDP deflator. The right

potentially important problems. First, as discussed above, both the value and CO_2 content of imports are driven by confounding factors that are not taken account of in the figure. Second, Kyoto commitments may be endogenous. The next section addresses these issues.

Next, we turn to *sectoral bilateral* data. The summary statistics of Table 4.2 show that carbon intensities of imports differ significantly across sectors: they are particularly high in sectors 2 (mining, electricity), 3 (metals) or 5 (other minerals) and particularly low in sectors 1 (agriculture) and 8 (food products). On average, carbon imports are highest in the machinery, and mining and electricity sectors with 652 and 427 Mt of CO_2, respectively, followed by basic metals, non-specified industries and chemicals. Note that the average carbon import for the very energy-intensive mineral products sector is only 84 Mt of CO_2 – presumably because these products (like cement or clay) have high transportation costs. The carbon intensities also differ across different blocs of countries: the carbon intensity is highest for country pairs with a committed importer only and lowest for pairs with a committed exporter only. For the value of imports, no such clear pattern emerges.

4.4 Empirical strategy

In the following, we estimate gravity equations of the types derived in equation (4.4) and (4.6). For the moment, let us focus on the carbon content of trade. Taking logarithms on (4.6), one obtains a gravity equation for emissions embodied in bilateral imports that bears strong formal similarity to the standard gravity equation for bilateral trade in goods.

$$\begin{aligned} \ln E_{mxt} &= \alpha + \tilde{\kappa}_E(\ln t_{mt} - \ln t_{xt}) + \boldsymbol{W}_{mt}\beta_m + \boldsymbol{W}_{xt}\beta_x \\ &\quad -\sigma \ln \tau_{mx} + u_{mxt}, \end{aligned} \tag{4.12}$$

where α is a constant depending on μ, ω, σ and β; $\boldsymbol{W}_{mt}$ and $\boldsymbol{W}_{mt}$ are country-specific vectors of proxies for market and supply capacity, i.e. they include GDPs and multilateral resistance terms (and maybe technology) and u_{mxt} is an i.i.d. error term. The coefficient of interest is $\tilde{\kappa}_E$. It measures how the carbon content of imports is affected by differences in climate policy stringency. In the same manner, we are interested in the effect of climate policy on imports, carbon intensity and carbon terms of trade.

choice of the deflator is a contentious issue in the gravity literature. Fortunately, in the following analysis, the extensive use of country-and-time fixed effects makes deflation redundant (see Baldwin and Taglioni, 2007, for a similar argument).

Table 4.2: Summary statistics of the dependent variables

Sector			Differential Kyoto commitment ($Kyoto_m$-$Kyoto_x$)					
			1: Only importer		0: None		-1: Only exporter	
		Variable	Mean	Std.dev	Mean	Std.dev	Mean	Std.dev
7	Machinery	Q_{mx}	644.4	2,894.7	716.2	2,762.3	729.5	3,131.2
		η_x	0.8	0.9	0.7	1.0	0.4	0.5
		E_{mx}	652.1	3,201.1	295.1	2,528.4	154.9	741.4
2	Mining	Q_{mx}	87.6	368.9	43.1	190.1	52.4	327.7
		η_x	5.5	4.5	5.0	6.5	3.0	2.9
		E_{mx}	427.1	1,650.5	186.8	854.9	112.4	638.1
3	Basic metal	Q_{mx}	163.8	619.3	161.0	538.2	144.1	722.7
		η_x	2.7	2.3	2.5	2.8	1.6	1.8
		E_{mx}	404.0	1,460.7	299.3	1,318.5	166.7	842.6
12	Non-specified	Q_{mx}	307.5	1,315.2	268.1	1,013.0	274.1	1164.6
		η_x	1.9	1.7	1.2	1.6	0.5	0.4
		E_{mx}	358.1	1,336.2	192.9	1,524.8	116.0	723.0
4	Chemicals	Q_{mx}	272.3	1,275.3	334.9	1,131.1	379.1	1,411.7
		η_x	1.4	1.1	1.4	1.8	1.0	1.4
		E_{mx}	337.2	1361.8	228.7	797.3	163.8	634.5
11	Textiles	Q_{mx}	192.0	859.4	144.3	608.5	61.3	285.8
		η_x	0.7	0.6	0.5	0.7	0.3	0.3
		E_{mx}	229.8	1,152.8	93.7	773.2	15.3	70.3
6	Transport equip.	Q_{mx}	287.6	2,204.5	260.9	1,238.4	289.6	1,709.6
		η_x	0.7	0.8	0.5	0.9	0.3	0.4
		E_{mx}	162.2	950.9	80.7	466.1	61.5	391.1
5	Mineral products	Q_{mx}	36.2	175.7	46.9	160.3	38.9	171.9
		η_x	2.7	2.3	2.3	2.6	1.3	1.0
		E_{mx}	83.9	362.1	73.2	338.8	38.1	159.2
8	Food	Q_{mx}	121.5	516.0	149.4	483.4	82.6	430.9
		η_x	0.6	0.5	0.7	0.6	0.4	0.4
		E_{mx}	78.5	380.5	65.1	239.8	26.1	137.5
1	Agriculture	Q_{mx}	61.6	246.7	55.7	222.0	18.5	133.7
		η_x	0.6	0.3	0.6	0.5	0.5	0.4
		E_{mx}	38.1	174.1	28.0	120.2	8.7	75.2
9	Paper	Q_{mx}	58.1	437.5	94.2	349.0	54.8	376.6
		η_x	0.9	0.8	0.7	1.0	0.4	0.4
		E_{mx}	35.6	226.2	41.4	191.7	22.4	198.8
10	Wood	Q_{mx}	25.3	110.4	23.3	106.3	19.6	267.3
		η_x	0.7	0.6	0.6	0.8	0.4	0.4
		E_{mx}	25.6	144.7	14.0	106.3	8.0	116.5

Note: The table displays summary statistics of dependent variables sector-by-sector. Sectors are sorted in descending order of average CO_2 content of trade. For a detailed sector description with ISIC codes, see Table B.1 in Appendix B. Q_{mx}: Imports are in Mio US-$, η_x: CO_2 intensity of imports in kg CO_2 per US-$ and CO_2 content of imports in kt CO_2.

The empirical implementation of our gravity equations encounters a number of econometric problems. First, climate policy stringency is hard to measure because the available policy instruments are plentiful. Instead, we evaluate a quasi-experiment: the Kyoto Protocol. Under the auspices of this treaty some countries have agreed to emission caps. Second, in a model like (4.12) Kyoto ratification is most likely endogenous. Unobservable determinants of bilateral trade volumes as well as carbon content of trade might be correlated with Kyoto membership causing omitted variables bias (self-selection). Third, reverse causality might constitute a problem when trade flows or CO_2 intensities influence the ratification decision. In the following, we address these issues in turn.

4.4.1 Measuring the Kyoto effect

In the theoretical part, we investigate the effects of carbon prices on trade, embodied CO_2 intensity, carbon terms of trade and virtual CO_2 flows. In our empirical estimation, we are interested in a specific "natural" experiment: the ratification of the Kyoto Protocol.

Some policy background: The treaty was adopted in 1997 in Kyoto. It entered into force in 2005 after a critical mass of countries had ratified it in national parliament. For the period 2008-2012 the Kyoto Protocol aims at reducing anthropogenic greenhouse gas emissions by 5.2% with respect to the base year 1990. Each country has a specific emission reduction target. And countries are free in their choice of climate policy instruments to reach it. Most Kyoto countries started an emission cap-and-trade system (EU countries and Norway in 2005 and Switzerland and New Zealand in 2008) while some (Canada, Japan, and Russia) did not.[24]

However, this is only a small part of climate policy activities. Many countries invested in the extension of alternative energy sources (e.g. with feed-in tariffs or subsidies to R&D), passed regulations on gasoline consumption for cars or housing isolation, required labeling for electric equipment, etc. Due to this plethora of different policy activities, it is hard to assess whether Kyoto countries have increased their climate policy stringency. Yet, we attempt to provide some illustrative evidence that the Kyoto Protocol mattered for emission regulation.

The *IEA Policies and Measures: Addressing Climate Change* database contains information on energy-related policies aimed at reducing greenhouse gas emissions. It records 1,126

[24] These countries have not extended their GHG limitations for the second Kyoto commitment period from 2013-2020.

policy initiatives in force in the IEA member countries plus eleven non-members[25] and the European Union over the period 1974-2011. Policy types included are economic instruments such as feed-in tariffs or GHG emission allowances; information and education; policy support; regulatory instruments; research, development and deployment; and voluntary approaches. Examples are the EU's communication "20 20 by 2020: Europe's Climate Change Opportunity" in 2008, the UK's climate change levy in 2001, Germany's Fifth Energy Research Programme in 2005, or France's renewable energy feed-in tariffs in 2006.

The reported policies certainly vary in terms of their scope and intensity. To provide an overall picture we nevertheless count the initiatives per country and year.[26] The upper part of Figure 4.2 shows the average number of policies and measures entering into force per year for Kyoto (solid line) and non-Kyoto (dashed line) countries. The red solid line marks 2002: the year of Kyoto ratification in most Kyoto countries. In the early 1990s, climate policies start to occur more frequently. However before 2002, the average number of policies and measures is remarkably similar in both country groups. The only exception being a small spike in 2001 for Kyoto countries. After 2002, the gap between climate policies in Kyoto and non-Kyoto countries starts widening. This pattern is also visible in the average stock of climate policies and measures in the lower panel of Figure 4.2.[27] In summary, the figures suggest increased policy action in Kyoto countries after ratification.

Recently, two studies find that Kyoto ratifiers significantly reduced their CO_2 emissions (Aichele and Felbermayr, 2012; Grunewald and Martínez-Zarzoso, 2011). There is also evidence that Kyoto countries have a cleaner energy mix and higher fuel prices (Aichele and Felbermayr, 2011b).

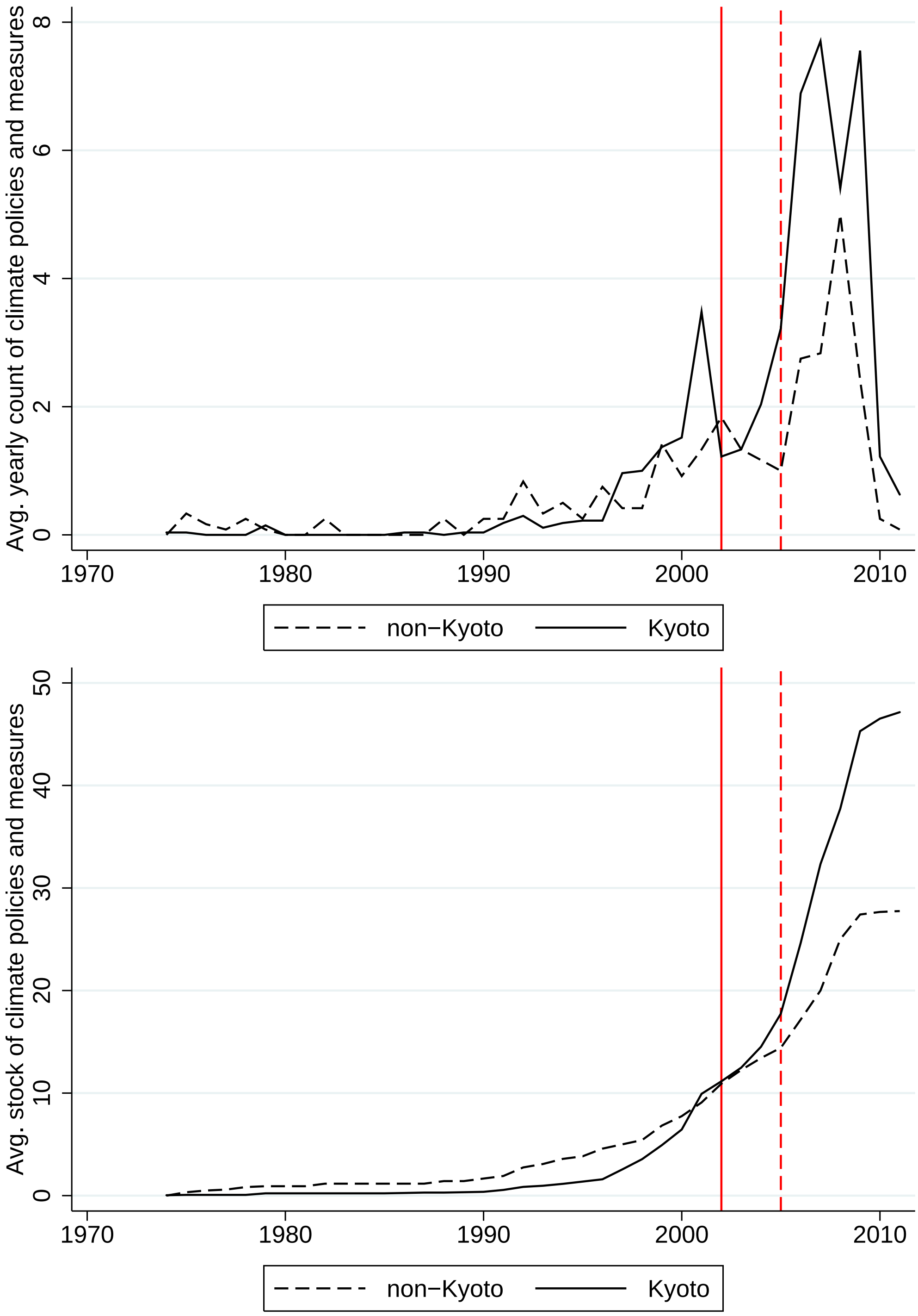
In summary, the Kyoto Protocol seems to matter. Nevertheless, measuring the implementation of and policy stringency under the Protocol is hard. Therefore, we work with a simple catch-all variable in our empirical approach: a dummy variable for Kyoto commitment. It is clear that a Kyoto dummy is only a very inaccurate measure for the scope and intensity of a country's treatment under the Protocol. The same problem occurs in the related literature on the trade flow effects of international agreements. For example, the degree of trade liberalization varies for different free trade agreements (FTA). Yet, researchers simply define

[25] Brazil, China, Algeria, Ghana, India, Iceland, Israel, Mexico, Malaysia, Russia, South Africa.

[26] EU member countries inherit the EU initiatives.

[27] The database also includes policies that have ended or were superseded. Unfortunately, the IEA database does not contain information on the corresponding dates, hence ended or superseded policies are included in the stock.

Figure 4.2: Average climate regulation activities and stock



Note: The graph shows for both Kyoto (solid line) and non-Kyoto (dashed line) countries the average number of climate regulation activities per year in the upper part, and the average stock of climate policies and measures in the lower part from 1974-2011. Data is obtained from the IEA Policies and Measures: Addressing Climate Change database. The red solid line signifies the date of Kyoto ratification for most Kyoto countries. The red dashed line marks the entry into force of the Kyoto Protocol.

a binary variable that takes value 1 if the two members of a country pair are both members in the same FTA (Baier and Bergstrand, 2007), or the WTO (Rose, 2004a), or if they are in a currency union (Baldwin and Taglioni, 2007), and so forth. Studies evaluating the treatment effects of international environmental agreements such as the Montreal, Helsinki or Oslo Protocols take a similar stance and code a binary variable, see e.g. Ringquist and Kostadinova (2005). Following the literature, we take the year of ratification of any binding commitments within the Kyoto Protocol as the decisive indicator for a country's stance of climate policy.

In our regressions we use

$$Kyoto_{i,t} = \begin{cases} 1 & \text{if country } i \text{ has a binding emission cap and } t \geq \text{year of ratification} \\ 0 & \text{else} \end{cases},$$

where t indexes years. For instance, $Kyoto_{i,t} = 0$ for a country i that has not ratified the Protocol yet or has no binding emission targets under the Protocol. The ratification of commitments by national parliaments started in 2001 (Czech Republic and Romania). Most countries have ratified in the years 2002 and 2003; Russia has ratified the Protocol in 2004 and Australia has ratified in 2007. So, there is some heterogeneity in the timing of ratification.

We assume ratification is the decisive treatment date. Two alternative dates come to mind: signature and entry into force. We discard signature in 1997 as treatment date because it is just an expression of intention. And we expect anticipation effects already before entry into force in 2005. Once a national parliament ratifies Kyoto commitment, the political course is set and economic agents adapt their expectations.[28] This explains our choice of treatment date. In a robustness check we use a long differences-in-differences estimator on pre- and post-treatment averages. By defining a broad treatment window (2001-2003), this uncertainty about the exact timing of treatment is less severe.

4.4.2 Omitted variables bias

A challenge in gravity modeling is how to deal with country-pair specific unobserved heterogeneity, due, for instance, to imperfect observability of trade costs τ_{mx}. In our context,

[28] Ratification in national parliament might also be anticipated. But since there is a lot of time until entry into force and Kyoto's entry into force hinged on ratification in a critical mass of countries, we believe this anticipation effect is not relevant.

also classic determinants of comparative advantage like differences in endowments, climatic conditions or preferences for the environment in a country pair might affect trade flows as well as the decision to select into the Kyoto Protocol leading to omitted variable bias on Kyoto estimates. For this reason, we use fixed-effects estimation (i.e., include country-pair effects into the regression) in equation (4.12).[29] A differences-in-differences strategy has the advantage of controlling for all historical (e.g., the bilateral trade position at the beginning of the sample) and geographical determinants that may have lead to self-selection of countries into the Kyoto Protocol and FTAs. This strategy effectively controls for *all* time-invariant determinants of bilateral trade in CO_2, including determinants that are country-specific and not country-pair specific. So a within transformation of the outcome variables brings us closer to an interpretation as causal effect. An important assumption with this identification strategy is that, within country-pair-sector, trends in outcome variables are the same in the counterfactual (i.e. a world without the Kyoto Protocol) than in the control group. The control group consists of country pairs with the same Kyoto status, i.e. pairs where either both countries or none are Kyoto countries. We test this assumption with placebo treatments.

However, the within transformation fails to control for unobserved changes in country-specific characteristics (e.g., if a change in consumer preferences leads at the same time to less carbon imports and to stricter climate control policies).[30] This constitutes an additional source of omitted variables bias.

4.4.3 Selection into Kyoto

There are many reasons to believe that selection into Kyoto membership is not random. We can think of the Kyoto Protocol as outcome of a political cost-benefit analysis that is influenced by economic, environmental and political determinants.[31] Signing on to an emission cap brings certain environmental and reputational benefits. These benefits are country specific. For example, a country's geographic location influences its exposure to the adverse effects of global warming. The benefits could also vary over time because environmental preferences or the awareness for environmental problems may change over

[29] This is proposed by Baier and Bergstrand (2007) to estimate treatment effects of joint FTA membership that suffers from the same problem.

[30] Country-and-time effects would pick this effect up if it would be proportional for all trade partners.

[31] See also a growing literature on international environmental agreement (IEA) formation, for example Murdoch and Sandler (1997); Beron et al. (2003); Egger et al. (2011a).

the course of years; especially when a country's per-capita GDP increases. We summarize the benefits in the threshold χ_{it}.

On the other hand, climate policy is costly. First, it requires resources for cleaning-up. These costs are lower for a country where carbon-free opportunities to produce energy are abundant. Countries well endowed with oil, coal and other natural resources might be reluctant to join the Kyoto Protocol because they want to keep global energy prices high. York (2005) finds a negative relationship between Kyoto membership and population growth. Costs of signing up are higher because a growing population means more stress on the environment. And politically stable countries might find it easier to commit to long-term goals like climate change mitigation. Second, climate policy reduces a country's international competitiveness. It is important to note that the competitiveness loss relates to *a country's position relative to the whole world*: if it unilaterally decides to cap its emissions, its cost competitiveness relative to *all* trading partners changes. It will join if the share of imports that come from countries that are likely to adopt caps as well is large enough and refuse to join in the opposite case. Egger et al. (2011a) stress the importance of openness for IEA membership. Some of the determinants are observable and summarized in the vector $\boldsymbol{W}_{it}$ while others are unobservable and collected in the vector $\boldsymbol{O}_{it}$.

From this discussion, it becomes apparent that Kyoto ratification is a country-and-time specific decision. It does not depend on bilateral variables because it is a multilateral agreement. This modeling assumption is standard in the literature on IEA membership. More importantly, as soon as the (unobservable) country-specific threshold χ_{it} exceeds costs, the country's cost-benefit analysis is in favor of Kyoto membership and it ratifies the treaty.

With these considerations in mind, we can write down a simple reduced form or latent variable model of Kyoto membership.

$$Kyoto_{i,t} = \begin{cases} 1 & \text{if } \delta_0 + \boldsymbol{W}_{i,t}\delta_1 + \boldsymbol{O}_{i,t}\delta_2 \leq \chi_{i,t} \\ 0 & \text{if } \delta_0 + \boldsymbol{W}_{i,t}\delta_1 + \boldsymbol{O}_{i,t}\delta_2 > \chi_{i,t} \end{cases} . \tag{4.13}$$

Let us go back to equation (4.12). If the error term in the short (within-transformed) model consists of an unobservable and an error component $u_{mxt} = \theta_1 \boldsymbol{O}_{m,t} + \theta_2 \boldsymbol{O}_{x,t} + r_{mxt}$, where r_{mxt} is a random, i.i.d. shock, the OLS estimator will be biased. In other words, if the country-and-time specific unobservables matter for the carbon content of trade and are correlated with Kyoto, the Kyoto coefficient still suffers from omitted variables bias. An example is technological change. It affects exporter's unit costs and thus CO_2 intensity and the carbon content of trade. It might also matter for Kyoto selection since efficiency gains bring down resource use.

Fortunately, the structure of our data offers some ways to control for the relevant determinants of ratifying binding obligations. Since we observe *country pairs* for a series of years in every sector, we can control for country characteristics at each year by including dummies for each country-and-year combination. Since we have 13 years and 40 countries, this amounts to including a maximum of 520 dummy variables, each representing a country's situation at a given year. As long as a country's decision to join the Protocol is *multilateral* (does not depend on individual trade partners but on their aggregate), inclusion of these country-and-year effects controls for all conceivable determinants of Kyoto commitments. As the dependent variable in our gravity equation is *bilateral*, we still have variance left to identify the effect of *differential* Kyoto commitment.

The major drawback of this strategy is that we can only identify the effect of two countries' differences in Kyoto status rather than each country's Kyoto status separately. However, relative to an instrumental variables approach, its key advantage is its generality. It is difficult to find a convincing instrumental variable; the validity of the exclusion restriction can only be assumed. A second advantage of our strategy lies in the fact that it makes estimation of our gravity equation particularly easy: we do not have to use proxies for the importer's demand capacity and the exporter's supply capacity.

Note also that country-pair-time specific shocks might be a problem for our identification strategy. But only if they affect trade flows and at the same time are correlated with differential Kyoto commitment. Then there is still an omitted variables bias. These shocks have to be pair-time specific. Concurrent country-specific shocks on the other hand are no problem because they are filtered out (country-time shocks affect all observations of this country, so they are absorbed by country-and-time dummies).

4.4.4 Reverse causality

So far, we have discussed endogeneity bias due to omitted variables. Reverse causality might also be a problem when trade patterns determine Kyoto Protocol ratification instead of the other way around. As we have argued above, countries will base their ratification decision on the trading possibilities with respect to all trade partners, i.e. on their multilateral openness. Single bilateral import and export flows are too small to matter – at least for small, unimportant trade partners. This is even more true for sectoral bilateral trade flows. Though plausible in most cases, the small country assumption might be violated for the US or China. So we also employ an instrumental variables approach to address reverse causality.

4.4.5 Regressions estimated

Our empirical strategy can be summarized as follows. We are interested in the impact of differential Kyoto commitment on the four outcome variables imports, embodied CO_2 intensity of imports, carbon terms of trade and carbon content of imports. We eliminate the country-pair effects by applying the within-transformation operation.[32] Note that country pair effects $\nu_m \times \nu_x$ nest simple country effects ν_m and ν_x. To address the remaining omitted variables bias due to country-time specific unobservables (like country-specific technological changes), we include country-and-time dummies $\nu_m \times \nu_t$ and $\nu_x \times \nu_t$.

Let $\mathbf{POL}_{mxt}$ be a vector of trade policy variables in dummy form (common WTO, FTA and EU membership) and u_{mxt} an error term with the usual properties. These considerations lead us to write (4.6) in estimable form as

$$\begin{aligned} \ln Y_{mxt} &= \kappa_Y \left(Kyoto_{mt} - Kyoto_{xt} \right) + \mathbf{POL}_{mxt} \boldsymbol{\gamma} \\ &\quad + \nu_m \times \nu_t + \nu_x \times \nu_t + \nu_m \times \nu_x + u_{mxt}, \end{aligned} \tag{4.14}$$

where $Y \in \{Q, \eta, CCT, E\}$ and sectoral indexes are again suppressed.[33] The κ_Y- coefficients measure the percent change of the variable Y with differential Kyoto commitment in the importer and exporter. They differ from those in (4.9) in that they are not elasticities due to the binary nature of the treatment variable. When the dependent variable is the log of imports (in U.S. dollar), Q, we refer to the respective Kyoto coefficient κ_Q as *scale effect*. The respective Kyoto coefficient κ_η for regressions using the log of the embodied CO_2 intensity of imports, η, is referred to as *technique effect*. By equation (4.9), the scale and technique effect add up to the total effect on virtual CO_2 imports, $\kappa_E = \kappa_Q + \kappa_\eta$.

[32] Alternatively we could also first difference our data. But, due to the unbalanced nature of the panel, time-differencing implies a substantial loss in degrees of freedom. Moreover, it is well known that FE estimation is preferable when error terms are serially uncorrelated while FD is better when they follow a random walk. So, we report FE estimates and relegate FD estimates to Appendix D Table D.2.

[33] It is understood that covariates' coefficients are estimated separately for each dependent variable. To keep notation simple, the respective $Y-$index is supressed.

Our identifying assumptions are as follows: (i) Kyoto ratification is the decisive treatment date. (ii) There are no anticipation effects in Kyoto countries prior to ratification. (iii) Within country-pair sector trends in outcome variables are the same in the counterfactual (i.e. a world without the Kyoto Protocol) than in the control group. (iv) Kyoto ratification is country-and-time specific and not bilateral.[34] (v) Country-pair-sector-time specific shocks other than changes in trade costs (joint FTA, EU, WTO membership) are uncorrelated with Kyoto ratification. (vi) There is no reverse causality, i.e. bilateral sectoral trade flows do not matter for Kyoto ratification. When our identification strategy holds, we can interpret κ_Y as causal effect of Kyoto ratification.

We pool across sectors and run a single regression, treating country-pair × sector as the cross-sectional dimension. We also run equation (4.14) separately for each of our twelve sectors and four dependent variables.

4.5 Results

4.5.1 Pooled regressions: benchmark

For a first analysis, we pool our sectoral data. So the following analysis is based on 223,499 bilateral sectoral flows (or 18,588 clusters). Table 4.3 provides the results of log-linear regressions as in equation (4.14). A within transformation deals with unobserved time-invariant country-pair-sector heterogeneity. All models include a full set of interactions between country and year effects, so that only variables that vary across country-pairs *and* time (such as differential Kyoto commitment, or the stance of bilateral trade policy) can be identified.[35] Under the identifying assumptions in Section 4.4, these regressions account for all conceivable reasons for which a country may wish to adopt binding Kyoto commitments.

Column (1) uses the log of imports (in U.S. dollar), Q, as the dependent variable. The estimated scale effect of Kyoto commitment is 0.050 and statistically significant at the 1% level. It implies that differential Kyoto commitment (either the importer is committed and the exporter is not, or the reverse) increases imports by about 5%. This suggests a negative competitiveness effect of Kyoto commitment. Production apparently relocates to non-Kyoto

[34] Time-invariant country-pair selection e.g. through differences in endowments are no problem to our identification strategy either. This source of omitted variable bias is taken care of by within transformation.

[35] Results without country-and-time dummies and separate Kyoto dummies for the importer and exporter can be found in the working paper version.

Table 4.3: Benchmark: Regressions on pooled data

Dep. variable:	(1) Ln imports, Q_{mx}	(2) Ln CO_2 intensity of imports, η_x	(3) Ln CTT, η_i/η_j	(4) Ln CO_2 imports, E_{mx}
Kyoto_m-Kyoto_x	0.050***	0.028***	-0.043***	0.078***
	(0.011)	(0.003)	(0.005)	(0.011)
Joint FTA membership	0.103***	0.010		0.113***
	(0.031)	(0.008)		(0.032)
Joint WTO membership	-0.144	-0.001		-0.144
	(0.163)	(0.036)		(0.165)
Joint EU membership	0.019	0.019**		0.038
	(0.035)	(0.009)		(0.035)
Country × year effects	YES	YES	YES	YES
Country-pair sector effects	YES	YES	YES	YES
Observations	223,499	223,499	215,917	223,499
No. of countrypair-sectors	18,588	18,588	18,387	18,588
Adj. R^2	0.206	0.709	0.036	0.074
F-stat	46.316	879.091	11.691	15.245
RMSE	0.829	0.179	0.305	0.849

Note: Fixed-effects (FE) panel regressions on pooled sectoral data. Heteroskedasticity-robust standard errors (in brackets) are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. CTT: sectoral carbon terms of trade a la Antweiler (1996); computed as emission intensity of exporter over emission intensity of importer in a given sector.

countries. It is in line with carbon leakage. The coefficients obtained on covariates are fairly standard in gravity estimation. We find that creation of a bilateral free trade agreement increases bilateral imports by about 10% on average, statistically strongly significant. The coefficient of joint WTO membership is negative, but statistically not different from zero. Only Estonia (in 1999) and China (in 2001) change their WTO status in the observed time period, though. This makes inference hard. Joint EU membership has the expected positive sign, though the estimate is not statistically different from zero either. So we do not find a trade-enhancing effect from the EU eastern enlargement.[36] With our specification, we are able to explain 20% of the within variation in sectoral bilateral import flows.

We also find a positive technique effect of 0.028 in column (2), statistically strongly significant. Kyoto commitment of the importer but not the exporter increases imports' CO_2 intensity by almost 3% compared to the counterfactual of same Kyoto status. A finding

[36] We identify effects through within variation. The only new EU members in our observed time period are Eastern European countries in the 2004 enlargement wave.

consistent with the leakage hypothesis. Likewise, Kyoto commitment of the exporter but not the importer reduces the sectoral CO_2 intensity of imports: a cleaning-up effect in Kyoto countries. On a sidenote, the estimated technique effect could be the result of true changes in the CO_2 intensity of procution: Kyoto countries use cleaner and/or non-Kyoto countries dirtier technologies. But since our sectors are very broad categories (two digits ISIC or more) it could partly also be a *within*-sector composition effect. I.e. within a sector, the composition of trade becomes more dirty with Kyoto commitment. With our data, we cannot disentangle the two. Either effect is consistent with leakage. As might be expected, we do not detect an influence of joint FTA or WTO membership on the CO_2 intensity of imports. Interestingly, the coefficient on joint EU membership is 0.019 and strongly statistically signficant. It seems old EU member countries source out CO_2-intensive goods production to Eastern Europe. The positive sign on the technique effect in column (2) is supported by a negative Kyoto estimate in the carbon terms of trade specification in column (3).[37] Differential Kyoto commitment leads to a decline in the CTT of about 4%. So Kyoto countries' exports become less CO_2-intensive in comparison to their imports.

Finally, differential Kyoto commitment increases bilateral CO_2 imports by roughly 8%. See column (4). This is again consistent with carbon leakage. The positive Kyoto effect implies that, compared to the counterfactual, additional emissions occur in non-Kyoto countries and are then virtually "imported" by Kyoto countries. Since the scale and technique effect add up to the effect on virtual CO_2 imports, about two thirds of it are attributable to the scale effect. The remaining third is explained by the technique effect.

In sum, there is strong evidence that Kyoto policies have had non-negligible effects on the quantity, carbon intensity and total carbon content of bilateral import flows.

4.5.2 Pooled regressions: sources of endogeneity bias

Now that we have established a benchmark, we would like to understand the *sources of endogeneity bias*. Tables 4.4 and 4.5 present alternative specifications of the gravity of imports and CO_2 imports for pooled sectoral data. The focus is on the obtained Kyoto estimates. Let us start with import flows. Column (5) replicates the benchmark model for easier comparison. The model estimated in Table 4.4 column (1) is based on OLS estimation

[37] Note that the number of observations drops slightly because the CTT is not identified if either exports or imports in a country pair are zero.

Table 4.4: Endogeneity bias: OLS, FE, FE-IV and FE with country-and-time dummies

Dependent variable: Ln imports, Q_{mx}					
	(1)	(2)	(3)	(4)	(5)
Method:	OLS, 2006	OLS 1995-2007	FE	FE-IV	FE, country× time dummies
Kyoto_m-Kyoto_x	0.130***	0.071***	0.056***	0.060*	0.050***
	(0.039)	(0.014)	(0.011)	(0.033)	(0.011)
ln GDP_m	0.263***	0.304***	1.908***	1.909***	
	(0.023)	(0.006)	(0.063)	(0.060)	
ln GDP_x	0.267***	0.288***	1.142***	1.142***	
	(0.049)	(0.064)	(0.061)	(0.061)	
Ln distance	-1.258***	-1.226***			
	(0.045)	(0.012)			
Contiguity (0,1)	0.493***	0.532***			
	(0.089)	(0.023)			
Common language (0,1)	0.664***	0.620***			
	(0.084)	(0.022)			
Joint FTA membership	0.399***	0.265***	0.136***	0.136***	0.103***
	(0.083)	(0.022)	(0.032)	(0.030)	(0.031)
Joint WTO membership	11.672***	0.113	-0.063	-0.062	-0.144
	(2.804)	(0.146)	(0.161)	(0.154)	(0.163)
Joint EU membership	-0.179*	-0.166***	0.179***	0.180***	0.019
	(0.098)	(0.025)	(0.020)	(0.020)	(0.035)
MR terms[a]	YES	YES	YES	YES	
Year effects		YES	YES	YES	
Country × year effects					YES
Country-pair sector effects			YES	YES	YES
Observations	17,239	223,499	223,499	223,387	223,499
No. of countrypair-sectors			18,588	18,476	18,588
Adj. R^2	0.226	0.264	0.181	0.107	0.206
Underid test (p-value)				0.000	
Partial R^2				0.081	

Note: Regressions on pooled sectoral data. (1) and (2) OLS estimations in cross-section 2006 and pooled 1995-2007. (3)-(5) Fixed-effects (FE) panel regressions. (4) IV: Kyoto_m-Kyoto_x instrumented with ratification of the Statutes of the International Criminal Court (ICC_m-ICC_x). Heteroskedasticity-robust standard errors (in brackets). In FE estimation standard errors are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively.
[a] All relevant multilateral resistance (MR) control variables included (i.e., FTA, WTO, EU, distance, contiguity, common language); see Baier and Bergstrand (2009a).

in the cross-section 2006.[38] As is standard in cross-sectional gravity, the importer's and

[38] Our dataset comprises many missings in 2007. So here we use the cross-section of 2006 instead of the last sample year 2007.

exporter's GDP in logs control for market size and the log of bilateral distance and dummies for contiguity, common language and joint FTA, WTO and EU membership proxy for bilateral trade costs. Linear approximations of multilateral resistance terms a la Baier and Bergstrand (2009a) are also included. The estimated Kyoto coefficient is 0.13 and statistically significant at the 1% level. This implies a Kyoto effect of about 13.9% – 2.7 times larger than the benchmark estimate of 5.1%.

Column (2) presents results from pooling all sample years and estimating the same specification as in (1) albeit with year dummies to capture cyclical events like business cycles or oil price shocks. The Kyoto estimate is 0.071, again highly statistically significant. The obtained estimate is about one and a half times larger than in the benchmark. Exploiting the panel dimension and using country-pair-sector fixed effects estimation brings down the point estimate to 0.056 in column (3). It is about 12% larger than the benchmark estimate. Note that country-pair fixed effects also reduce the trade-increasing effect of FTAs from roughly 50% to about 15%; a standard result in the gravity literature.

In column (4) we investigate the issue of reverse causality. We use the IV strategy of Aichele and Felbermayr (2012) and instrument Kyoto ratification with ratification of the Statutes of the International Criminal Court. The instrument seems strong and the first stage (see Table D.3 in Appendix D) shows a strong positive correlation between the differential ICC and Kyoto status. The Kyoto effect is estimated to be about 6%, statistically significant at the 10% level. We conclude reverse causality does not drive our results.

In line with the self-selection argument brought forward in Sections 4.4.2 and 4.4.3, the inclusion of country-and-time effects in column (5) brings the Kyoto point estimate down from 0.056 to 0.050; a rather minor further adjustment.[39] In the trade specification, the main source for omitted variables bias are time-invariant factors.

We repeat the same exercise for the carbon content of imports in Table 4.5. The same pattern emerges. Controlling for country-pair-sector unobserved heterogeneity brings down the Kyoto estimate from 0.21 to 0.099. The inclusion of country-and-time effects further reduces the point estimate to 0.078 in the benchmark. While we overestimate Kyoto's effect by almost 200% in column (1), the bias is only 28% in column (3).

[39] The country-and-year effects present in (5), however, are jointly strongly significant (the F-statistic is 35.83).

Table 4.5: Endogeneity bias: OLS, FE, FE-IV and FE with country-and-time dummies

Dependent variable: Ln CO_2 imports, E_{mx}					
	(1)	(2)	(3)	(4)	(5)
Method:	OLS, 2006	OLS, country-and-time dummies	FE	FE-IV	FE, country-and-time dummies
Kyoto_m-Kyoto_x	0.210***	0.343***	0.099***	0.103***	0.078***
	(0.040)	(0.025)	(0.011)	(0.033)	(0.011)
ln GDP_m	0.225***		1.920***	1.921***	
	(0.024)		(0.064)	(0.062)	
ln GDP_x	-0.288***		-0.007	-0.008	
	(0.024)		(0.066)	(0.063)	
Joint FTA membership	0.415***	0.310***	0.130***	0.131***	0.113***
	(0.089)	(0.055)	(0.033)	(0.032)	(0.032)
Joint WTO membership	10.889***	1.023***	-0.039	-0.038	-0.144
	(2.959)	(0.297)	(0.166)	(0.159)	(0.165)
Joint EU membership	-0.197*	-0.193***	0.028	0.028	0.038
	(0.103)	(0.058)	(0.035)	(0.034)	(0.035)
MR terms[a]	YES		YES	YES	
Year effects			YES	YES	
Country × year effects		YES			YES
Country-pair sector effects		YES		YES	YES
Observations	17,239	223,499	223,499	223,387	223,499
No. of countrypair-sectors			18,588	18,476	18,588
Adj. R^2	0.226	0.471	0.051	-0.035	0.074
Underid test (p-value)				0.000	
Partial R^2				0.081	

Note: Regressions on pooled sectoral data. (1) and (2) OLS estimations in cross-section 2006 and pooled 1995-2007. (3)-(5) Fixed-effects (FE) panel regressions. (4) IV: Kyoto_m-Kyoto_x instrumented with ratification of the Statutes of the International Criminal Court (ICC_m-ICC_x). Heteroskedasticity-robust standard errors (in brackets). In FE estimation standard errors are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively.
[a] All relevant multilateral resistance (MR) control variables included (i.e., FTA, WTO, EU, distance, contiguity, common language); see Baier and Bergstrand (2009a).

In summary, time-invariant country-pair-sector unobserved heterogeneity – which nests country-specific unobserved heterogeneity – constitutes a large part of omitted variables bias. The bias reduction achieved with country-and-time dummies is small for trade flows. Since we cover a time span of 13 years only, this seems reasonable. The situation for the carbon content of trade flows is different, though. Without country-and-time dummies Kyoto's effect on virtual CO_2 flows is overestimated by about 30%; a considerable upward bias. This seems plausible as well. From theory we know that the carbon content depends on country's unit costs. Omission of countries' sectoral production technology leads to an upward bias when technological improvements are positively correlated to Kyoto. Rerverse causality, on the other hand, seems like a minor issue because sectoral bilateral trade is too small to matter for Kyoto ratification.

4.5.3 Pooled regressions: Placebo tests

A key identifying assumption of our approach is that the trends in imports and CO_2 imports would be the same in Kyoto and non-Kyoto countries in a counterfactual world without Kyoto ratification. Does this assumption qualify? A concern one might raise is that, by and large, Kyoto ratifiers are developed countries and, by and large, non-Kyoto countries are developing countries. The Kyoto effect we are picking up could also reflect developing countries moving out of dirty goods and developing countries moving into dirty goods and catching up in terms of their trade flows. To see whether the results are in line with this alternative interpretation of our findings or whether it is truly a Kyoto story, we run two placebo tests. The first placebo test is based on imposing fictitious dates of ratification. The second on comparing Kyoto countries to high income non-ratifiers like the US.

First, we check whether there have been different trends in imports, CO_2 intensity of imports and CO_2 imports between Kyoto and non-Kyoto countries before Kyoto ratification. To do this, we limit attention to the sample years before the Kyoto ratification process started, i.e. the years 1995-2000. We then estimate effects of a fictitious i.e. a placebo treatment of Kyoto countries in one of the pre-treatment years 1997 and 1998; dates right in the middle of the pre-treatment period. The estimation strategy and set of covariates is as in the benchmark.[40] If we were to find positive and significant effects of Kyoto commitment, this would indicate that, already prior to ratification, Kyoto countries have been on a different trend in their outcome variables than non-Kyoto countries.

Column (1) to (3) in Table 4.6 presents results.[41] In Panel A, the log of imports is the dependent variable. Results on the CO_2 intensity of imports appear in Panel B and results on virtual CO_2 imports in Panel C. Column (1) assumes fictitious Kyoto ratification in 1997, column (2) in 1998 and column (3) runs a regression on the fictitious pre- and post-treatment

[40] Note that the EU dummy is dropped because no country accessed the EU in the period 1995-2000 and hence there is no time variation to exploit.

[41] Full regression output is in Appendix D Tables D.4 to D.7.

Table 4.6: Placebo tests

	(1)	(2)	(3)	(4)	(5)	(6)
Placebo:	Fictitious treatment date			Comparison with high-income non-Kyoto only		
	1997	1998	1997	USA	AUS, ISR, KOR, USA	USA Kyoto ratifier
Method:	FE	FE	Long DiD	FE	FE	FE
Panel A: Ln imports						
Kyoto_m-Kyoto_x	-0.014	-0.053***	0.023	0.055***	0.055***	0.046***
	(0.020)	(0.020)	(0.019)	(0.012)	(0.012)	(0.011)
Observations	103,782	103,782	35,350	101,020	109,373	223,499
Adj. R^2	0.040	0.040	0.091	0.175	0.175	0.206
Panel B: Ln CO_2 intensity						
Kyoto_m-Kyoto_x	0.002	0.005	0.004	0.034***	0.034***	0.029***
	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)
Observations	103,782	103,782	35,350	101,020	109,373	223,499
Adj. R^2	0.333	0.333	0.342	0.685	0.684	0.709
Panel C: Ln CO_2 imports						
Kyoto_m-Kyoto_x	-0.012	-0.048**	0.026	0.088***	0.089***	0.075***
	(0.021)	(0.021)	(0.019)	(0.012)	(0.012)	(0.011)
Observations	103,782	103,782	35,350	101,020	109,373	223,499
Adj. R^2	0.036	0.036	0.089	0.074	0.075	0.074

Note: Regressions on pooled sectoral data. (3) Long differences-in-differences (DiD): Estimation on pre- and post-treatment averages. The pre- and post-period are 1995-1996 and 1997-2000. In (4) and (5) the sample is restricted to trade between Kyoto and non-Kyoto countries and high-income non-Kyoto with non-Kyoto countries, i.e. the USA or Australia, Israel, Korea and the USA, respectively. (6) The USA is added to the set of Kyoto ratifiers (fictitious ratification date 2002). Heteroskedasticity-robust standard errors (in brackets). In FE estimation standard errors are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

averages, again for ratification in 1997. We find no evidence for a positive and significant correlation of the outcome variables with placebo treatment. In 1998, we even find a negative and significant relationship for the Kyoto variable. This would suggest Kyoto countries had a trend of declining imports and virtual CO_2 imports in the pre-treatment period. An alternative explanation might be the Asian and the Russian financial crises in 1998. Overall, there is no compelling evidence supporting different pre-treatment trends in imports, CO_2 intensity or CO_2 imports prior to Kyoto ratification.

The second placebo test is based on the idea of comparing the outcomes of developed Kyoto countries with developed non-Kyoto countries like the USA only. To this end, we drop all observations where both trade partners are non-Kyoto countries unless one of them is the USA. Additionally we drop all observations where both trade partners are Kyoto countries.[42] It implies we compare a Kyoto country's imports from non-Kyoto countries with the US imports from non-Kyoto countries and similarly a Kyoto country's exports to non-Kyoto countries with the US exports to non-Kyoto countries. Column (4) in Table 4.6 provides the results. The estimated coefficients are broadly in line with the benchmark. Results are again very similar if we include all developed non-Kyoto countries – i.e. Australia[43], Israel, South Korea and the United States – in the comparison group, see column (5).

In a similar vein, if instead we include the USA in the set of Kyoto countries (with a hypothetical ratification in 2002), the estimated coefficients get somewhat smaller in terms of magnitude compared to the benchmark and are still statistically significant (see Table 4.6 column (6)). If the alternative interpretation of developed versus developing countries were true, this change in the set of Kyoto ratifiers should increase the point estimates. However, this is not the case.

In summary, the first placebo tests on fictitious treatment dates reveal no Kyoto treatment effect. The identifying assumption of similar trends without Kyoto seems to hold. So with our estimates, we do not merely pick up sectoral trend changes in the Kyoto ratifiers. And the second placebo test of comparing Kyoto countries with other developed non-Kyoto ratifiers only shows that the Kyoto effects are not common to all high income countries.

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[42] We obtain similar results if we do not drop these observations.

[43] Australia ratified the Kyoto Protocol in 2007 but is only in the sample until 2006 (see Table D.1). So it is coded as non-Kyoto country for the whole time period.

4.5.4 Pooled regressions: further robustness checks

Table 4.7 presents further sensitivity analyses to Table 4.3. Panel A varies the way the CO_2 content of bilateral imports is measured. Panel B uses an alternative sample as well as reporting results for aggregated data. In Panel (A) and (B) the econometric models are as in the benchmark. In contrast, Panel C varies the estimation method. To save space, the table only reports effects on imports, carbon terms of trade and carbon content of imports[44] and suppresses all coefficients other than those on the Kyoto variable.[45]

Alternative measures of CO_2 imports. Columns (A1) to (A3) in Panel A use the MRIO measure of the carbon content of bilateral imports rather than the SRIO one. Comparing the results to the corresponding columns (2), (3) and (4) in Table 4.3, using the MRIO method to compute the carbon content of trade yields almost identical results. Results do not appear to be sensitive to whether upstream emissions in third countries are taken into account or not.

Column (A4) in Panel A uses input-output tables and emission coefficients from the year 2000 for the SRIO computation of the CO_2 content of trade, making no use of the updates for following or past years. Thereby, energy-saving technical progress remains unaccounted for. Comparing results with those obtained in Table 4.3, differential commitment increases CO_2 imports by 5%; this is only about 65% of the effect obtained when technical change is allowed for. Alternatively, if only the input-output table is fixed in the year 2000 this does not affect the estimated coefficient, see column (A5) for the MRIO case. So changes in the economies' supply structure play no role for Kyoto's effect on embodied carbon.

Alternative sample. Does the massive increase in Chinese exports following its accession to the WTO in 2001 drive the findings in Table 4.3? Or are the results driven by former communist countries from Eastern Europe, which have quite substantially reduced their CO_2 emissions due to the rapid modernization of their industries in the aftermath of 1989-1992? Columns (B1) to (B3) of Panel B report regression output for a reduced sample from which China and transition countries have been dropped.[46] The overall picture remains intact:

[44] The effect on carbon intensity is the difference between the coefficient on total CO_2 imports and the one on imports.

[45] Details are found in Tables D.10 to D.12 of Appendix D.

[46] Excluding only China or only the transition countries does not lead to different results.

Table 4.7: Regressions on Pooled Data, Robustness Checks

Panel A: Alternative measures of CO_2 imports					
	(A1)	(A2)	(A3)	(A4)	(A5)
Measure:		MRIO		I-O, e fixed	I-O fixed
Dep. var.:	Ln η_x	Ln CTT	Ln E_{mx}	Ln E_{mx}	Ln E_{mx}
Kyoto$_m$-Kyoto$_x$	0.023***	-0.031***	0.072***	0.050***	0.073***
	(0.002)	(0.004)	(0.011)	(0.011)	(0.011)
Observations	223,460	215,879	223,460	223,384	223,499
Adj. R^2	0.708	0.034	0.085	0.206	0.064

Panel B: Alternative sample and aggregate data						
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)
Data:	w/o China & transition countries[a]			Aggregate data		
Dep. var.:	Ln Q_{mx}	Ln CTT	Ln E_{mx}	Ln Q_{mx}	Ln CTT	Ln E_{mx}
Kyoto$_m$-Kyoto$_x$	0.048***	0.032***	0.033***	0.056***	-0.172***	0.143***
	(0.012)	(0.006)	(0.013)	(0.019)	(0.020)	(0.022)
Observations	136,392	134,046	136,392	19,623	19,588	19,623
Adj. R^2	0.136	0.043	0.058	0.604	0.034	0.233

Panel C: Alternative estimators						
	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)
Estimation	Pre- and post treatment averages[b]			Poisson pseudo maximum likelihood		
Dep. var.:	Ln Q_{mx}	Ln CTT	Ln E_{mx}	$Q_{mx} > 0$	$Q_{mx} \geq 0$	$E_{mx} \geq 0$
Kyoto$_m$-Kyoto$_x$	0.054**	-0.077***	0.103***	-0.014	-0.013	0.050*
	(0.022)	(0.009)	(0.023)	(0.017)	(0.017)	(0.027)
Observations	36,269	35,612	36,269	223,387	234,278	234,278
Adj. R^2	0.333	0.051	0.112			
Log likelihood				-32,066.300	-32,093.527	-4,902.334

Note: Country-pair(-sector) fixed effects estimations. All regressions include full set of country × year effects, trade policy controls (joint FTA, WTO, and EU membership) and constant. Heteroskedasticity-robust standard errors (in brackets) corrected for clustering within country-pair(-sector); ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively.
[a] Transition countries are CZE, EST, HUN, POL, ROU, RUS, SVN, SVK.
[b] Estimation on pre- and post-treatment averages. The pre- and post-period are 1995-2000 and 2004-2007.

The scale effect is again about 5%. Differential Kyoto commitment leads to an increase in the volume of carbon imports by about 3% (column B3). In this specific sample, however, differential commitment lowers the carbon intensity of imports by about 2% (the technique effect (not shown), which is the difference between the coefficient on overall CO_2 imports and imports ($0.03 - 0.05 = -0.02$), is strongly positive, too.) This finding is supported by a positive effect on the CTT. The fact that CO_2 intensity falls may be a sign that China and transition countries have increased their export sales in labor-intensive and thus relatively carbon-free sectors.

Aggregated data. Columns (B4) to (B6) in Panel B report results for data aggregated over sectors. Compared to the benchmark in Table 4.3, the sign patterns of the Kyoto coefficients are unchanged. The scale effect is again roughly 5%. However, estimated coefficients on the carbon terms of trade and virtual carbon imports (and thus also CO_2 intensity) are considerably larger. This may be due to the presence of aggregation bias. In the absence of aggregation bias, the larger aggregate effects might be evidence for a *between*-sector composition effect, i.e. a change in the sectoral composition of trade.[47] Kyoto countries shift out of dirty goods sectors into relatively clean sectors. This alternative interpretation would suggest additional leakage from a change in the sectoral composition of trade.

Long differences-in-differences estimator. Bertrand et al. (2004) argue that estimated treatment effects could be spurious if both the treatment variable and the dependent variable have a common trend. This might be an issue in our framework: Since no Kyoto country has so far withdrawn from the Kyoto Protocol, once the Kyoto dummy switches to one it does not change thereafter. Bertrand et al. propose a long differences-in-differences estimator, i.e. estimating the treatment effect with pre- and post-treatment averages, to cure the problem. Most Kyoto countries ratified the Protocol between 2001 and 2003, so we choose this as our treatment window. The pre-treatment period is 1997-2000 and the post-treatment period is 2004-2007, see also Figure 4.1.[48] The obtained estimates can be interpreted as long-run effects of Kyoto commitment. Columns (C1) to (C3) in Panel C show results for the long differences-in-differences estimator on the pooled sectoral data. Our benchmark results are supported. Kyoto commitment of the importer but not the exporter is associated with 5% more imports and 10% higher carbon imports, both statistically significant at the 1% level. So the earlier results do not stem from spurious correlation.

Dealing with zero trade flows Finally, a concern raised with log-linear gravity specifications is that by taking logs zero trade flows are dropped from the sample. In standard trade contexts, the arising sample selection bias can be sizable. For example Helpman et al. (2008) document that about half of the country pairs do not trade with each other. Our

[47] The technique effect in pooled sectoral estimations results from within-sector variation. It could be a true change in the CO_2 intensity of production. Or it could partly be a *within*-sector composition effect since our sectoral disaggregation is rough.

[48] Russia ratified in 2004 and is treated as a Kyoto country, Australia ratified in late 2007 and is put in the control group.

sample of 40 countries mainly consists of large and important trading countries. There are only 11,946 zero trade flows in 235,833 countrypair-sector-year observations.[49] So, with around 5% zero import flows the sample selection bias should be small. To check this, we run Poisson pseudo maximum likelihood (PPML) estimations, as proposed by Santos Silva and Tenreyro (2006).[50] The inclusion of many dummies into the PPML routine is known to lead to convergence problems. So we use a country-pair-sector fixed effects specification[51] with logs of GDPs and multilateral resistance terms instead, compare column (3) of Table 4.4. In column (C5) of Panel C, the point estimate of the Kyoto commitment variable in the virtual CO_2 import equation is identical to the benchmark, although only statistically significant at the 10% level. The Kyoto coefficient in the import equation is no longer statistically different from zero. However, note that the PPML results with and without zeros are virtually identical, see columns (C4) and (C5). So the different coefficient estimates for imports is not due to zeros in the sample but rather due to the non-linear estimation procedure.

4.5.5 Differential commitment sector by sector

In the next step, we run regression (4.14) sector-by-sector.[52] Table 4.8 reports the results for FE and long differences-in-differences estimation. Sectors are sorted in descending order of Kyoto's effect on virtual CO_2 flows. Differential commitment has strong effects on total carbon imports in 8 out of 12 sectors (basic metals, paper and pulp, electricity, transport equipment, machinery, other non-metallic minerals, electricity, non-specified industries and chemicals). The measured coefficient $\hat{\kappa}_E$ ranges between 8 and 20%. It is highest in carbon-intensive industries (such as basic metals or paper and pulp) and/or industries in which the degree of product differentiation is low.

The reasons for increased carbon imports vary across sectors. Leakage in the basic metals, non-metallic mineral products, transport equipment and machinery sector is solely due to Kyoto countries' significant increases in imports. From our theory we would expect larger scale effects for sectors with a low σ, i.e. a high elasticity of substitution and hence rather

[49] Note that we have an unbalanced panel and consequently missings in the sample due to emission coefficient data availability issues. To base the import and CO_2 import regressions on the same sample, potentially available (positive and zero) import flows are dropped for the country-year combinations listed in Table D.1. This leaves us with 235,833 instead of 243,360 balanced panel observations.

[50] In Appendix D Tables D.8 and D.9, we also provide results on the hands-on approach of adding an increment of one before taking logs and a Heckman selection model as applied in Helpman et al. (2008).

[51] The corresponding STATA routine xtpqml is due to Simcoe (2007).

[52] Full regression output is found in Tables D.13 to D.16 in Appendix D

Table 4.8: Sector-by-sector regressions: differential commitment

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variable:	Ln imports		Ln CO_2 intensity		Ln CO_2 imports	
Method:	FE	long FE	FE	long FE	FE	long FE
(3) Basic metals	0.20***	0.21**	-0.00	0.01	0.20***	0.21**
	(0.04)	(0.08)	(0.01)	(0.01)	(0.04)	(0.08)
(9) Paper, paper products, pulp and printing	0.15***	0.16**	0.02***	0.04***	0.17***	0.19***
	(0.04)	(0.07)	(0.01)	(0.01)	(0.04)	(0.07)
(6) Transport equipment	0.15***	0.18**	0.01	0.01	0.16***	0.21**
	(0.04)	(0.08)	(0.01)	(0.02)	(0.04)	(0.09)
(7) Machinery	0.13***	0.10**	0.01	0.00	0.15***	0.11**
	(0.02)	(0.05)	(0.01)	(0.02)	(0.03)	(0.05)
(5) Other non-metallic mineral products	0.14***	0.17***	-0.00	0.00	0.14***	0.18**
	(0.03)	(0.07)	(0.01)	(0.02)	(0.03)	(0.07)
(2) Electricity, energy, mining and quarrying	0.08	0.14	0.05***	0.10***	0.13**	0.24**
	(0.06)	(0.12)	(0.01)	(0.02)	(0.06)	(0.12)
(12) Non-specified industries	-0.01	-0.02	0.09***	0.11***	0.09***	0.10**
	(0.02)	(0.04)	(0.01)	(0.02)	(0.03)	(0.05)
(4) Chemicals and petrochemicals	0.02	0.02	0.06***	0.07***	0.08***	0.09*
	(0.03)	(0.05)	(0.01)	(0.02)	(0.03)	(0.06)
(8) Food products, bever-, ages, tobacco	0.01	0.06	0.01**	0.03**	0.02	0.10
	(0.03)	(0.07)	(0.01)	(0.01)	(0.04)	(0.08)
(1) Agriculture, forestry, fishing	-0.04	-0.02	0.02***	0.06***	-0.02	0.05
	(0.04)	(0.08)	(0.01)	(0.01)	(0.04)	(0.08)
(10) Wood and wood products	-0.10**	-0.15	0.02**	0.05***	-0.08*	-0.09
	(0.05)	(0.09)	(0.01)	(0.02)	(0.05)	(0.09)
(11) Textile and leather	-0.12***	-0.19***	0.02***	0.03*	-0.09***	-0.15**
	(0.03)	(0.06)	(0.01)	(0.02)	(0.03)	(0.06)

Note: Each cell is the result of a separate regression. The explanatory variable listed is differential Kyoto commitment and takes values (-1,0,1). The method of estimation is either fixed effects (within, FE) or long differences-in-differences estimation on pre- and post-treatment averages (long FE). Each regression includes trade policy controls (joint WTO, FTA, and EU membership) and a full set of country×year effects. Heteroskedacticity-robust standard errors (in brackets) are adjusted for within country-pair clustering. ***, ** and * denote statistical significance at the 1%, 5% and the 10% levels, respectively.

homogeneous goods, and/or sectors with a high β, i.e. CO_2 intensity and/or sectors for which transport costs are not that important. And we indeed observe this pattern. We find the largest scale effect for base metals and paper (high σ and high β), transport equipment and machinery (low ad valorem trade costs) and non-metallic mineral products (high β). Less carbon-intensive products have no scale effect like food products or agriculture.[53] On the contrary, in the agricultural, electricity, chemicals, food products, wood, textiles and leather, and non-specified industries sector the carbon intensity of committed countries' imports rises and gives thus rise to more carbon imports.

Interestingly, there is only one sector (paper and pulp) featuring a positive and significant scale *and* technique effect. For all other sectors, we observe that the adjustment is either along the quantity traded or along the emission intensity.

In conclusion, some sectors seem to be more prone to carbon leakage than others. And only some sectors' competitiveness is affected through Kyoto commitment.

4.6 Conclusions

We have developed a gravity model for carbon embodied in trade. Stricter domestic climate policies reduce domestic emissions but may raise them elsewhere as consumers switch suppliers. This phenomenon – carbon leakage – is equivalent to more emissions embodied in imports and less emissions embodied in exports. Therefore we suggest to test for carbon leakage with a gravity-type equation for CO_2 embodied in trade within a novel data set of bilateral sectoral carbon flows embodied in trade flows. When implementing this test for carbon leakage one has to acknowledge that commitment in the Kyoto Protocol might not be random. The structure of our data allows us to use country-and-time effects to control for self-selection into treatment. Furthermore, it also allows us to control for country-pair specific unobserved heterogeneity in carbon imports and exports.

We show that carbon leakage is empirically relevant. Our within estimations imply that sectoral carbon imports of a committed country from an uncommitted exporter are about 8% higher than if the country had no commitments. The carbon intensity of those imports is about 3% higher. The empirical evidence also hints at technological cleaning-up in Kyoto countries. Sector-by-sector regressions show that some sectors are more prone to carbon leakage than others.

Note that the finding of increased carbon imports of committed importers from non-Kyoto countries is a necessary and sufficient condition for the existence of carbon leakage.

[53] One sector (textiles and leather) displays a negative and significant coefficient. This result is likely spurious and vanishes if we drop Mexico and Australia from the sample.

Nevertheless, we cannot directly compare our estimates with the carbon leakage measures obtained in CGE studies. To this end, we would need an estimate of the average Kyoto country's emission savings due to its climate policy. Due to the problem of self-selection into treatment, this causal relationship is not easily uncovered in an econometric setup. The inclusion of country-and-time effects is no feasible option for this problem since this makes identification of the coefficient of interest impossible. Aichele and Felbermayr (2012) provide first results using instrumental variables estimates on country-level data. Taking their estimate of 7% emission savings at face value, we could engage in a little back-of-the-envelope style calculation. Take the year 2007. On the one hand, in 2007 an average Kyoto country has 7% emission reductions and an average of 277 Mt of domestic CO_2 emissions. This implies it saved roughly 21 Mt of CO_2 with its climate policy efforts. On the other hand, it has increased its virtual CO_2 imports by 8% and in 2007 imports on average 109 Mt of CO_2. This implies 8 Mt of additional CO_2 imports due to Kyoto commitment and consequently 8 Mt of additional emissions elsewhere. The implied leakage rate is $\frac{8}{21} \approx 40\%$ – a sizeable number. Due to the back-of-the-envelope calculation, this number should be taken with a pinch of salt. Nevertheless, the message is clear. Carbon leakage is not negligible. In a globalized world in which countries trade a lot, carbon leakage is a real threat to the effectiveness of unilateral climate policy.

A limitation to our exercise is that due to data constraints we cannot take emissions from international transportation into account. Hence, our carbon leakage estimate could be interpreted as lower bound. International transportation (a carbon-intensive business) will add additional emissions when goods are shipped more and more between countries.

Nevertheless, our results suggest that the issue of carbon leakage is a serious challenge to international climate saving programs. Since a multilateral agreement that commits all countries to binding emission targets does not exist and looks increasingly unlikely, the first-best policy to combat climate change, namely a world-wide cap on emissions, is not feasible. Policymakers in the European Union and the U.S. have called for border tax adjustments to tackle the problem. Establishing the empirical relevance of carbon leakage as a result of unilateral climate policy, our analysis lends support to these policy positions. Since such taxes pose important informational problems and may be conceived as protectionist, more research into their design is needed.

Before closing, we want to stress that our empirical strategy was geared toward identifying the average effect of unilateral climate policy. Our empirical results cannot straightforwardly be used for the simulation of global CO_2 emissions as a response to climate policy scenarios, e.g., the potential commitment to an emission cap by the U.S., or the counterfactual situation of no global climate policy at all. To that end, one would need to use the estimated elasticities in a structural general equilibrium model. We view this as a challenging but worthwhile avenue for further research.

Chapter 5

Carbon Leakage with Structural Gravity

5.1 Introduction

Countries vary greatly with respect to their willingness to commit to international climate policy efforts. This is partly attributable to domestic political constraints, as for example in North America. It also reflects different risks of exposure to negative effects of global warming. For example small island states are more prone to rising sea levels or extreme weather events. In addition, there is a historical responsibility of industrialized countries for greenhouse gas (GHG) concentrations in the atmosphere.[1] Developing countries claim the same right for unconstrained growth. Political constraints and equity considerations make partial climate deals, like the Kyoto Protocol and the EU's Emission Trading System, a likely feature of the future climate policy architecture.

However, if only a sub-group of countries increases the costs of GHG emissions, this changes international relative goods prices (terms-of-trade effect). In response, production might shift to countries with lax climate regulation. The possibility to relocate production and engage in international trade can undermine the environmental effectiveness of a partial climate deal. *Carbon leakage* refers to such a situation where a stricter climate policy regulation in one country or region causes higher emissions elsewhere (Felder and Rutherford,

[1] This is referred to as "principle of common but differentiated responsibilities" under the United Nations Framework Convention on Climate Change (UNFCCC). The UNFCCC is an international environmental treaty and constitutes the legal framework for negotiations of GHG emission reductions in so-called "Protocols".

1993).[2] The extent of carbon leakage is typically quantified with a percentage number: the total emission increases in other countries as a share of emission savings in the climate-active region.

In this paper, we want to understand how unilateral climate policy shapes the location of emissions in a globalized world. Guided by an *estimated structural gravity model*, we *quantify the extent of leakage* in counterfactual climate policy scenarios. A large sample is crucial for this exercise because it allows to capture the majority of global production shifts. Hence, we work with the largest possible sample of 103 countries containing all major trading nations. More importantly, the sample does not concentrate on industrialized countries only but also comprises many developing countries with relatively low carbon prices.

Point of departure is the workhorse model in international trade: A new trade theory gravity model (in its form as derived in Anderson and van Wincoop, 2003, with Armington-type preferences). We allow for several production factors: labor, capital, land, and energy. They are combined in a Cobb-Douglas production function to produce a country's variety. Implicit carbon prices, i.e. the implicit price for burning fuels, are crucial to the analysis. Aside from country-specific energy market conditions they also reflect climate policy stringency. Climate policy adds a price premium to costly energy use (Aldy and Pizer, 2011). Whereas climate policy stringency is not observable, implicit carbon prices are. Thus they provide a simple way to calibrate our model to the data.

The model's key parameters are empirically estimated. The model gives rise to a standard gravity equation. Bilateral trade costs are consistently estimated from the gravity equation with importer and exporter fixed effects. As in Bergstrand et al. (2013), these estimated trade costs and observed factor cost shares are then fed back into the structural model to identify the elasticity of substitution.

We conduct two types of policy experiments: partial climate policy initiatives and trade liberalization. The latter scenarios discuss environmental aspects of regional free trade agreement (FTA) formation in the presence of carbon price differentials. The climate policy

[2] There are two additional reasons for carbon leakage aside from international trade. First, since the global climate is a global public good there is an incentive to free-ride on others' emission savings and to relax one's own climate policy efforts in response (see e.g. Carraro and Siniscalco, 1998; Congleton, 2001; Copeland and Taylor, 2005; Elliott et al., 2010). Second, depending on the supply elasticity of energy inputs a reduced energy demand in one part of the world may lower the world market energy price (see, e.g. Sinn, 2008). This can lead to increased energy demand, and thus emissions, in countries which do not have a strict climate policy in place ("supply-side leakage"). In this paper we focus on the leakage channel relating to trade and relative goods prices.

scenarios focus on the emission relocation implied by an increase in the EU's emission allowance price. Briefly, we also study the implications of proposed commitments under a second period of the Kyoto Protocol (Kyoto II) for leakage.[3]

The theoretical possibility of carbon leakage is well documented (see e.g. Copeland and Taylor, 2005). But Di Maria and van der Werf (2008), for example, show that directed technical change weakens carbon leakage. So there exist theoretical arguments diminishing its role.

In this paper, we bring together two strands of literature that assess the importance of leakage. First, a growing body of literature assesses the competitiveness and leakage effects of unilateral climate policy with the help of computable general equilibrium (CGE) models. Results from such models are ambiguous. Resulting leakage rates are typically moderate and lie between 5 and 20% (Felder and Rutherford, 1993; Elliott et al., 2010; Burniaux and Oliveira Martins, 2012), but the leakage rate can even exceed 100% (Babiker, 2005). The models are sensitive to parameter choices (see, e.g., the literature survey in Burniaux and Oliveira Martins, 2012) like elasticity of substitution and bilateral trade costs. These are often not founded on empirical estimates. In our structural gravity approach, we also employ simulation techniques to create counterfactual policy scenarios. Yet in contrast, our model's parameters are structurally linked to econometric estimates; i.e. the model's bilateral trade costs and elasticity of substitution are consistent with the data.

Second, an empirical strand of literature works with international trade data and applies the gravity equation in the context of climate policy.[4] It provides ex-post evidence on actual policy experiments like carbon taxes or the Kyoto Protocol. The empirical evidence presented so far suggests a *direct* trade effect of climate policy (see for example World Bank, 2008). Employing a panel strategy to control for Kyoto's endogeneity, Aichele and Felbermayr (2011a) estimate a Kyoto country increases its imports from non-Kyoto countries due to Kyoto commitment by about 5%. Sato (2011) finds that an electricity price gap of 10 US-$ per MWh reduces exports by 1-2%. The evidence is also consistent with leakage: the carbon content of imports of Kyoto countries from non-Kyoto countries increases by 8% with Kyoto commitment (Aichele and Felbermayr, 2011a). The empirical literature provides average

[3] At the time of writing of this thesis, it was not clear whether the Kyoto Protocol would expire. Due to recent proposals for a second period from 2013-2020, we add a Kyoto II scenario.

[4] This literature is embedded in a broader empirical literature exploring the pollution haven effect of environmental regulation stringency on trade flows with a focus on local pollutants (see Ederington et al., 2005; Levinson and Taylor, 2008, for some examples).

treatment effects of climate policy or energy prices. Country-specific general equilibrium responses to policy changes are absorbed in country or country-and-time fixed effects. So, the empirical estimates cannot offer a *general equilibrium* (GE) quantitative perspective on the leakage problem. General equilibrium changes in GDPs and third country effects like trade diversion are neglected. This paper fills the gap and goes beyond average treatment effects. We resort to structural estimation and simulation techniques to quantify leakage in general equilibrium.

Methodologically, the paper is related to structural gravity applications. In this strand of literature, policy experiments typically deal with the effects of trade costs on trade patterns and welfare in general equilibrium. Several studies investigate the effects of abolishing the Canada-US border (see, for example Anderson and van Wincoop, 2003; Bergstrand et al., 2013). Other studies simulate the gains from trade of trade liberalization (see for example Eaton and Kortum, 2002) or free trade agreement (FTA) formation (Egger et al., 2011b; Egger and Larch, 2011) or deal with the role of trade imbalances for welfare (Dekle et al., 2007). So far, the structural gravity approach has not been applied to the carbon leakage context.

We find that leakage is moderate but non-negligible. An EUA permit price of 15 US-$ allows the EU to fulfill its Kyoto target. EU countries increase their import shares from non-EU countries. The induced emission relocation amounts to 10%. The counterfactual emission increase through production relocation in non-EU countries is heterogeneous and governed by proximity to the EU, country size, and relative carbon prices. Results are robust to the econometric estimation procedure chosen. Not accounting for country-specific heterogeneity in factor use leads to a slight underestimation of the extent of leakage. Emission limitations as proposed for the second period of the Kyoto Protocol lead to 8% of emission relocation. This leakage rate is smaller than in the ETS scenario because with Kyoto II a larger part of the world is constrained.

The rest of the paper proceeds as follows. Section 5.2 describes our model and parameter identification strategy. Section 5.3 presents our empirical estimates of the model's parameters. Section 5.4 shows the results of counterfactual policy experiments.

5.2 Methodology

The gravity equation is the workhorse model of international trade. It explains bilateral trade flows with bilateral trade costs, GDPs and prices. A large model class featuring iceberg trade costs, constant elasticity of substitution preferences, perfect specialization, linear cost functions and one factor of production leads to an isomorphic formulation of the gravity equation and the gains from trade (Eaton and Kortum, 2002; Arkolakis et al., 2012). In a seminal paper, Anderson and van Wincoop (2003) rationalize the gravity equation with Armington (1969)-type preferences. The authors point out that it is important to account for the endogeneity of price levels or *multilateral resistance terms* in a structural gravity model. Krugman (1980)-type gravity models provide an alternative formulation with monopolistic competition and increasing returns to scale. Eaton and Kortum (2002) focus on technology differences between countries in a Ricardian continuum of goods framework. Chaney (2008) and Helpman et al. (2008) deal with firm heterogeneity and the role of zero trade flows. The empirical gravity specification resulting from many of these models (Anderson and van Wincoop, 2003; Eaton and Kortum, 2002; Feenstra, 2004, amongst them) is similar, and thus are trade cost parameter estimates. Even though the gravity equation is structurally similar, the theoretical underpinning of the gravity equation will determine both the magnitudes and the transmission mechanisms of comparative statics effects (Egger et al., 2011b).

We follow the theoretical gravity formulation in Anderson and van Wincoop (2003). With this choice of the theoretical model, we shut down two potential transmission channels of climate policy shocks. Effects on the extensive margin of trade are neglected. We look at aggregate trade flows in 103 countries. Since zero trade flows only make up 10% of our observations, this choice is defendable. In addition, it seems plausible that climate policy affects how much is traded (*intensive margin*) much rather then the decision to trade at all (*extensive margin*). Since their is no role of firm heterogeneity in the Anderson and van Wincoop (2003) model, we rule out effects on firm distribution as well. Climate policy might shut down firms at home while making production and possibly exporting profitable for relatively less efficient (and thus more energy-intensive) firms abroad. This may provide an additional leakage channel. To study these effects, one would require firm level information on trade and emission intensity however. Krugman (1980)-type models are not often used in structural gravity since the number of varieties needs to be calibrated. In conclusion, the

Anderson and van Wincoop (2003) framework provides a good point of departure for our analysis.

The model extension in Section 5.2.1 allows for labor, capital, land and energy as production factors. This is key to disentangle the effects of carbon prices on trade and emissions. Section 5.2.2 describes our strategy to structurally estimate the model's parameters. Section 5.2.3 presents the methodology for evaluating counterfactual scenarios.

5.2.1 Gravity model with energy and conventional production factors

Trade flows. Our model world is populated by $i, j = 1, \ldots, N$ countries. There is one sector of production. Sectoral varieties are differentiated by country of origin and each country produces one such variety. We assume that the representative consumer's preferences display a constant elasticity of substitution (CES) over varieties, with $\sigma > 1$ common across all countries. σ is a key parameter in international trade since it is a crucial driver of trade effects. The assumption of a common σ across all countries is fairly standard in the gravity literature (see for example Anderson and van Wincoop, 2003; Bergstrand et al., 2013; Anderson and Yotov, 2010). The corresponding Frechet-distribution parameter in Eaton and Kortum (2002)-type models or the Pareto parameter in Melitz (2003)-type models is typically assumed common across countries as well.[5] So we follow the literature here.

In reality, industries display heterogeneity in their elasticity of substitution (see, e.g., Broda and Weinstein, 2006). The sectoral effects of climate policy will crucially depend on the degree of product differentiation. A cost shock will translate into higher relocation effects the more substitutable/homogeneous a sector's varieties are. With a one sector model, we cannot capture this. However, with several sectors model calibration and simulation is computationally more involved. For this reason, there are only few applications of multi-sector structural gravity models. So in a first attempt we abstract from sectoral heterogeneity in relocation effects; accepting a potential aggregation bias. Nonetheless, including a sectoral structure is an important avenue for future research.

[5] In a recent paper, Feenstra et al. (2012) work with a a nested CES structure and distinguish two elasticities – a macro elasticity between domestic and foreign varieties on the upper tier and a micro elasticity between foreign varieties of different countries of origin. The micro elasticity which is linked to bilateral trade flows is still common across all foreign countries. To identify the macro elasticity as in Feenstra et al. (2012), we would need information on domestic unit values which we do not have. So we stick to the standard formulation.

International trade of sectoral varieties is costly. $\tau_{ij} \geq 1$ units have to be shipped from country i to country j for one unit to arrive, i.e. a fraction of the quantity shipped between countries melts away (iceberg trade costs).[6] Country i charges the ex-factory price p_i. Then, the price of goods from i shipped to j is $p_{ij} = \tau_{ij} p_i$. Let b_i be the usual positive parameter. So the consumer in country j maximizes her utility U_j over the consumed quantity c_{ij} of i's variety

$$\max U_j = \left[\sum_{i=1}^{N} b_i^{\frac{1-\sigma}{\sigma}} c_{ij}^{\frac{\sigma-1}{\sigma}} \right]^{\frac{\sigma}{\sigma-1}}$$

subject to the budget constraint $\sum_{i=1}^{N} p_{ij} c_{ij} = Y_j$. National income Y_j comprises factor income (such as wage income, interest payments and land rents).

From the utility maximization problem, the value of bilateral trade flows X_{ij} from country i to j is determined as

$$X_{ij} = p_{ij} c_{ij} = \left(\frac{b_i p_i \tau_{ij}}{P_j} \right)^{1-\sigma} Y_j. \tag{5.1}$$

As usual, $P_j = \left[\sum_{i=1}^{N} (b_i p_i \tau_{ij})^{1-\sigma} \right]^{\frac{1}{1-\sigma}}$ is the ideal price index.

Gravity equation. Goods market clearing requires that a country's supply of its variety equals the quantity demanded (inclusive trade costs):

$$Y_i = \sum_{j=1}^{N} X_{ij} = (b_i p_i)^{1-\sigma} \sum_{j=1}^{N} \left(\frac{\tau_{ij}}{P_j} \right)^{1-\sigma} Y_j. \tag{5.2}$$

Define $Y^w \equiv \sum_i Y_i$ as world GDP and $\theta_i \equiv \frac{Y_i}{Y^w}$ as country i's GDP share. Solving (5.2) for equilibrium scaled prices $(b_i p_i)^{1-\sigma}$ and plugging into (5.1) gives the gravity equation

$$X_{ij} = \frac{Y_i Y_j}{Y^w} \left(\frac{\tau_{ij}}{\Pi_i P_j} \right)^{1-\sigma}, \tag{5.3}$$

where

$$\Pi_i \equiv \left[\sum_{j=1}^{N} \left(\frac{\tau_{ij}}{P_j} \right)^{1-\sigma} \theta_j \right]^{\frac{1}{1-\sigma}}. \tag{5.4}$$

[6] In reality, trade costs differ between industries. Some industries are "footloose" because their trade costs are low, while others like cement, for example, have relatively high transportation costs. Due to the lack of a sectoral structure we abstract from this here.

Plugging equilibrium scaled prices into the ideal price index gives

$$P_j = \left[\sum_{i=1}^{N} \left(\frac{\tau_{ij}}{\Pi_i} \right)^{1-\sigma} \theta_i \right]^{\frac{1}{1-\sigma}} . \tag{5.5}$$

The system of equations (5.3)-(5.5) corresponds to the one in Anderson and van Wincoop (2003). Π_i and P_j are called outward and inward multilateral resistance terms or sellers' and buyers' incidence, respectively, and measure a country's trade barriers with all other countries. With symmetric trade costs $\tau_{ij} = \tau_{ji}$ outward and inward multilateral resistance are equivalent, $\Pi_i = P_i$, and the gravity equation simplifies to

$$X_{ij} = \frac{Y_i Y_j}{Y^w} \left(\frac{\tau_{ij}}{P_i P_j} \right)^{1-\sigma} , \tag{5.6}$$

with multilateral resistance terms as implicit solution to

$$P_j = \left[\sum_{i=1}^{N} \left(\frac{\tau_{ij}}{P_i} \right)^{1-\sigma} \theta_i \right]^{\frac{1}{1-\sigma}} . \tag{5.7}$$

This derivation assumes trade is multilaterally balanced; an assumption violated in the data. Trade imbalances could be accommodated in the model by adjusting countries' expenditure levels with nominal transfers as observed in the data (see, e.g., Dekle et al., 2007; Alvarez and Lucas, 2007). Since there is no theory of trade imbalances in the gravity framework we would keep nominal transfers in our application constant. In this case the choice of numeraire matters (Ossa, 2011b). Changes in nominal prices have implications for real transfers.[7] Due to these complications, we abstract from trade imbalances in our analysis.

The gravity equation in (5.3) implicitly considers countries' endowment and energy price differences via their effect on GDPs. In the following, we will lend the Anderson and van Wincoop (2003) framework more structure by explicitly modeling the production structure. This allows to investigate the effect of changes in implicit carbon prices for trade flows, emissions and leakage.

[7] In his structural gravity application on trade wars, Ossa (2011b) therefore purges the actual data from trade imbalances before doing counterfactual analysis. But this implies a policy experiment evaluation against another hypothetical counterfactual.

Production. The representative firm in country i combines labor, capital, land and fossil energy to produce the output quantity q_i.[8] Burning fossil energy causes CO_2 emissions, i.e. emissions and energy use are directly related via chemical processes. For simplicity, we assume a one-to-one relationship. That implies we can think of emissions as an input instead of a side output; a fairly standard modeling assumption in the trade and environment context.[9] It will be convenient to model the production function directly with emissions instead of energy. In the data, we observe that factor cost shares add up to one on average (see Section 5.3.1). So for simplicity we assume a constant returns to scale Cobb-Douglas production function. Given our large sample of 103 countries, technologies are likely to differ across countries. To incorporate this into the model, we introduce productivity differences with a technology shifting parameter A_i. The higher A_i the more productive a country is. Let V_{if} be country i's endowment with factor $f \in$ (labor, capital, land) and E_i its emission level. A country's output level follows

$$q_i = A_i \; {E_i}^{\beta_i} \prod_f {V_{if}}^{\alpha_{if}} \,, \tag{5.8}$$

where β_i is the country-specific cost share of emissions (as well as energy) and α_{if} is the country-specific cost share of factor f.[10] Constant returns to scale imply $\beta_i + \sum_f \alpha_{if} = 1$.

Let e_i denote a country's implicit carbon price and w_{if} denote its price of factor f. With perfect competition, the ex-factory price p_i is equal to the minimum unit costs dual to (5.8):

$$p_i = \frac{1}{A_i} \left(\frac{e_i}{\beta_i} \right)^{\beta_i} \prod_f \left(\frac{w_{if}}{\alpha_{if}} \right)^{\alpha_{if}} . \tag{5.9}$$

With Shepard's lemma, the factor market clearing conditions $\alpha_{if} \frac{p_i}{w_{if}} q_i = V_{if}$ pin down equilibrium factor prices as

$$w_{if} = \frac{\alpha_{if} Y_i}{V_{if}} \qquad \forall f. \tag{5.10}$$

A constant fraction of nominal GDP is spent on labor, capital and land income.

[8] Even though we model labor, capital, land and energy as input factors only, the formulation of the production function is rather general. It is straightforward to extend the model to include more input factors that are in fixed supply.

[9] See for example Copeland and Taylor (2003). Typically, the modeling of emissions as an input factor is motivated with an abatement technology that uses up part of the output. Since we observe an implicit CO_2 price in the data and will alter it in counterfactual scenarios, we choose to model energy as a direct input instead.

[10] We use country-specific factor cost shares in the model because we observe this type of heterogeneity in our data, see Section 5.3.1.

In our data, we observe substantial variation in energy and implicit carbon prices across countries, see the data summary in Section 5.3.1. A country's implicit carbon price captures country-specific energy market conditions like fuel endowments and energy mix. But it also reflects climate policy. Climate policy – be it a carbon tax, an emission cap-and-trade system or a feed-in tariff – constitutes a premium on energy prices (see, e.g., Aldy and Pizer, 2011). It differs across countries and reflects the perceived risk of exposure to adverse effects from global warming and willingness to contribute to a global public good. It implies that, in the end, the national government sets a country's carbon price.

These observations motivate our stylized energy market model. Each country i is endowed with fossil fuel in the ground which is owned by the government, can be extracted but is not traded internationally. For simplicity we normalize extraction costs to zero. Fuel is elastically available at the prevailing carbon price set by the government. Fuel rents (inclusive carbon taxes) are rebated in a lump-sum fashion to the representative consumer and are thus part of national income.[11] Since energy and carbon prices are directly observable – while stringency of climate policy is not – this provides a simple way to calibrate the benchmark model and experiment with carbon taxes in counterfactual scenarios.

Alternatively, energy could be supplied on a world market at a given world price. Country-specific climate policy drives a wedge between the country's and the world energy price. This leads to a similar formulation for implicit carbon prices. If the fuel world market price is normalized to zero, the outcome on national incomes is identical. Else resource ownership matters for national income because the non-tax part of fuel rents is transfered to resource-owning countries. With our stylized model, we abstract from these rent transfers.

This modeling of climate policy has several implications. First, leakage through free-riding is ruled out because there is no strategic component to climate policy. Second, supply-side leakage via the world energy price is not possible either. This would require a (rather) inelastic energy supply. On the contrary, in our model the energy supply reacts to demand changes only. Consequently, climate policy affects emissions through the production pattern and trade. This is the focus of the present paper.

Similar to the derivation of other factors' unit requirements, Shepard's lemma gives the unit emission intensity $\eta_i = \beta_i p_i / e_i$. So a country's emission level is $E_i = \frac{\beta_i Y_i}{e_i}$. In consequence,

[11] This does not imply a government can increase national income by setting higher carbon taxes. This will reduce energy use, fuel rents and diminish other factor prices in general equilibrium.

energy usage – and thus emissions – react to the carbon price set by the government and the overall level of production. The latter is driven by technology, trade and GE effects.

With this production structure, we can rewrite trade flows from (5.1) using (5.9) and (5.10) as

$$X_{ij} = \frac{\left[\frac{b_i}{A_i}\left(\frac{e_i}{\beta_i}\right)^{\beta_i}\prod_f\left(\frac{Y_i}{V_{if}}\right)^{\alpha_{if}}\right]^{1-\sigma}\tau_{ij}^{1-\sigma}}{\sum_{k=1}^{N}\left[\frac{b_k}{A_k}\left(\frac{e_k}{\beta_k}\right)^{\beta_k}\prod_f\left(\frac{Y_k}{V_{kf}}\right)^{\alpha_{kf}}\right]^{1-\sigma}\tau_{kj}^{1-\sigma}}Y_j, \tag{5.11}$$

subject to the goods market clearing constraint (5.2). X_{ij}, Y_i and $\frac{Y_i}{V_{if}}$ are endogenously determined in the model.

The elasticity of substitution σ is a crucial model parameter. It governs how equilibrium prices and trade react to carbon price or trade cost shocks. In the next subsection, we show how (5.11) allows us to identify the parameter σ empirically.

5.2.2 Identifying the model's parameters

In this section, we describe the methodology to identify bilateral trade freeness $\tau_{ij}^{1-\sigma}$, the elasticity of substitution σ and the factor cost shares β_i and α_{if} empirically. With a constant returns to scale Cobb-Douglas production function $\beta_i = \frac{e_i E_i}{Y_i}$ and $\alpha_{if} = \frac{w_{if} V_{if}}{Y_i}$. Labor, capital and factor income as well as emissions and implicit CO_2 prices are given in GTAP8. So a country's factor cost shares are directly observable in the data. Country-specific factor cost shares are the default. In a robustness check, we use average factor cost shares in the production function. This will give us an idea how important factor cost share differences are for the extent of emission relocation. To get average estimates $\hat{\beta}$ and $\hat{\alpha}_f$ we run simple regressions of factor incomes on GDPs.

Typically, the gravity literature proceeds by estimating equation (5.6) or (5.11) in log-linearized form. Bilateral trade costs τ_{ij} are not observable. They are proxied by bilateral distance d_{ij} and a vector of dummy variables for other observables $\mathbf{Z}_{ij}$ like contiguity, common language, and bilateral free trade agreements. As in Anderson and van Wincoop (2003) and many other gravity applications, the functional relationship is assumed to be $\tau_{ij}^{1-\sigma} = \mathrm{d}_{ij}^{(1-\sigma)\varrho} e^{(1-\sigma)\delta\mathbf{Z}_{ij}}$. Countries' price levels are endogenous. To avoid omitted variables bias due to the non-linear multilateral resistance terms, equation (5.11) is estimated with importer and exporter fixed effects. From these considerations, the gravity equation in estimable form follows as

$$\ln X_{ij} = a + (1-\sigma)\varrho \ln d_{ij} + (1-\sigma)\delta \mathbf{Z}_{ij} + \nu_i + \mu_j + \epsilon_{ij}, \tag{5.12}$$

where $a \equiv -\ln Y^w$, $\nu_i \equiv (1-\sigma)\ln\left[\frac{b_i}{A_i}\left(\frac{e_i}{\beta_i}\right)^{\beta_i}\prod_f\left(\frac{Y_i}{V_{if}}\right)^{\alpha_{if}}\right]$ is the exporter fixed effect, $\mu_j \equiv \ln\frac{Y_j}{\sum_{k=1}^N\left[\frac{b_k}{A_k}\left(\frac{e_k}{\beta_k}\right)^{\beta_k}\prod_f\left(\frac{Y_k}{V_{kf}}\right)^{\alpha_{kf}}\right]^{1-\sigma}\tau_{kj}^{1-\sigma}}$ is the importer fixed effect and ϵ_{ij} is an i.i.d. measurement error term.

General equilibrium effects are absorbed by fixed effects. In other words, the gravity parameter estimates could arise from a large class of models (for example Anderson and van Wincoop, 2003; Eaton and Kortum, 2002; Feenstra, 2004). They provide general and GE-consistent estimates for trade cost parameters $\widehat{\tau_{ij}^{1-\sigma}} = \mathrm{d}_{ij}^{\widehat{(1-\sigma)\varrho}} e^{\widehat{(1-\sigma)\delta}\mathbf{Z}_{ij}}$.

In the presence of heteroskedasticity, OLS leads to biased estimates of trade cost elasticity parameters in a log-linearized gravity equation (Santos Silva and Tenreyro, 2006). Poisson pseudo maximum likelihood (PPML) estimation is suggested to circumvent the problem. This method also cures the problem of zero trade flows and a possible sample selection bias due to log-linearization.[12] So we apply both OLS and PPML estimation. After comparing the R^2 and the model fit of predicted with actual data, we choose PPML as default method and provide results from OLS estimation in a sensitivity check.

Theoretically, σ could be estimated with energy price and wage data. But due to the importer and exporter fixed effects, the impact of country-specific variables cannot be identified in (5.12). Consequently, σ cannot be estimated directly with this empirical specification. However, Bergstrand et al. (2013) provide a methodology to isolate σ by using (5.11). The first step involves dividing X_{ij} by the trade flow of a reference country m to the same importer j to get rid of j-specific unobservables:

$$\frac{X_{ij}}{X_{mj}} = \frac{\left[\frac{b_i}{A_i}\left(\frac{e_i}{\beta_i}\right)^{\beta_i}\prod_f\left(\frac{Y_i}{V_{if}}\right)^{\alpha_{if}}\right]^{1-\sigma}\tau_{ij}^{1-\sigma}}{\left[\frac{b_m}{A_m}\left(\frac{e_m}{\beta_m}\right)^{\beta_m}\prod_f\left(\frac{Y_m}{V_{mf}}\right)^{\alpha_{mf}}\right]^{1-\sigma}\tau_{mj}^{1-\sigma}}. \tag{5.13}$$

We can use the estimated gravity parameters from (5.12) to predict $\widehat{\tau_{ij}^{1-\sigma}}$ and $\widehat{\tau_{mj}^{1-\sigma}}$, respectively. Using these predicted trade costs, we can use the model structure (5.3)-(5.5) to predict $\hat{X}_{ij}$ and $\hat{X}_{mj}$, respectively. Assuming taste parameters are identical between country-specific

[12] In our sample, we observe zero trade flows in about 10% of the observations.

varieties, $b_i = b_m$, we can solve (5.13) for σ:

$$\hat{\sigma} = 1 - \ln\left[\frac{\hat{X}_{ij}\widehat{\tau_{mj}^{1-\sigma}}}{\hat{X}_{mj}\widehat{\tau_{ij}^{1-\sigma}}}\right] / \ln\left[\frac{A_m}{A_i}\frac{(e_i/\beta_i)^{\beta_i}}{(e_m/\beta_m)^{\beta_m}}\prod_f \frac{(Y_i/V_{if})^{\alpha_{if}}}{(Y_m/V_{mf})^{\alpha_{mf}}}\right], \tag{5.14}$$

where Y_i and V_{if} are observed in the data and A_i is an estimate of total factor productivity. Using all combinations i, j and $m\,(m \neq i)$ gives $N^2(N-1)$ estimates for σ. In our sample, the distribution of elasticities of substitution is skewed to the right. Therefore, we follow Bergstrand et al. (2013) and use the median value as summary statistic $\hat{\sigma}$.[13] Standard errors for $\hat{\sigma}$ are obtained from bootstrapping.[14]

5.2.3 Counterfactual analysis

With the observed parameters β_i and α_{if} and the estimated parameters $\widehat{\tau_{ij}^{1-\sigma}}$ and $\hat{\sigma}$, we can create counterfactual worlds. In this paper, we are interested in two types of policy experiments: climate policy and trade liberalization scenarios. First, we are interested in the effect of a stricter unilateral climate policy on trade flows and emissions with the ultimate aim to assess the extent of carbon leakage. We can manipulate implicit carbon prices e_i to simulate this type of policy experiment. Second, we study the effect of trade liberalization – say for example an FTA between the EU and the USA or the EU and China – when carbon prices differ across countries. This provides a perspective on environmental aspects of FTA formation. To simulate this type of policy experiment, we can manipulate $\mathbf{Z}_{ij}$ to generate counterfactual trade costs.

Let superscript c denote a variable's counterfactual value. First, we reformulate the goods market clearing condition (5.2) using (5.1) and dividing both sides by $Y^{w,c}$

$$\theta_i^c - \psi_i^c \sum_{j=1}^{N} \widehat{\tau_{ij}^{1-\sigma}}^{\,c} \psi_j^c = 0, \tag{5.15}$$

[13] We drop all estimates of $\hat{\sigma}_{imj} < 1$ since they would imply higher trade costs increase trade.

[14] See for example Anderson and Yotov (2010) for bootstrapping in a similar context. First, we generate 250 bootstrapped gravity parameter estimates which are then used to calculate 250 sets of imj-specific elasticities of substitution and the according median $\hat{\sigma}_r$ for the rth replication. The standard error follows as $\hat{\text{se}}_\sigma = \sqrt{\frac{\sum_{r=1}^{250}(\hat{\sigma}_r - \hat{\sigma})^2}{250}}$.

where $\psi_i^c \equiv (b_i p_i^c)^{1-\sigma}$ are scaled equilibrium prices in the counterfactual. We will be able to describe counterfactual changes in all variables with changes in ψ_i. Counterfactual GDPs depend on equilibrium prices. Hence, counterfactual GDP shares in (5.15) are endogeneous.

Counterfactual GDP. From the production structure (5.8) using $E_i = \frac{\beta_i Y_i}{e_i}$ we can find an expression for GDP that only depends on equilibrium prices, observables, and estimated parameters:

$$Y_i \quad = \quad (p_i A_i)^{\frac{1}{(1-\beta_i)}} \left(\frac{\beta_i}{e_i}\right)^{\frac{\beta_i}{1-\beta_i}} \prod_f V_{if}{}^{\frac{\alpha_{if}}{1-\beta_i}} . \tag{5.16}$$

The counterfactual change in GDPs is

$$\frac{Y_i^c}{Y_i} = \left(\frac{p_i^c}{p_i}\right)^{\frac{1}{(1-\beta_i)}} \left(\frac{e_i}{e_i^c}\right)^{\frac{\beta_i}{1-\beta_i}} = \left(\frac{\psi_i^c}{\psi_i}\right)^{\frac{1}{(1-\beta_i)(1-\hat{\sigma})}} \left(\frac{e_i}{e_i^c}\right)^{\frac{\beta_i}{1-\beta_i}} . \tag{5.17}$$

Solve (5.17) for Y_i^c and divide both sides by $Y^{w,c} \equiv \sum_{k=1}^N Y_k^c$ to find the equilibrium condition for counterfactual GDP shares:

$$\theta_i^c \equiv \frac{Y_i^c}{\sum_{k=1}^N Y_k^c} = \frac{\left(\frac{\psi_i^c}{\psi_i}\right)^{\frac{1}{(1-\beta_i)(1-\hat{\sigma})}} \left(\frac{e_i}{e_i^c}\right)^{\frac{\beta_i}{1-\beta_i}} \theta_i}{\sum_{k=1}^N \left(\frac{\psi_k^c}{\psi_k}\right)^{\frac{1}{(1-\beta_k)(1-\hat{\sigma})}} \left(\frac{e_k}{e_k^c}\right)^{\frac{\beta_k}{1-\beta_k}} \theta_k}. \tag{5.18}$$

In the counterfactual equilibrium, equilibrium prices ψ_i^c and GDP shares θ_i^c for all countries i are simultaneously determined from equations (5.15) and (5.18). After a shock (for example to e_i or $\tau_{ij}^{1-\sigma}$), the system adjusts in a non-trivial way due to the non-linear structure of the problem. Counterfactual GDP, emissions and trade flows follow from counterfactual equilibrium prices and GDP shares. The US price index is chosen as numeraire, i.e. $P_{\text{USA}} = P_{\text{USA}}^c = 1$.[15]

A first interesting outcome variable is a country's real GDP Y_j/P_j. In trade liberalization scenarios we can interpret it as a measure of gains from trade. Counterfactual changes in real GDP depend on how multilateral resistance terms, i.e. countries' multilateral trade barriers,

[15] The solution to the multilateral resistance terms in (5.7) adopts a particular normalization (Anderson and van Wincoop, 2003, footnote 12). Using the US price index as numeraire ensures the same normalization in benchmark and counterfactual scenario.

react to the policy shock. They can be expressed in terms of equilibrium prices ψ_i^c

$$P_j^c = \left[\sum_{i=1}^{N} \widehat{\tau_{ij}^{1-\sigma}}^{c} \psi_i^c\right]^{\frac{1}{1-\hat{\sigma}}}. \tag{5.19}$$

The percentage change in real GDP $\Delta\frac{Y_j}{P_j} \equiv (\frac{Y_j^c/P_j^c}{Y_j/P_j} - 1) \cdot 100$ is given by

$$\Delta\frac{Y_j}{P_j} = \left[\left(\frac{\psi_i^c}{\psi_i}\right)^{\frac{1}{(1-\beta_i)(1-\hat{\sigma})}} \left(\frac{e_i}{e_i^c}\right)^{\frac{\beta_i}{1-\beta_i}} \left[\frac{\sum_{i=1}^{N} \widehat{\tau_{ij}^{1-\sigma}}\psi_i}{\sum_{i=1}^{N} \widehat{\tau_{ij}^{1-\sigma}}^{c} \psi_i^c}\right]^{\frac{1}{1-\hat{\sigma}}} - 1\right] 100. \tag{5.20}$$

Real GDP adjusts because of market size effects and non-trivial adjustments in trade cost weighted equilibrium prices.

Counterfactual emissions. The focus of this paper lies on explaining how emissions shift across the globe in reaction to partial climate policy shocks. Counterfactual changes in emissions (in %) can be expressed as

$$\Delta E_i \equiv \left[\frac{E_i^c}{E_i} - 1\right] 100 = \left[\left(\frac{\psi_i^c}{\psi_i}\right)^{\frac{1}{(1-\beta_i)(1-\hat{\sigma})}} \left(\frac{e_i}{e_i^c}\right)^{\frac{1}{1-\beta_i}} - 1\right] 100. \tag{5.21}$$

From (5.21), we see that emissions are affected in two ways in counterfactual climate policy scenarios. First, all countries with a carbon price shock ($\frac{e_i}{e_i^c} \neq 1$) will directly experience emission changes. A counterfactual increase in a country's carbon price is associated with a proportional emission reduction. Second, there is a general equilibrium effect via changes in equilibrium prices, i.e. a terms-of-trade effect. It affects all countries' emissions, whether they actively changed their climate policy or not. A priori, the direction and magnitude of this effect is not clear and heterogeneous across countries. In the case of trade liberalization scenarios, only the equilibrium price channel remains.

For the counterfactual climate policy scenarios, we would like to express the extent of emission relocation in a single number. A natural choice is the amount of emission increases in other countries divided by the emission savings in the country or region with a carbon price shock. Let $N_\ell \subset N$ denote the subsample of countries pursuing the partial climate policy initiative, for example the European Union. Emission savings in this region are $\sum_{i \in N_\ell} \Delta E_i/100 \cdot E_i$. What about emission changes in other countries? In our model, higher carbon prices shrink the size of the world (in terms of nominal GDP) since they reduce the implicit world "endowment" with emissions. Hence, we probably observe emission savings in all countries after a partial climate policy initiative. But we want to focus on emission changes brought about by production relocation. So we assess the extent of emission relocation against a situation where the counterfactual world GDP is produced without production relocation, i.e. without GDP share changes. In a shrunken world with benchmark GDP shares and energy prices, a country's emissions are given by $\bar{E}_i = \frac{\sum_k Y_k^c}{Y^w} E_i$. From this alternative baseline, unilateral climate policy shifts GDP shares and consequently counterfactual emissions according to

$$\Delta \bar{E}_i \equiv \left[\frac{E_i^c}{\bar{E}_i} - 1 \right] 100 = \left[\frac{\theta_i^c}{\theta_i} \frac{e_i}{e_i^c} - 1 \right] 100. \tag{5.22}$$

In all countries $i \notin N_\ell$, there is no carbon price change. Consequently, emission increases induced by production relocation into these countries are given by $\sum_{i \notin N_\ell} (\frac{\theta_i^c}{\theta_i} - 1)\bar{E}_i$. Summarized in a single number, the emission relocation (in percent) is given by

$$L = \frac{\sum_{i \notin N_\ell} (\frac{\theta_i^c}{\theta_i} - 1)\bar{E}_i}{-\sum_{i \in N_\ell} \left[\left(\frac{\psi_i^c}{\psi_i} \right)^{\frac{1}{(1-\beta_i)(1-\hat{\sigma})}} \left(\frac{e_i}{e_i^c} \right)^{\frac{1}{1-\beta_i}} - 1 \right] E_i} 100. \tag{5.23}$$

This is a measure for terms-of-trade leakage. It is similar in spirit to carbon leakage measures. But in (5.23) only the emission increases in foreign due to market size effects are considered. Typical carbon leakage measures would have total emission changes in other regions in the numerator, i.e. $\sum_{i \notin N_\ell} \left[\left(\frac{\psi_i^c}{\psi_i} \right)^{\frac{1}{(1-\beta_i)(1-\hat{\sigma})}} \left(\frac{e_i}{e_i^c} \right)^{\frac{1}{1-\beta_i}} - 1 \right] E_i$. L does not take into account endogeneous responses in other countries' carbon prices (i.e. supply-side leakage and free-riding) either. The extent of emission relocation can be broken down by country as well.

L depends, amongst others, on market size, equilibrium price, and carbon price changes as well as the elasticity of substitution and emission cost shares. The extent of emission relocation is non-trivial and depends on direct and general equilibrium effects. In particular, it depends on the strength of redistribution of GDP shares across the globe. These will be higher, the higher the elasticity of substitution.

As is standard in the literature, we can also distinguish a scale and a technique effect of climate policy or trade liberalization (see for example Grossman and Krueger, 1993; Copeland and Taylor, 2003).[16] The total effect on emissions can be decomposed into a change of

[16] Note that in a one sector world, there is no effect on the sectoral composition of output (*composition effect*).

emission intensities (*technique effect*)

$$\Delta\eta_i = \left[\left(\frac{\psi_i^c}{\psi_i}\right)^{\frac{1}{1-\hat{\sigma}}} \frac{e_i}{e_i^c} - 1\right] 100, \tag{5.24}$$

and a change in the scale of production (*scale effect*)

$$\Delta q_i = \Delta Y_i - \Delta p_i. \tag{5.25}$$

That is $\Delta E_i = \Delta\eta_i + \Delta q_i$.

Counterfactual trade flows. The reaction of trade flows to carbon price and trade cost shocks depends on changes in bilateral relative to multilateral trade barriers as well as market size effects:

$$\Delta X_{ij} = \left[\frac{\widehat{\tau_{ij}^{1-\sigma}}^{c}/(P_i^c P_j^c)^{1-\sigma}}{\widehat{\tau_{ij}^{1-\sigma}}/(P_i P_j)^{1-\sigma}} \frac{\theta_i^c \theta_j^c Y^{w,c}}{\theta_i \theta_j Y^w} - 1\right] 100. \tag{5.26}$$

In climate policy scenarios, the evolution of relative trade barriers is shaped by GE-driven changes in trade cost incidences only. The market size effect is driven by GDP shares and changes in nominal world GDP.

To study trade creation and trade diversion, it will be convenient to study trade shares $\frac{X_{ij}}{Y_j}$, i.e. country j's import value from exporter i as a fraction of its GDP. In climate policy scenarios, we expect that climate-active countries will increase their import shares from other regions. While their respective share in the other country's GDP will most likely fall. But it all depends on non-trivial changes in GDPs and price levels. Alternatively, we can express bilateral trade in embodied CO_2 associated with the respective trade flow instead of in dollar values. This informs about bilateral emission relocation. As with goods trade, let us express emission imports as a share of domestic emissions $\frac{\eta_i' X_{ij}}{E_j}$. The import share is evaluated with the exporter's emission intensity $\eta_i' \equiv E_i/Y_i$ taking into consideration that trade flows are given in dollar values. It may rise or fall in the counterfactual. Ultimately, it is an empirical question how emission imports change in a given policy experiment.

5.3 Empirical evidence

Before we turn to counterfactual simulation, we structurally estimate the model and assess how well it predicts the benchmark data. Section 5.3.1 describes the data. Section 5.3.2 presents results for our structural gravity parameter estimates obtained with the methodology laid down in section 5.2.2.

5.3.1 Data

We investigate a cross-section of country pairs in the year 2007. This choice is driven by data availability of a cross-section of carbon and energy prices for that year. Additionally, the financial crisis had not yet hit the world economy and 2007 also coincides with the last year before the first Kyoto commitment period. Data on bilateral exports in free-on-board values stem from the UN Comtrade database.[17] Bilateral distance and dummies for contiguity and common language are obtained from the CEPII distance dataset generated by Mayer and Zignago (2011). Data on bilateral free trade agreements (FTA) stem from the WTO homepage.

The method to identify σ requires information on capital stocks, labor force and land endowments. Data on labor force, population and land endowments are taken from the UN World Development Indicators (WDI) 2011. The labor force series comprises the economically active population aged 15 and older. For land endowments, we use the land area series which measures surface area.[18] A country's physical capital stock is computed with the perpetual inventory method.[19] The necessary data on investment and GDP in constant PPP-adjusted dollars are taken from the Penn World Tables 7.0. It is not available for Azerbaijan, Estonia, Russia and Ukraine. Consequently, these countries cannot be included in the computation of $\hat{\sigma}$ and the correlation of baseline predictions with actual data. Yet, the counterfactual equilibrium does not depend on fixed endowments and TFP, see (5.18). A large sample containing all important countries and a realistic production structure are important to ensure a credible benchmark. So despite the missing capital stock data for four countries, the default is to work with four production factors and the full sample. In a

[17] The exports series originally includes re-exports. The data series is adjusted for re-exports with the re-exports data provided by UN Comtrade.

[18] Arable land is not available for all countries. Results are unaffected by using arable land instead.

[19] The respective STATA routine "stockcapit" is due to Amadou (2011).

robustness check, we use labor and energy as only production factors and compute the fit in the whole sample.

The computation of a country's total factor productivity (TFP or A_i) stems from the production structure.[20] Take logs on both sides of (5.8) and solve for $\ln A_i = \ln q_i - \beta_i \ln E_i + \sum_f \alpha_{if} \ln V_{if}$. The output quantity q_i is proxied by a country's real GDP per capita from the PWT 7.0. TFP of the USA is normalized to one.

Finally, implicit carbon prices are constructed from the Global Trade Analysis Project (GTAP) 8 database. For its base year 2007, GTAP 8 provides information on a country's firms' expenses on fuels, i.e. oil, gas, coal and petroleum products. Tax payments for energy use are a part of this. Implicitly, we can think of these expenses as costs of CO_2 emissions. GTAP also features information on firms' fuel usage (in million tons of oil equivalents, Mtoe) and CO_2 emissions (in Mt of CO_2). This allows us to construct a country's average implicit CO_2 price as the sum of firms' fuel expenses divided by the firms' CO_2 emissions.[21] In a similar vein, we could also construct an average fuel price. GTAP also provides information on firms' tax payments for fuel use. This gives an indication of the climate policy induced part of implicit CO_2 prices.

Table 5.1 provides summary statistics. There are 9,446 country pairs or equivalently 103 countries in the dataset.[22] Average bilateral exports amount to about one billion US dollars. A country pair's major economic centers on average lie about 7,000 kilometers apart. About 30% of the country pairs have signed a free trade agreement. Average industrial CO_2 emissions amount to 192 Mt of CO_2. The average physical capital stock amounts to 1,602 billion US-\$. The average country's labor force is about 27 million people and its land area amounts to 999,000 square kilometers. In terms of implicit carbon prices, we observe substantial variation in our data. The average price for one ton of CO_2 is 322 US-\$ with a standard deviation of 177 US-\$ per ton. With below 100 US-\$ per t of CO_2, Mongolia, South Africa and Kazakhstan have the lowest carbon prices followed by China with 102 US-\$ per t of CO_2. On the other end of the distribution are the Netherlands, Sweden and Singapore with carbon prices above 800 US-\$. The correlation between a country's carbon and fuel price is 60%, and highly significant. The less than perfect correlation could be due to differences

[20] For a detailed methodological description see, for example, Benhabib and Spiegel (2005), Hall and Jones (1999) or Hall (1990).

[21] This is the average carbon price for a country's industry. Households' and governments' expenses for energy as well as their emissions are disregarded.

[22] A country list is relegated to the Appendix.

Table 5.1: Summary statistics for year of 2007

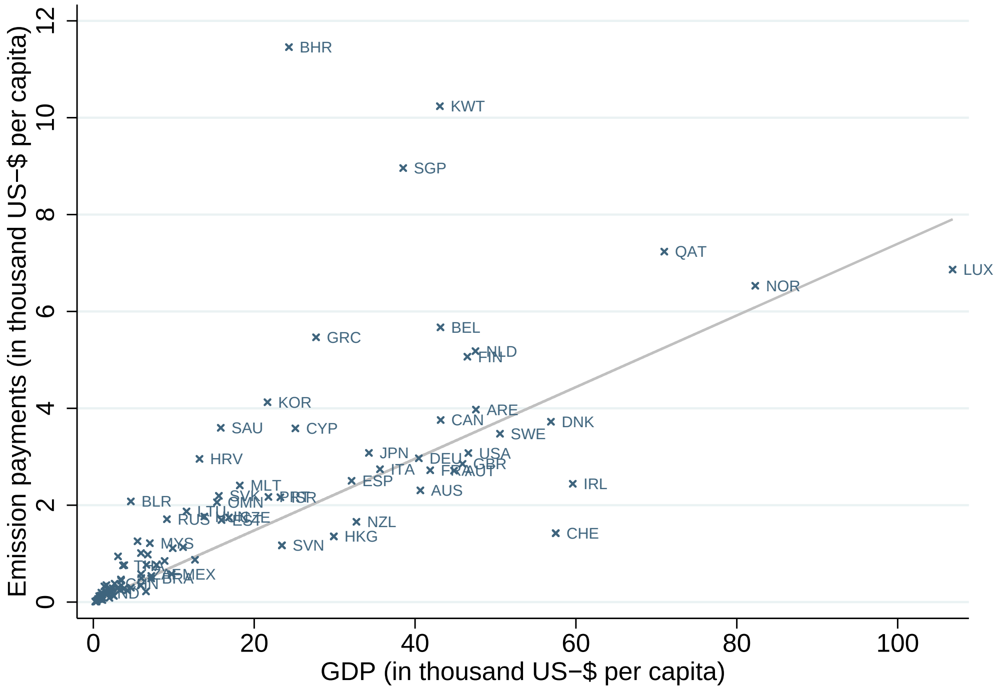
Variable	Obs.	Mean	Std. Dev.	Min	Max
Bilateral variables					
Exports, million US-$	9,446	1,232	7,897	0	310,480
Distance, km	9,446	7,237	4,483	60	19,812
Contiguity	9,446	0.03	0.17	0.00	1.00
Common language	9,446	0.13	0.33	0.00	1.00
FTA	9,446	0.28	0.45	0.00	1.00
Country-specific variables					
GDP, billion US-$	103	522	1,556	4	14,062
Emission, Mt of CO_2	103	192	630	1	4,886
Labor force, thousands	103	27,086	88,607	172	771,079
Physical capital, billion US-$	99	1,602	4,360	18	33,245
Land area, thousand km^2	103	999	2,434	0	16,378
Implicit CO_2 price, US-$ per t CO_2	103	322	177	62	1,010
Implicit CO_2 tax, US-$ per t CO_2	103	37	45	-19	179
Technology parameters					
TFP	103	0.38	0.36	0.02	2.18
β	103	0.12	0.07	0.02	0.47
α_{labor}	103	0.44	0.12	0.15	1.21
α_{capital}	103	0.42	0.10	0.20	0.88
α_{land}	103	0.02	0.02	0.00	0.09

Note: The table shows summary statistics of bilateral and country-specific gravity variables in the year 2007. The physical capital stock is given in PPP-adjusted 2005 constant US-$. Other monetary variables are in current US-$. TFP is total factor productivity relative to the USA.

in countries' fuel mixes. On average, a country's implicit carbon tax is 13 US-$ per ton of CO_2. The highest carbon tax is found in the Netherlands and Sweden (179 and 171 US-$ per ton, respectively). But some countries also subsidize energy use. Countries with very low carbon taxes are typically oil-exporting countries like Bahrain, Qatar, United Arab Emirates or Saudi Arabia.

Table 5.1 also shows technology parameters as observed in the data. Productivity differs between countries. TFP ranges from a minimum of 0.02 in Ethiopia to a maximum of 2.18 in Luxembourg. An average TFP of 0.38 implies the average sample country is less productive than the US. The average β observed in our data is 0.116 with a standard deviation of 0.07. We observe heterogeneity in countries' firms' emission use. Figure 5.1 plots countries' implicit payments for CO_2 emission against GDP; for scaling reasons on a per capita basis. The gray line results from a simple OLS regression of emission payments on GDP. It represents the estimated average emission cost share $\hat{\beta} = 0.072$ (standard error of 0.006, $R^2 = 0.9$).

Figure 5.1: Implicit payments for emissions and GDP



Note: The graph shows countries' per-capita implicit emission payments and GDPs. The gray line results from a simple OLS regression of emission payments on GDP, the slope coefficient is the average $\hat{\beta} = 0.072$, robust standard error of 0.006. Countries above the gray line have higher than average emission cost shares, and vice versa.

Countries above the gray line have higher than average emission cost shares, and vice versa. Table 5.1 also provides the average cost share of labor (44%), capital (42%) and land (2%).[23] Together with the energy cost share of 11.6%, these cost shares add up to approximately one. The assumption of constant returns to scale seems adequate. Repeating the simple OLS regression of factor payments on GDP, we find estimated average cost shares $\hat{\alpha}_{\text{labor}} = 0.633$ (s.e. 0.034), $\hat{\alpha}_{\text{capital}} = 0.282$ (s.e. 0.024), and $\hat{\alpha}_{\text{land}} = 0.004$ (s.e. 0.001). These values seem plausible, too. For example, the TFP literature (Hall, 1990; Benhabib and Spiegel, 2005) typically works with labor and capital shares of two and one thirds, respectively.

5.3.2 Structural gravity parameter estimates

With this data, we now apply the method laid out in section 5.2.2 to structurally estimate the model parameters. Table 5.2 provides gravity estimates and the implied estimate for $\hat{\sigma}$

[23] The maximum observed cost share of labor is 1.21 in Belarus. Note that this must be a measurement error. Without Belarus, the maximum cost share of labor is 0.67, the average cost share of capital is 0.68. The averages are unaffected by excluding Belarus though.

for OLS and PPML estimation. Table 5.2 also shows correlations of trade flows, GDPs and emissions predicted from the benchmark model with actual data.

First, the upper panel in Table 5.2 column (1) shows gravity parameters from an OLS estimation of (5.12) with importer and exporter fixed effects. The parameter estimates of all trade cost proxies are sensible and highly statistically significant. Bilateral distance affects trade flows negatively. A shared border, a common language and a bilateral FTA on the other hand increase bilateral trade flows. With OLS, we are able to explain roughly 80% of the variation in bilateral trade flows.

Second, the estimated $\hat{\sigma}$ is 4.801 with a bootstrapped standard error of 0.031. Last, once we have solved the benchmark model for equilibrium scaled prices we can test how well the model predicts actual GDPs, CO_2 emissions and trade flows. The correlation of predicted with actual GDPs is 0.967 and highly statistically significant. The match between predicted and actual emission levels is high, too. The correlation is 0.967. Ultimately, the correlation of predicted and actual trade flows is 0.697. This is comparable in size with the baseline correlations found in Bergstrand et al. (2013). In summary, the model is fairly successful in predicting actual data.

Column (2) presents the corresponding results from PPML estimation. First, all trade cost elasticities are highly significant. They differ from the respective OLS estimates, as is typical (see for example Santos Silva and Tenreyro, 2006). Taking into account zero trade flows raises the explanatory power of the model to roughly 90%. Second, the estimated $\hat{\sigma}$ is 5.259 with a bootstrapped standard error of 0.048. It is significantly higher than the estimate obtained from OLS estimation – but in the same order of magnitude. Last, the correlation of model prediction and actual data is again very high. Correlation of both GDP and emissions is comparable to the OLS case. However, in terms of trade flows the model prediction with PPML-estimated trade costs and elasticity of substitution achieves a higher correlation of 80% with the actual data. Consequently, we choose PPML estimation and the ensuing parameter estimates as default.

We experiment with technology assumptions and the data used. Since technology is absorbed by importer and exporter fixed effects gravity estimates are unaffected. Table 5.3 presents the implications for the estimated elasticity of substitution in column (1) and the baseline model fit in columns (3)-(5). The first row replicates the default, i.e. four production factors and country-specific factor cost shares, for ease of comparison. Disregarding country

Table 5.2: Estimation results

	OLS	PPML
Gravity estimates		
$(1-\sigma)\varrho$	-1.311***	-0.626***
	(0.036)	(0.036)
$(1-\sigma)\delta_{\text{FTA}}$	0.436***	0.478***
	(0.058)	(0.068)
$(1-\sigma)\delta_{\text{Contiguity}}$	0.867***	0.439***
	(0.135)	(0.077)
$(1-\sigma)\delta_{\text{Common language}}$	0.932***	0.307***
	(0.072)	(0.079)
Observations	9,446	10,506
R^2	0.782	0.881
F-statistic	165.079	
Log-likelihood		-5,714
Parameter estimates		
$\hat{\sigma}$	4.801	5.259
	(0.031)	(0.048)
Correlation baseline predictions with actual data		
X_{ij}	0.697***	0.798***
Y_i	0.967***	0.972***
E_i	0.967***	0.960***

Note: OLS and Poisson pseudo maximum likelihood (PPML) gravity estimation includes exporter and importer fixed effects and constant (not shown). Heteroskedasticity-robust standard errors in parentheses. $\hat{\sigma}$ from Bergstrand et al. (2013) methodology, see Section 5.2.2. Technology assumptions: all four production factors with country-specific factor cost shares and TFP differences. Standard errors for $\hat{\sigma}$ obtained via bootstrapping. The lower part of the table shows correlations of the actual data with model predictions in the baseline. *, **, *** denotes statistical significance at the 10, 5 and 1% level, respectively.

heterogeneity in factor use leads to a lower correlation of predicted with actual data. Using labor and emissions as only production factors gives a comparable $\hat{\sigma}$. It leads to a lower correlation of predicted with actual data for GDPs and trade flows, though. If we use fuel prices and energy use instead of implicit CO_2 prices and emissions we again get results very similar to the default. Given that the implicit CO_2 price takes into account energy use and is not just based on emission taxation this is not surprising. With population instead of labor force in the calculation of σ the estimated elasticity of substitution is 4.855.

In summary, the default we choose is most successful in replicating the baseline scenario. Acknowledging country-specific factor use is important. The error from using labor and emissions only and disregarding capital and land seems minor. The finding of a σ in the order of magnitude of 5 is fairly robust.

Table 5.3: Elasticity of substitution, technology and data fit

	(1)	(2)	(3)	(4)	(5)
	Estimates		Correlation prediction with data		
	$\hat{\sigma}$	$\hat{\text{se}}_\sigma$	X_{ij}	Y_i	E_i
PPML estimation					
Default	5.259	(0.048)	0.798***	0.972***	0.960***
Average factor cost shares	5.259	(0.048)	0.619***	0.890***	0.971***
Labor and emission only	5.110	(0.048)	0.660***	0.916***	0.976***
Labor, E_i only + avg. shares	5.159	(0.071)	0.613***	0.887***	0.970***
Fuel[a]	5.215	(0.065)	0.788***	0.981***	0.970***
Population[b]	4.855	(0.028)	0.784***	0.978***	0.967***
OLS estimation					
Average factor cost shares	4.801	(0.031)	0.501***	0.892***	0.972***
Labor only	4.746	(0.027)	0.527***	0.918***	0.974***
Labor only + avg. cost shares	4.743	(0.035)	0.497***	0.891***	0.970***
Fuel[a]	4.893	(0.052)	0.680***	0.979***	0.977***

Note: Elasticity of substitution $\hat{\sigma}$ estimated with Bergstrand et al. (2013) methodology based on gravity estimates from PPML and OLS estimation, respectively, as in Table 5.2. Standard errors for $\hat{\sigma}$ in column (2) obtained via bootstrapping. [a] Energy prices and energy use to compute $\hat{\sigma}$ and correlation. [b] Population instead of labor force. *, **, *** denotes statistical significance at the 10, 5 and 1% level, respectively.

The elasticity of substitution is a key parameter in international trade. Numerous studies provide an estimate. Anderson and van Wincoop (2003) assume $\sigma = 5$ in their benchmark, and consider a range of 2 to 10 as plausible. More recently, Feenstra et al. (2012) use a nested CES utility function and distinguish the Armington elasticity between domestic and imported varieties on the upper tier and imported varieties from different countries of origin on the lower tier. They find an elasticity of 3.1 between foreign varieties, while their estimates indicate a low substitutability between domestic and foreign goods i.e. an upper tier elasticity close to one. Bergstrand et al. (2013) uncover a $\hat{\sigma}$ of about 7 with their proposed method. They investigate the Canada-US border puzzle (where productivity differences might be minor) and labor is the only production factor. Given that our model relaxes the assumption of one production factor and allows for TFP differences across countries it is no surprise that our estimate differs from their result.

Concluding, in our structural gravity model important model parameters are consistently estimated from the data. The estimated value of $\hat{\sigma} = 5.259$ lies well within the range of typical estimates. The model is fairly successful in replicating the baseline. It provides a benchmark against which to evaluate counterfactual scenarios.

5.4 Counterfactual climate and trade policy scenarios

We now conduct two types of policy experiments and evaluate their effects on GDPs, trade, emission and emission relocation. First, we develop counterfactual climate policy scenarios. We focus on an EU emission certificate price increase. The certificate price is set such that the EU achieves its Kyoto target of -8%. What fraction of emission savings relocates to non-EU countries? And how heterogeneous is carbon leakage across countries? The same policy experiment is repeated for emission targets promised for the second commitment period under the Kyoto Protocol.

A second set of policy experiments deals with the effect of trade liberalization on trade and emissions. More specifically, we want to shed light on environmental aspects of FTA formation. Recently, a discussion on a transatlantic FTA between the EU and the US has come up. We study effects on trade and emissions of this hypothetical FTA.

5.4.1 Counterfactual increase in the EU's emission allowance price

Under the Kyoto Protocol, the 27 European Union countries have promised a reduction of their GHG emissions by 8% on average in the period 2008-2012 compared to the base year 1990. To reach this goal, the EU's ETS has been in place since 2005. Via national allocation plans, each country is assigned a specific emission cap. Subsequently emission certificates can be traded to ensure cost-minimizing emission reductions, e.g. on the European Climate Exchange in London or the European Energy Exchange in Leipzig. This implies there is one uniform EU emission allowance (EUA) permit price.

However, in 2007 the EUA permit price fell to almost zero.[24] At the same time, the EU's emissions still stood above 92% of the 1990 CO_2 emission level. Taking GTAP data to be model-consistent[25], the required emission reduction amounts to 131.8 Mt of CO_2. To reach its Kyoto target, the EU has to reduce its emission allowances. Or equivalently increase its carbon price via an increase in the EUA permit price; we simulate such a policy experiment. We add a uniform counterfactual EUA permit price to each EU country's implicit carbon

[24] See, EUA spot price data provided by the European Energy Exchange on `www.eex.com`.

[25] In GTAP, EU emissions total 4,033 Mt of CO_2 (of which 3,109 Mt stem from domestic firms' emissions). The official UNFCCC data report 4,420 Mt of CO_2. So the GTAP data is approximately 5% below this level.

price. The analysis focuses on emission effects, but also touches on implications for trade shares and real GDP.

Let us start with the default modeling assumptions, i.e. country-specific factor cost shares, PPML estimation of trade costs and an estimated $\hat{\sigma} = 5.259$. Our model simulation predicts that an EUA permit price of 15 US-$ will suffice to bring the EU approximately on track. This reduces nominal world GDP by about 0.01% because the world "emission endowment" has shrunk. Column (1) in Table 5.4 shows the corresponding EU emission savings of about 131.4 Mt of CO_2. Without production relocation the world emission savings could have been larger, though. Some emission migrates to other countries. The predicted emission relocation amounts to 10.0%.

Table 5.4: Key statistics ETS scenarios

	(1)	(2)	(3)	(4)	(5)
	Default		Sensitivity analysis		
Assumptions:		OLS	Avg. cost shares	Labor + emission	Fuel[a]
ΔEUA price (in US-$ per t of CO_2)	15	15	15	15	26
$\hat{\sigma}$	5.259	4.801	5.259	5.110	5.215
Emission saving (in Mt of CO_2)	131.4	131.4	128.6	131.3	131.8
Emission relocation (in %)	10.0	10.1	9.0	10.0	10.9

Note: The table shows the assumed carbon price increase, estimated $\hat{\sigma}$ as well as counterfactual EU emission savings and emission relocation for the policy experiment of increasing the EUA price. Column (1) shows the default scenario. Columns (2)-(5) show the same statistics for sensitivity checks varying underlying assumptions. [a] In the fuel scenario the price increase is like an energy tax, given in US-$ per toe.

These aggregate numbers mask considerable country heterogeneity. Starting with EU countries, we look at effects on country-specific emissions, trade shares and real GDP. The results are summarized in Table 5.5. Then we turn to non-EU countries.

The average EU country's industrial emission falls by roughly 5%[26], but the effect ranges from -1.91% in Sweden to -12.08% in Estonia. First and foremost, this is explained by a *technique effect*, i.e. a reduction in EU countries' emission intensities. The counterfactual price increase is added to the initial carbon price. Hence, heterogeneity in the technique effect results from heterogeneity in the initial prices across the EU. For countries like Sweden and the Netherlands, with high CO_2 prices of 824 and 835 US-$ per t CO_2, respectively, an extra

[26] The emission change is measured against domestic firms' emissions, not against a country's total emissions. Households' CO_2 emissions from consumption activities like heating or car driving are not factored in.

Table 5.5: Heterogeneity of country effects for EUA permit price increase

Country	Emission effects			Trade share with non-EU countries				Real
	Total	Technique	Scale	Goods		Emissions		GDP
				Import	Export	Import	Export	
	ΔE_i	$\Delta \eta_i$	Δq_i	$\Delta \frac{X_{ji}}{Y_i}$	$\Delta \frac{X_{ij}}{Y_j}$	$\Delta \frac{\eta_j' X_{ji}}{E_i}$	$\Delta \frac{\eta_i' X_{ij}}{E_j}$	$\Delta \frac{Y_i}{P_i}$
Estonia	-12.08	-10.87	-1.28	0.04	-1.00	12.43	-1.10	-1.06
Poland	-8.52	-7.69	-0.87	0.22	-0.61	8.55	-0.67	-0.86
Czech Republic	-8.32	-7.49	-0.86	0.27	-0.60	8.41	-0.67	-0.90
Slovenia	-8.20	-7.81	-0.41	0.16	-0.23	9.06	-0.31	-0.42
Bulgaria	-7.79	-6.07	-1.78	0.17	-1.37	6.88	-1.43	-1.57
Latvia	-6.84	-6.38	-0.47	-0.01	-0.29	7.02	-0.35	-0.31
Cyprus	-6.41	-5.52	-0.91	0.33	-0.67	5.85	-0.70	-1.02
Malta	-6.13	-5.34	-0.81	0.09	-0.56	5.91	-0.62	-0.69
Romania	-5.68	-5.14	-0.55	0.29	-0.35	5.68	-0.40	-0.67
Ireland	-5.58	-5.35	-0.23	-0.01	-0.09	5.88	-0.14	-0.11
Greece	-5.32	-4.29	-1.05	0.13	-0.73	5.10	-0.77	-0.90
Luxembourg	-4.93	-4.62	-0.32	0.08	-0.15	5.29	-0.20	-0.26
Denmark	-4.64	-4.34	-0.30	0.03	-0.14	4.74	-0.18	-0.20
Spain	-4.10	-3.79	-0.32	0.18	-0.16	4.19	-0.19	-0.36
Hungary	-3.69	-3.23	-0.47	-0.04	-0.27	3.59	-0.30	-0.26
Portugal	-3.59	-3.24	-0.36	0.10	-0.19	3.53	-0.21	-0.31
Slovak Republic	-3.56	-3.07	-0.50	0.19	-0.29	3.29	-0.31	-0.51
Germany	-3.48	-3.22	-0.25	0.10	-0.10	3.52	-0.12	-0.23
Finland	-3.35	-2.99	-0.37	0.09	-0.20	3.27	-0.23	-0.32
United Kingdom	-3.24	-3.04	-0.20	0.07	-0.07	3.38	-0.09	-0.17
Italy	-3.24	-2.99	-0.25	-0.06	-0.10	3.26	-0.12	-0.07
France	-2.77	-2.60	-0.18	0.27	-0.04	2.90	-0.06	-0.34
Austria	-2.75	-2.58	-0.17	0.21	-0.02	2.70	-0.04	-0.26
Lithuania	-2.74	-2.30	-0.44	0.13	-0.26	2.55	-0.27	-0.41
Belgium	-2.32	-2.02	-0.30	0.04	-0.14	2.22	-0.15	-0.21
Netherlands	-1.96	-1.74	-0.21	0.16	-0.06	1.54	-0.07	-0.26
Sweden	-1.91	-1.78	-0.13	-0.00	-0.01	1.81	-0.02	-0.04
EU average	-4.93	-4.43	-0.52	0.12	-0.32	4.91	-0.36	-0.47

Note: The policy experiment is an EUA permit price increase of 15 US-\$. The table shows percentage changes of the respective variables. Index $i \in$ EU refers to the respective EU country. For trade effects, $j \notin$ EU indexes non-EU countries.

charge of 15 US-\$ per ton matters relatively little. Consequently, the adjustment in their emission intensity is comparably small. On the other hand, for countries with relatively low initial implicit carbon prices like Estonia (120 US-\$ per t CO_2) the relative CO_2 price increase is large; and thus the technique effect. A small fraction of total emission reductions is explained by a *scale effect*, i.e. a reduction of the output level.

Next, we investigate trade effects. Carbon leakage is observed when a unilateral strengthening of climate policy in one region results in production relocation and increased imports from other countries. Indeed, most EU countries increase imports (measured as a share of domestic GDP) from an average non-EU country. On average, the import share goes up by 0.12%, with large heterogeneity across the EU. These average effects on countries' trade shares mask heterogeneity at the country-pair level. In general, import shares increase most from countries in close proximity to the EU. Such as, for example, Albania, Croatia, Turkey, Switzerland, Tunisia, Norway, Morroco or Russia. At the same time, the average EU country's share of exports in foreign GDP drops by roughly -0.32%. Least affected are Sweden (-0.01%) and Austria (-0.02%); whereas Bulgaria's (-1.37%) and Estonia's (-1.00%) export shares drop most. Distance also affects the change in bilateral export shares. They fall most with respect to countries like Brazil, Mexico and the USA.

By looking at emission trade shares, we can investigate bilateral emission relocation. Due to trade, the CO_2 emitted during the production of the traded good flows virtually, i.e. embodied in the traded good, from the exporter to the importer. In all EU countries, emission imports as a share of domestic emissions go up; by between 1.8 and 12.4%. On the other hand, emission exports as a share of foreign emissions go down. That implies EU countries' net emission imports rise in our policy experiment. This supports the finding of emission relocation on the aggregate level.

Last, nominal GDP falls in all EU countries while the price levels increase. This results in an unambiguous reduction of real GDP of about -0.47% on average.[27] The country-specific real GDP changes display heterogeneity. Small EU and predominantly Eastern European countries like Bulgaria, Estonia, Poland and the Czech Republic are among the biggest losers.

In non-EU countries, emissions fall as well. On average by 0.009%, see Table 5.6. This mirrors the reduction in nominal world GDP. There are regional disparities though, driven by proximity to the EU. For ease of presentation, Table 5.6 summarizes average changes in percent by continent. Emission reductions are strongest in non-EU European countries and Africa. But production relocation leads to emission increases in non-EU countries, see column (2) in Table 5.6. Figure 5.2 illustrates the country-specific heterogeneity in a world map. Emission relocation is a result of general equilibrium effects only. The magnitude is

[27] Real GDP is a measure for welfare in the standard gravity literature without emissions. With CO_2 emissions, there is an additional welfare channel. Emission reductions on the world level will positively influence welfare everywhere. The magnitude of the effect depends on a damage function, typically in additive form. How this damage function looks like is an open issue.

Table 5.6: Region-specific effects in non-EU countries for EUA permit price increase

Region	(1) Emission	(2) Emission relocation effects Total	(3) Technique	(4) Scale	(5) Real GDP
	ΔE_i	$\Delta \bar{E}_i$	$\Delta \eta_i$	$\Delta \theta_i - \Delta p_i$	$\Delta \frac{Y_i}{P_i}$
North America	-0.004	0.081	-0.004	0.085	0.135
Oceania	-0.005	0.080	-0.005	0.085	-0.110
South America	-0.006	0.079	-0.005	0.084	-0.023
Asia	-0.007	0.078	-0.006	0.083	0.009
Africa	-0.010	0.075	-0.009	0.084	-0.047
Non-EU Europe	-0.020	0.065	-0.016	0.080	-0.018
Average	-0.009	0.076	-0.008	0.084	-0.014

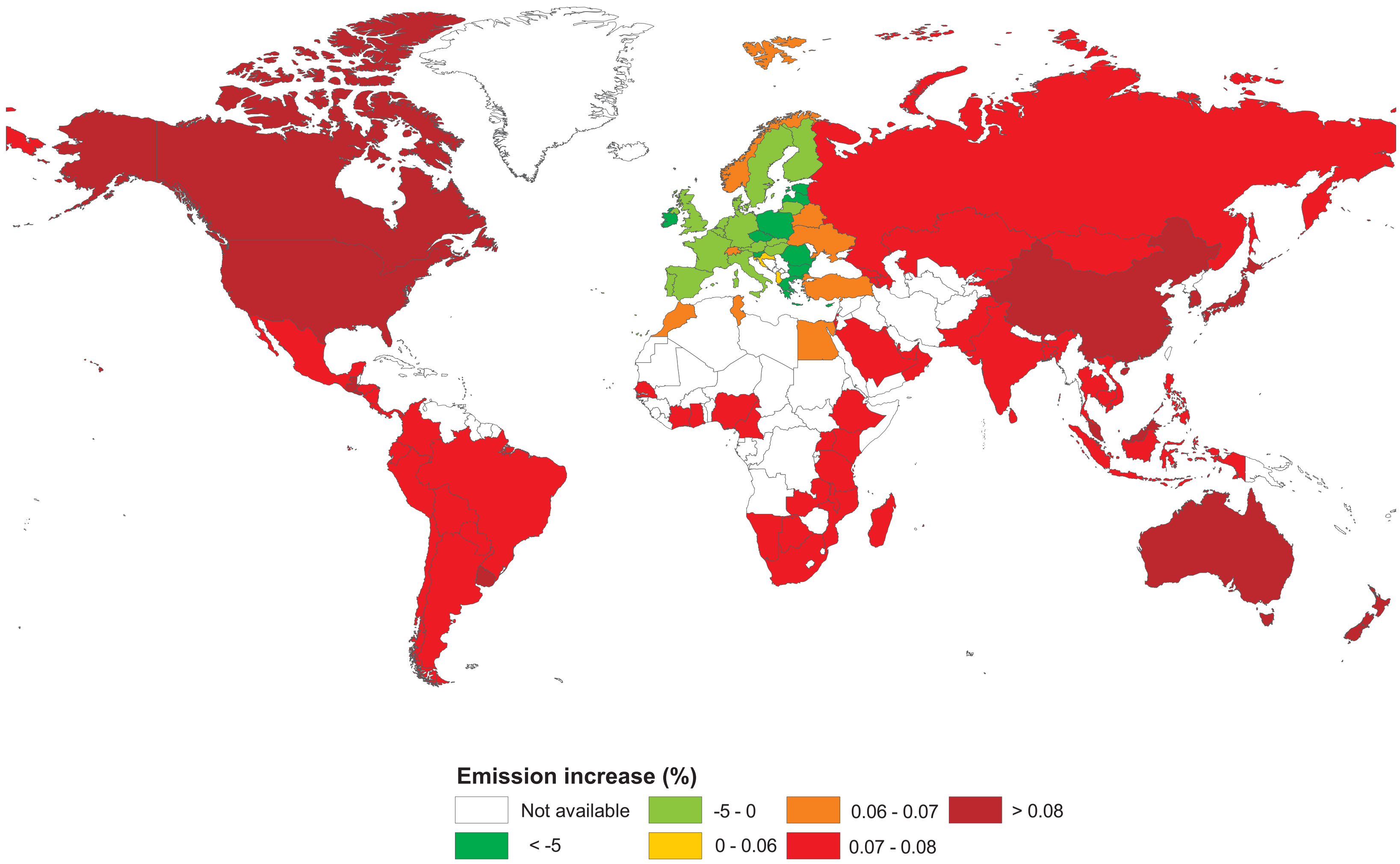
Note: The table shows regional average changes in respective variables (in %) for the policy experiment of a 15 US-$ EUA permit price increase. Emission relocation effects refer to emission changes from a benchmark without emission relocation. According to theory, technique and scale effect add up to the total effect.

small and ranges from an increase of 0.058% in Albania to an increase of 0.082% in Canada. This implies that country size matters for absolute emission relocation, i.e. in tons of CO_2. In absolute terms, large countries attract more of relocating emission. About 30 and 24% of the aggregate emission relocation effect of 10.0% is attributable to emission increases in the US and China, respectively. India's, Russia's, Japan's and Canada's share in the leakage rate are betweeen 7 and 2%. The emission relocation effect is made up of a negative technique effect and a positive market size effect, see columns (3) and (4), respectively. All non-EU countries slightly reduce their emission intensity because equilibrium scaled prices fall. The positive scale effect reflects gains in GDP shares in most non-EU countries. In terms of real GDP, North America and Asia profit from the EUA permit price increase. However, with the policy experiment at hand real GDP on average falls by 0.014% in non-EU countries.

Furthermore, Table 5.7 shows heterogeneity in trade effects distinguished by continent and with respect to EU and non-EU trade partners. The share of exports to EU countries rises most in Africa and non-EU Europe. The share of EU imports falls most in North America and Asia. We also observe trade creation between non-EU countries. In terms of embodied CO_2, other European countries increase their emission export share to the EU most.

So far, we focus on the effects of an EUA permit price of 15 US-$ because it corresponds nicely to the EU's Kyoto target. Nevertheless, we briefly discuss emission savings and implied

Figure 5.2: Country-specific emission increase from EUA permit price increase



Note: The graph shows countries' emission increase (in %) due to emission relocation, i.e. $\Delta \bar{E}_i$, resulting from an EUA permit price increase of 15 US-\$ using the default method; i.e. PPML estimation and $\hat{\sigma} = 5.259$.

Table 5.7: Heterogeneity of trade effects in non-EU countries

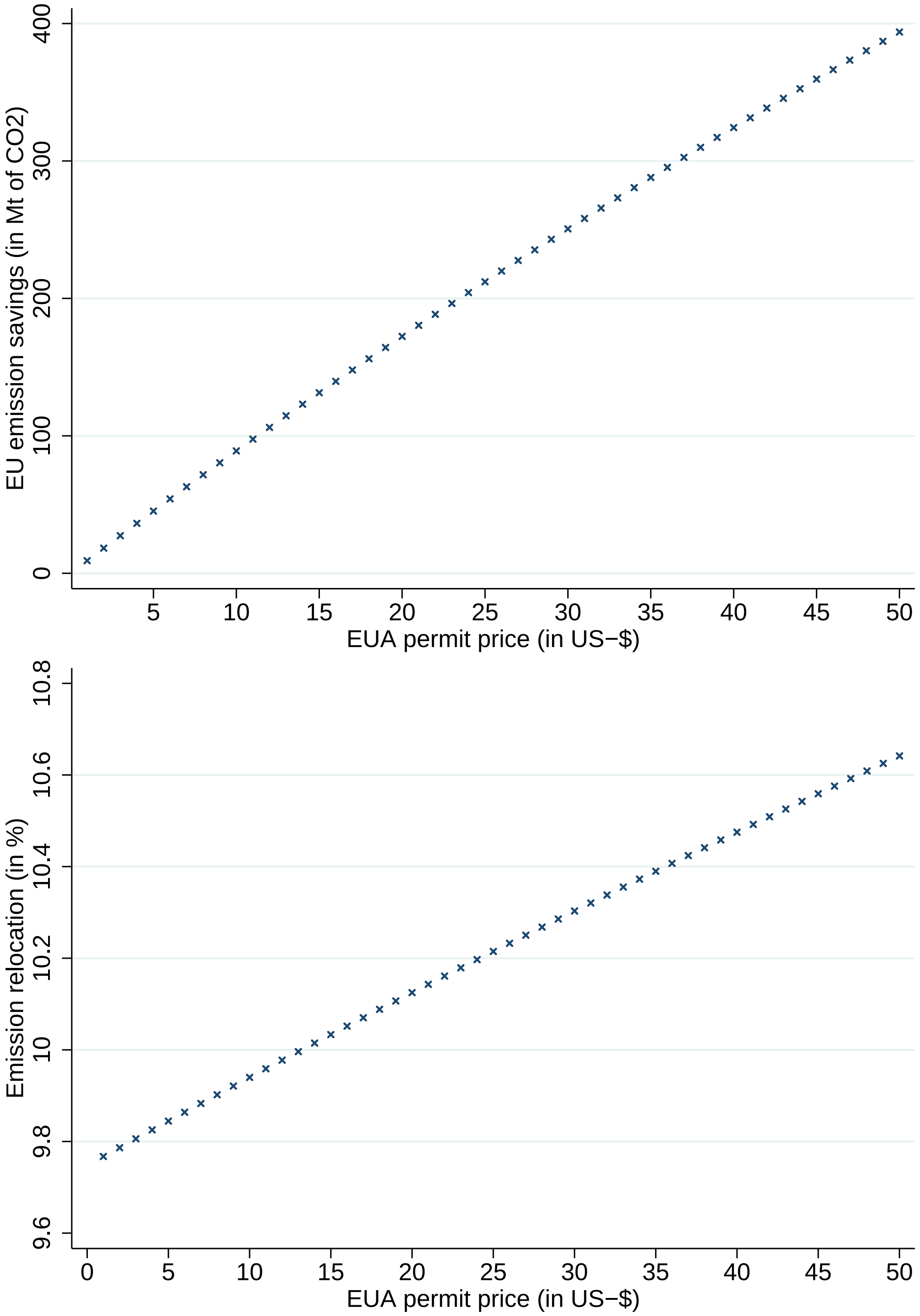
Region	(1) Goods trade share Import	(2) Goods trade share Export	(3) Emission trade share Import	(4) Emission trade share Export
Trade with EU countries				
Africa	-0.28	0.13	-0.36	4.83
Asia	-0.35	0.11	-0.37	4.85
Non-EU Europe	-0.29	0.15	-0.25	4.92
North America	-0.48	0.10	-0.42	4.89
Oceania	-0.24	0.11	-0.33	4.90
South America	-0.31	0.11	-0.37	4.74
Trade with non-EU countries				
Africa	0.10	0.06	0.06	0.07
Asia	0.03	0.05	0.04	0.05
Non-EU Europe	0.07	0.09	0.18	0.09
North America	-0.10	0.04	0.00	0.04
Oceania	0.14	0.04	0.09	0.05
South America	0.05	0.04	0.04	0.05

Note: The table shows changes in trade shares (in %) with EU and non-EU countries distinguished by continent.

emission relocation rates for a realistic range of EUA permit prices of 1-50 US-$ per ton of CO_2. EU emission savings appear to be approximately linear in the permit price, see the upper part of Figure 5.3. A 1 US-$ increase in the permit price brings about 7.8 Mt of CO_2 emission savings. On the other hand, the lower part of Figure 5.3 shows that the extent of emission relocation also increases with the EUA permit price. A 50 US-$ increase is associated with a one percentage point higher emission relocation. The leakage problem is more severe, the more stringent climate policy gets.

Sensitivity analysis. Table 5.4, columns (2)-(5) provide sensitivity checks. We vary the gravity estimation procedure and the technology assumptions. To save space, the focus is on EU emission savings and emission relocation in the aggregate. In column (2), gravity parameters are estimated with OLS. The EUA permit price of 15 US-$ per ton of CO_2 leads to virtually the same EU emission savings and emission relocation. This is true on the aggregate level, but also the predicted country-specific effects are very much in line (compare for example Table E.1 in Appendix E). Predicted percentage emission increases in other countries are slightly larger (compare Figure E.1 in Appendix E) but the correlation between the predictions is 1.000 and highly statistically significant. Column (3) assumes average

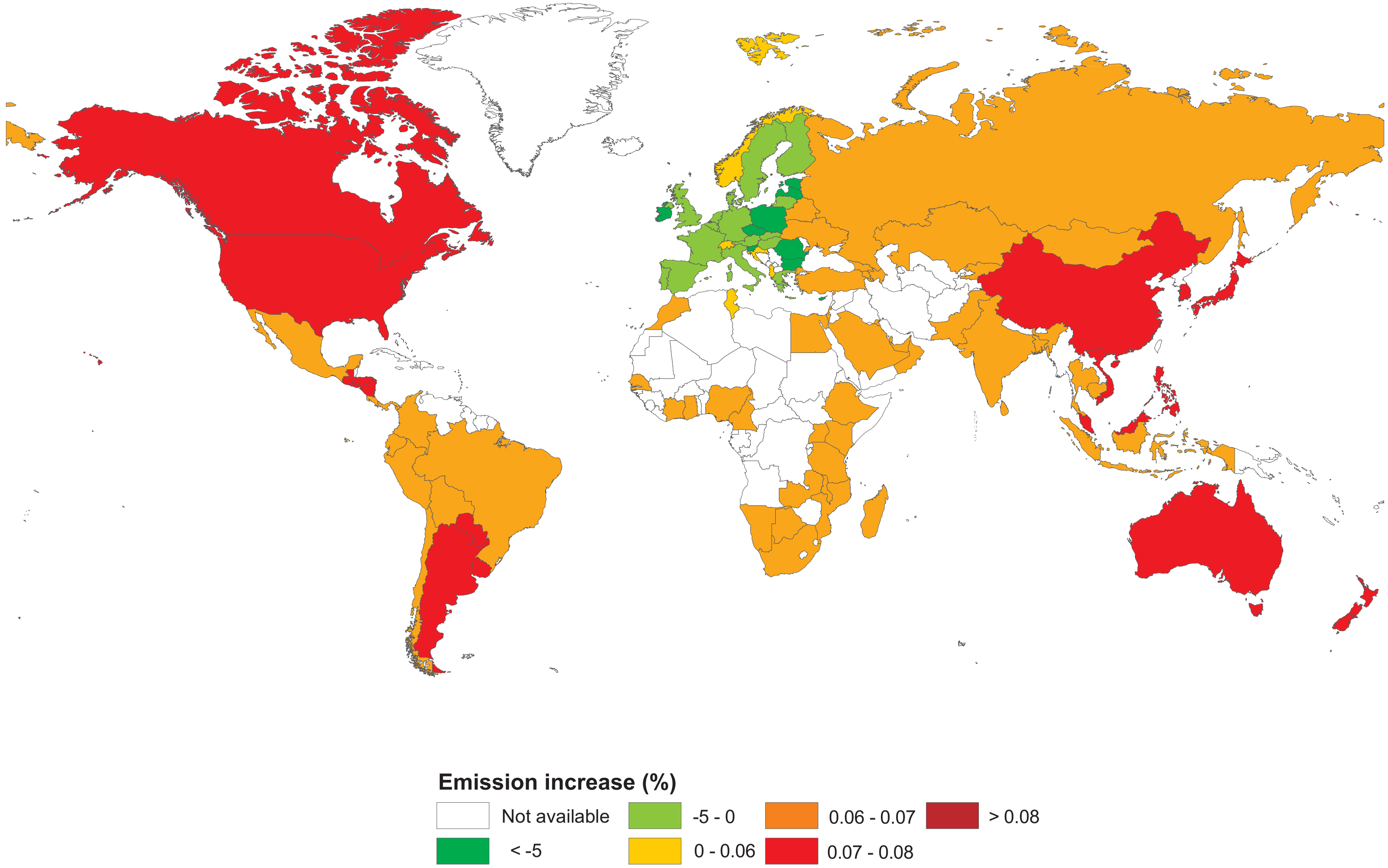
Figure 5.3: Emission savings, emission relocation and ETS permit price



Note: The upper part of the graph shows EU emission savings, the lower part the extent of emission relocation depending on the ETS price. The calculation assumes the default elasticity of substitution of $\hat{\sigma} = 5.259$.

instead of country-specific factor cost shares. Implied emission savings drop to 128.6 Mt of CO_2. The predicted emission relocation is 9.0% only. The heterogeneity of country-specific effects is also diminished, see Figure 5.4. In conclusion, disregarding country-specific factor

Figure 5.4: Country-specific emission increase from EUA permit price increase with average factor cost shares



Note: The graph shows countries' emission increase (in %) due to emission relocation, i.e. $\Delta\bar{E}_i$, resulting from an EUA permit price increase of 15 US-$ using PPML estimation and $\hat{\sigma} = 5.259$ but average factor cost shares.

intensities leads to an underestimation of leakage. If we use parameter estimates obtained from a model with labor and emissions as only production factors, emission relocation again is about 10.0%. Since emission relocation in the counterfactual does not depend on other production factors, see equation (5.23), this difference is attributable to a lower estimated $\hat{\sigma}$ only. Last, we analyze fuel prices instead of carbon prices. This requires a slight variation of the policy experiment because the policy variable is a country's energy price. An energy tax increase of about 26 US-$ per ton of oil equivalent would bring the EU on track. The predicted leakage rate is 10.9% and slightly higher than in the default. This is explained by the less than perfect correlation between implicit carbon and energy prices. The relative price increases differ in the two scenarios, and thus emission savings and relocation.

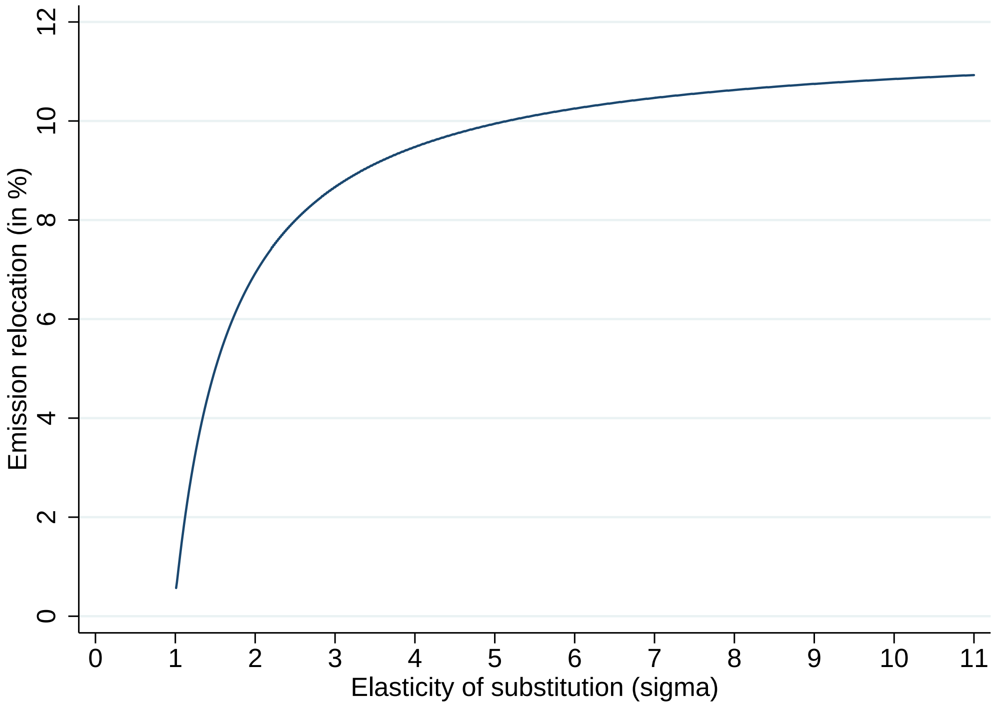
Summarizing, results are robust to the estimation technique chosen. However, the modeling of country-specific production features like country-specific factor cost shares matters. And the employed elasticity of substitution affects results as well. Figure 5.5 illustrates this point. It shows the extent of emission relocation plotted against elasticities of substitution in the range of 1 to 11. The function approximately grows logarithmically in σ. This implies results are very sensitive with respect to σ, especially for low levels of the parameter. Employing an elasticity of substitution informed by the data is crucial to get a reliable prediction of the extent of emission relocation. It is one of the major advantages of the structural gravity approach applied in this paper.

In conclusion, the EU ETS is a viable tool to help the European Union bring down its carbon emissions. An ETS price of around 15 US-$ will suffice to bring the EU on track for its Kyoto target. However, one has to take this with a pinch of salt. Without trade, world emission savings could have been larger. The analysis shows that due to production relocation there is an emission increase in non-EU countries in the order of magnitude of around 10% of EU emission savings. This effect is moderate but non-negligible. Trade undermines the environmental effectiveness of the hypothetical increase in the EUA permit price.

5.4.2 Other climate policy scenarios

The extent of emission relocation depends, amongst others, on a country's or region's climate policy stringency, the magnitude of the carbon price increase and the proximity to other trade partners. To conclude the section on climate policy scenarios, we briefly describe effects of

Figure 5.5: Emission relocation and elasticity of substitution of ETS price of 15 US-$



Note: The graph shows the extent of carbon leakage depending on the elasticity of substitution (σ) for an ETS price of 15 US-$.

policy experiments in other regions. We focus on Kyoto II but also briefly touch on climate policy in the US, Canada and China. To save space the analysis focuses on the extent of emission relocation and applies the default method only.

The Kyoto Protocol is the first international treaty establishing country-specific emission limits. On the Conference of Parties in Doha in December 2012, an extension of the Kyoto Protocol for a second period from 2013-2020 was discussed. In the following, we will refer to this second period as Kyoto II. Table 5.8 summarizes the promised emission reduction targets for Kyoto II countries, as well as the implied emission reduction requirements as of 2007. Kyoto II puts an emission cap on about 15% of world GHG emissions.[28] Kazakhstan, Belarus and Ukraine are already in line with their Kyoto II targets. This is explained by the choice of base year and massive emission reductions in the wake of industrial restructuring in these countries in the 1990s (*hot air*). However, compared to the benchmark year of our analysis the Kyoto II region has to reduce its CO_2 emissions by 490.2 Mt of CO_2 in total.

[28] Note that Japan, Russia and Canada had emission caps for the first Kyoto period from 2008-2012 but have not committed to emission reductions under Kyoto II.

Table 5.8: Kyoto II targets and emission saving requirements

	(1)	(2)	(3)
Country	Required CO_2 savings		Target as of base year
Reference year:	2007		1990[a]
	(in %)	(in Mt of CO2)	(in %)
Norway	35.7	23.8	-16
Croatia	25.7	5.8	-20
European Union	15.9	640.8	-20
Switzerland	14.4	6.3	-15.8
Australia	13.2	50.4	-2
Kazakhstan	-23.5	-45.9	-5
Belarus	-56.7	-30.9	-12
Ukraine	-60.5	-159.9	-24
Total	-9.7	490.2	

Note: The Table shows region-specific CO_2 emission saving requirements for Kyoto II between 2013-2020. Column (3) shows the promised target as specified in the proposed Addendum to the Kyoto Protocol. Columns (1) and (2) show relative and absolute emission savings compared to 2007. Regions are sorted in descending order of the relative required emission reduction. New Zealand is a Kyoto II country but has not yet specified a target. Iceland is a Kyoto II country but not part of the dataset.
[a] The Australian base year is 2000.

To simulate the Kyoto II policy experiment, we add a uniform carbon tax to the implicit carbon price in all Kyoto II countries.[29] In other words, we assume that the flexible mechanism Emission Trading aligns the climate policy price premium on CO_2 emissions in Kyoto II countries. The carbon tax is set at 39.1 US-$ per ton of CO_2 because it ensures the required emission savings, compare Table 5.9. World nominal GDP falls by 0.2% with this counterfactual policy change. Our model predicts emission relocation of 8.0%. This is smaller than the leakage rate predicted for the EU ETS scenario. It seems sensible. Kyoto II applies to a larger region. Possibilities to shift production away are more limited. Thus, less of the achieved emission savings leak away.

Table 5.10 summarizes emission, trade and real GDP effects in Kyoto II and non-Kyoto II countries. Real GDP falls almost everywhere except in North America. The relative emission reduction in Kyoto II countries ranges from -6% in Croatia to -32% in Kazakhstan. The average emission reduction in the European Union is 11%. Due to its mere size, 315.6 Mt of CO_2 or about 64% of the total emission reduction takes place in the EU. Within the Kyoto II countries, emission certificate trade will take place. Ukraine, Kazakhstan, Belarus and Australia generate more emission savings than they require. These countries will sell

[29] The exception being Iceland because it is not in the dataset; and New Zealand because it has not yet issued a specific Kyoto II target.

Table 5.9: Key statistics other climate policy scenarios

Scenario:	(1) Kyoto II	(2) US Kyoto target	(3) Canadian Kyoto target	(4) China
Carbon tax (in US-$ per t of CO_2)	39.1	105.5	139.0	108.7
Emission saving (in Mt of CO_2)	490.1	1,256.8	155.5	2,744.9
Emission relocation (in %)	8.0	7.1	9.9	3.2

Note: The table shows the carbon price increase assumed in the respective policy experiment, counterfactual emission savings for the respective region as well as emission relocation. Analysis based on default method, i.e. PPML and $\hat{\sigma} = 5.259$.

Table 5.10: Country effects for Kyoto II experiment

Country	Emission effects				Import shares[a]		Real
	Total		Technique	Scale	Goods	Emissions	GDP
	ΔE_i	$\Delta \bar{E}_i$	$\Delta \eta_i$	Δq_i	$\Delta \frac{X_{ji}}{Y_i}$	$\Delta \frac{\eta_j' X_{ji}}{E_i}$	$\Delta \frac{Y_i}{P_i}$
Kyoto II countries							
Kazakhstan	-32.267	-31.955	-28.334	-4.674	0.145	41.550	-4.413
Australia	-21.517	-21.223	-20.431	-1.220	0.317	26.482	-1.191
Ukraine	-19.815	-19.496	-14.181	-6.093	0.244	18.744	-5.305
Belarus	-12.375	-12.105	-7.065	-5.511	0.342	9.142	-4.723
European Union	-11.519	-11.242	-10.426	-1.172	0.359	12.645	-1.126
Switzerland	-9.265	-8.996	-9.047	-0.229	0.359	10.241	-0.326
Norway	-7.827	-7.557	-7.229	-0.621	0.204	8.159	-0.503
Croatia	-6.142	-5.856	-4.800	-1.376	0.315	5.883	-1.197
Non-Kyoto II countries							
North America	-0.016	0.258	-0.014	0.274	-1.223	-1.228	0.125
South America	-0.021	0.252	-0.019	0.272	-0.968	-1.088	-0.042
Asia	-0.033	0.241	-0.028	0.267	-1.020	-1.120	-0.035
Africa	-0.036	0.238	-0.033	0.272	-0.898	-1.042	-0.089
Oceania	-0.041	0.233	-0.039	0.273	-0.730	-1.057	-0.299
Europe	-0.069	0.205	-0.058	0.263	-0.697	-0.946	-0.247

Note: The table shows percentage changes of emissions, trade shares and real GDP for the policy experiment Kyoto II. [a] For Kyoto II countries import shares refer to imports from non-Kyoto II countries. For non-Kyoto II countries import shares refer to imports from Kyoto II countries.

certificates to the EU, Norway, Switzerland and Croatia.[30] Emission relocation leads to emission increases in non-Kyoto II countries. Again, we see that proximity to the climate-active region matters. The additional emissions in other European countries is smallest.

[30] This also implies negative real GDP effects for these countries are exaggerated because they receive additional income from international permit sales.

Kyoto II countries increase their share of non-Kyoto II imports. Compared to their domestic emissions, Kyoto II countries now import between 6 and 42% more emissions embodied in trade from non-Kyoto II countries. The emission import share increases by 13.4% on average. On the other hand, they export less to non-Kyoto II countries, see the respective import shares of non-Kyoto II regions. Addtionally, trade diversion leads to increased trade between non-Kyoto II countries (not shown).

The US is one of the world's largest CO_2 emitters with a comparatively low implicit carbon price of about 240 US-$ per t of CO_2. There have been discussions about possible US climate policy initiatives like the Waxman-Markey bill in recent years. In this light, it is interesting to simulate US climate policy efforts. Let us take the hypothetical US Kyoto target as a reference point.[31] The US emission savings required to meet this target would be huge: 1,255 Mt of CO_2 or 22% of the US firms' emissions in 2007. A carbon price of 105.5 US-$ would suffice to achieve this goal, see column (1) Table 5.9. The simulated emission relocation rate is 7.1%.

Canadian CO_2 emissions have increased quite substantially over the last two decades. Emission savings fall short of the -6% Kyoto target by 155 Mt of CO_2 or 28% of Canadian firms' emissions. Our findings imply that an extra charge of about 139 US-$ would bring Canada in line with its Kyoto promise. A price increase of almost 50%. Not surprisingly, Canada withdrew from the Kyoto Protocol in 2011. Column (2) in Table 5.9 shows the resulting emission relocation amounts to 9.9%.

Until now, all policy experiments dealt with carbon price increases in industrialized countries. We find that a doubling of the Chinese implicit carbon price to, for example, the Indonesian level[32] of 211$ per t of CO_2 would amount to no more than 2.8% leakage. Similarly, a 1% increase in the Chinese implicit carbon price would lead to 2.3% emission relocation only.

Summarizing, a first insight from our counterfactual climate policy simulations is that carbon leakage is moderate but non-negligible. In various climate policy experiments we found emission relocation rates between 2 and 10%. The order of magnitude is in line with most CGE findings. As a regularity, large countries attract most of the emission relocation; and more so if their implicit carbon price is low. Relative emission increases are smallest in

[31] The US is an Annex B country under the Kyoto Protocol and would thus have an emission reduction target of 7% compared to 1990. However, it never ratified the treaty.

[32] This is also in the neighborhood of the Mexican, US, Bulgarian or Ukrainian level.

countries close to the climate-active region. In general equilibrium, they are hampered most by the reduction of demand of important trade partners. Interestingly, a strenghthening of the Chinese climate policy leads to little leakage only.

5.4.3 Trade liberalization and emissions

With our framework, we can also study how trade liberalization affects trade and emissions. Globalization critics argue that trade liberalization could be bad for the environment when dirty production moves to countries with lax environmental regulation, i.e. these countries become *pollution havens*. Even though our model only features one sector of production, we might still learn how carbon price differentials shape countries' emissions and trade shares in trade liberalization scenarios.[33] We perform three counterfactual experiments. First, starting from a counterfactual without any free trade agreements, we introduce FTAs as observed in 2007. Second, we introduce a hypothetical FTA between EU countries and the USA. Third, we discuss the formation of a hypothetical free trade agreement between EU countries and China. These policy experiments deliver a perspective on environmental aspects of FTA formation. We apply the default method in all scenarios.

Effect of FTAs as observed in 2007. Regional trade integration is a defining feature of the organization of world trade. In 2007, an average sample country was in an FTA with roughly one quarter of all other sample countries. To evaluate the effects of regional trade liberalization on emissions and trade, we create a counterfactual world without free trade agreements and compare it to our benchmark.

Due to the regional reduction of trade barriers with FTA formation, nominal world GDP rises by 1.9%. There is substantial heterogeneity in the gains from trade across continents, see column (4) in the upper panel of Table 5.11. Real GDP increases most in North America and Europe. Canada and Chile benefit most from FTA formation with a real GDP increase of about 16%. But in some African countries like Ethiopia, Madagascar or Senegal, we even predict falling real GDPs.

Table 5.11 also shows average percentage increases in regional CO_2 emissions. Heterogeneous changes in world market shares explain the heterogeneity in emission increases across

[33] The previous section focused on the extent of emission relocation closely linked to carbon leakage. In this Section, we hold climate policy and carbon prices constant, i.e. there is no leakage.

Table 5.11: Region-specific average emission and real GDP changes (in %)

Region	Emission effects			Real
	Total	Technique	Scale	GDP
Policy experiment: From no FTAs to FTAs as observed in 2007				
North America	4.75	4.39	0.33	10.06
Europe	3.87	3.43	0.42	7.95
South America	2.87	2.54	0.31	5.81
Asia	2.07	1.73	0.32	3.91
Africa	1.00	0.86	0.12	1.78
Oceania	0.93	0.88	0.05	1.71
Policy experiment: EU-USA FTA				
EU	1.91	1.70	0.20	1.83
USA	1.90	1.77	0.13	2.18
Asia	1.05	0.88	0.17	-0.05
Non-EU Europe	0.96	0.74	0.23	-0.31
Oceania	0.89	0.85	0.05	-0.36
South America	0.87	0.78	0.09	-0.38
Africa	0.86	0.79	0.07	-0.39
North America	0.60	0.56	0.04	-0.83
Policy experiment: EU-China FTA				
China	1.23	1.06	0.18	2.53
EU	0.25	0.22	0.03	0.58
North America	-0.04	-0.04	-0.00	0.10
South America	-0.06	-0.05	-0.01	-0.09
Oceania	-0.07	-0.06	-0.00	-0.20
Africa	-0.09	-0.08	-0.01	-0.18
Non-EU Europe	-0.13	-0.11	-0.03	-0.20
Asia	-0.14	-0.12	-0.02	-0.22

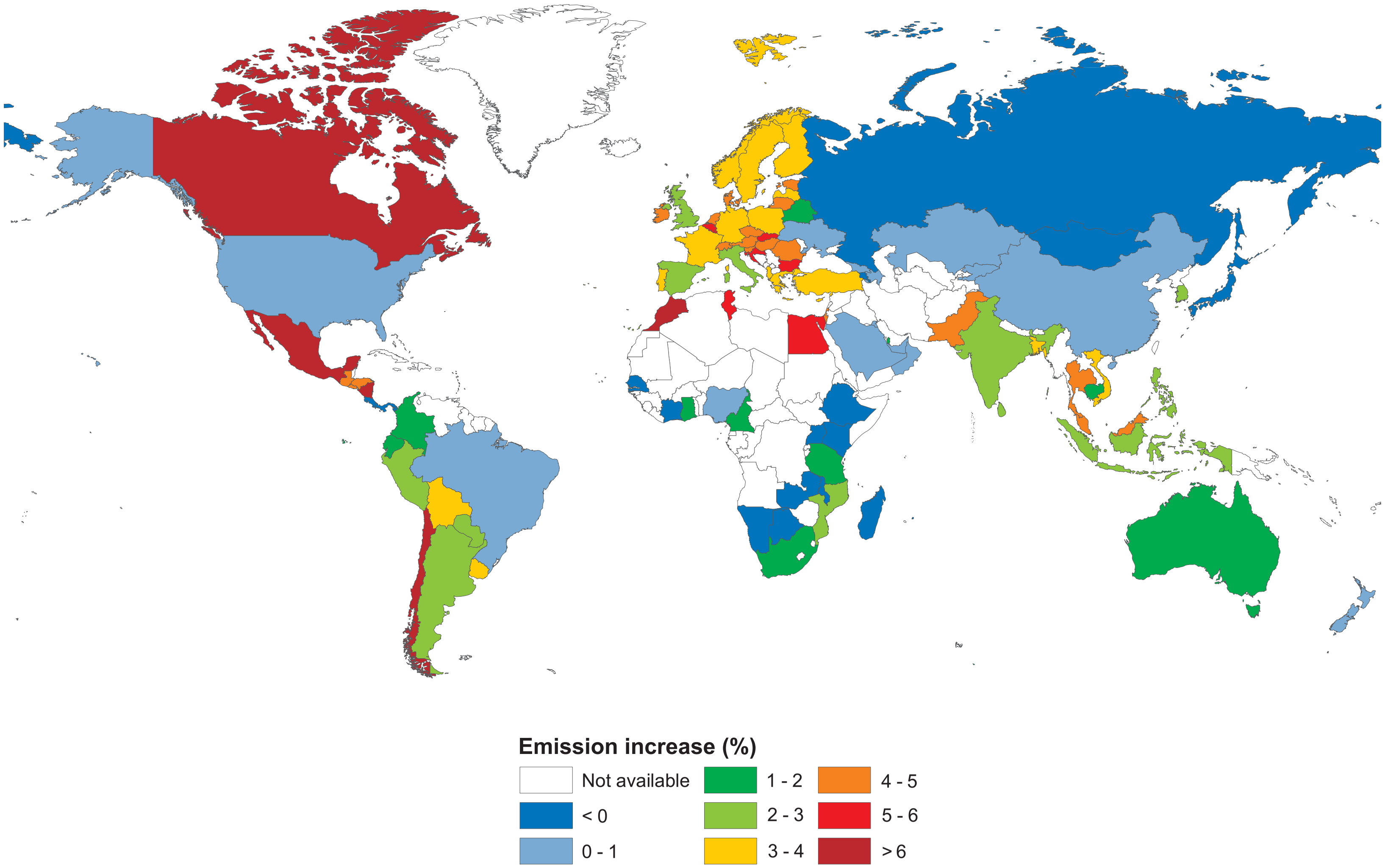
Note: The table shows region-specific changes in emissions and real GDP (in %) from various trade liberalization scenarios.

countries. This is displayed in a world map in Figure 5.6. But even though some countries lose world market shares, the overall increase in world demand overcompensates this negative effect and leads to emission increases in all but a few countries.[34] Again, we can decompose the total effect on emissions in a scale and a technique effect. While energy supply is elastic, other factors are in fixed endowment. This leads to an increase in emission intensity in all regions. The scale effect is largest in Europe. Here, output growth contributes most to emission increases.

Finally, the model predicts trade creation and trade diversion. The bilateral import share of countries signing an FTA goes up by 21.5% on average. Whereas the bilateral import

[34] Without carbon price changes $\Delta E_i = \Delta Y_i = \Delta\theta_i + \Delta Y^w$.

Figure 5.6: Country-specific emission increase from FTAs as of 2007 compared to no FTAs



Note: The graph visualizes country-specific emission increases (in %) due to the policy experiment of a counterfactual switch of a world without FTAs to one with FTAs as observed in 2007 using the default method; i.e. PPML estimation and $\hat{\sigma} = 5.259$.

Table 5.12: Trade share changes (in %) from FTAs as observed in 2007

from/to	Africa	Asia	Europe	North America	Oceania	South America
Africa	19.79	-0.26	-9.90	-14.37	-6.42	-1.27
Asia	1.15	2.39	-16.23	-13.96	-5.92	-2.32
Europe	-7.92	-14.91	10.48	-17.41	-15.76	-19.32
North America	-11.55	-12.57	-17.15	10.31	-8.83	-4.62
Oceania	-5.76	-6.77	-17.96	-11.12	50.95	-12.36
South America	-0.46	-3.23	-20.26	-5.83	-10.85	6.22

Note: The table shows counterfactual percentage changes in trade shares, i.e. $\Delta\frac{X_{ij}}{Y_j}$ where j is the importer and $i \neq j$, from a counterfactual switch of a world without FTAs to one with FTAs as observed in 2007.

share of countries not in an FTA drops by 15.5%. So trade is diverted away towards county pairs engaging in bilateral trade liberalization. Since FTA formation predominantly takes place within continents, we observe an increase in within-continent trade shares (compare Table 5.12). On the other hand, trade between continents is reduced in relative terms.

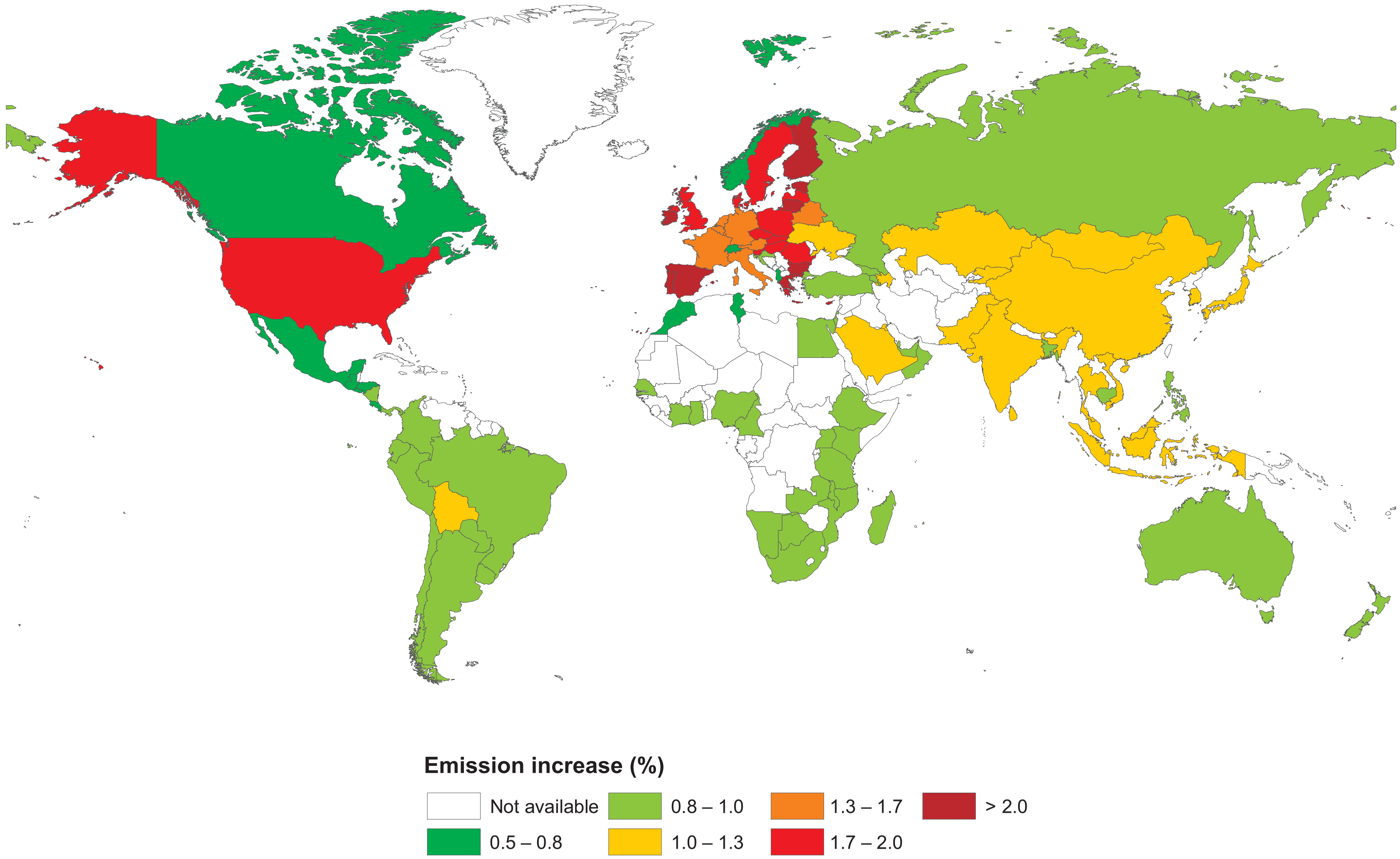
In summary, FTA formation generates gains from trade. But it also generates increases in emissions. These are larger for small countries and concentrate in regions with a lot of regional trade liberalization.

EU-USA free trade agreement. The formation of a transatlantic free trade agreement between the EU and the US is a realistic policy scenario. At the moment, the European Union is assessing effects of such an FTA. Carbon prices between the EU and the US differ substantially.[35] Therefore, the question of its environmental effects merits discussion.

The formation of an EU-USA FTA increases nominal world GDP by 1.5%. Our model predicts gains from trade of 2.18% for the US, and 1.83% for an average EU country. Within the EU, small countries benefit more in terms of real GDP than, e.g., France or Germany. Canada is amongst the biggest losers of such an FTA. Its real GDP falls by almost 1.4%. The model predicts emission increases in most countries. Figure 5.7 shows emission increases are larger for countries farther away from the EU or the US. Table 5.13 shows that the US import share of EU goods goes up by roughly 50%, and vice versa. But also non-members

[35] Compared to most EU countries, the US has a relatively low implicit CO_2 price of 238$ per t of CO_2. The price is lower in some Eastern European countries (Estonia, Slovenia, Poland, Czech Republic, Latvia and Bulgaria). However, the GDP-weighted EU carbon price is 484$ per ton of CO_2, i.e. more than double the US price. The simple average still is 400$ per t of CO_2.

Figure 5.7: Country-specific emission increase from EU-USA free trade agreement



Note: The graph visualizes country-specific emission increases (in %) due to the policy experiment of introducing a transatlantic FTA using the default method; i.e. PPML estimation and $\hat{\sigma} = 5.259$.

Table 5.13: Trade share changes (in %) from EU-USA and EU-China FTA

Policy experiment: EU-USA FTA								
from/to	EU	USA	Africa	Asia	Non-EU Europe	North America	Oceania	South America
EU	-6.62	49.74	-1.93	-2.42	-1.68	-0.84	-2.17	-1.86
USA	50.14	-7.38	-2.28	-2.77	-2.03	-1.18	-2.50	-2.22
Africa	-2.96	-3.57	1.87	1.34	2.14	3.04	1.63	1.90
Asia	-3.23	-3.82	1.58	1.07	1.85	2.74	1.37	1.58
Non-EU Europe	-2.58	-3.17	2.28	1.76	2.55	3.49	2.04	2.29
North America	-2.04	-2.63	2.89	2.35	3.12	4.06	2.62	2.92
Oceania	-3.24	-3.83	1.60	1.09	1.86	2.74	1.32	1.65
South America	-2.89	-3.49	1.90	1.40	2.20	3.10	1.72	2.01
Policy experiment: EU-China FTA								
from/to	EU	China	Africa	Asia	Non-EU Europe	North America	Oceania	South America
EU	-2.31	50.83	-0.67	-0.51	-0.58	-1.08	-0.70	-0.83
China	52.13	-9.98	-4.12	-3.97	-4.03	-4.52	-4.15	-4.29
Africa	-1.03	-5.29	0.62	0.77	0.70	0.21	0.58	0.45
Asia	-0.87	-5.14	0.78	0.95	0.88	0.37	0.75	0.62
Non-EU Europe	-0.94	-5.20	0.72	0.87	0.81	0.31	0.67	0.53
North America	-1.23	-5.48	0.43	0.58	0.52	0.05	0.39	0.24
Oceania	-1.12	-5.37	0.54	0.70	0.63	0.12	0.46	0.36
South America	-1.15	-5.42	0.50	0.63	0.58	0.07	0.46	0.31

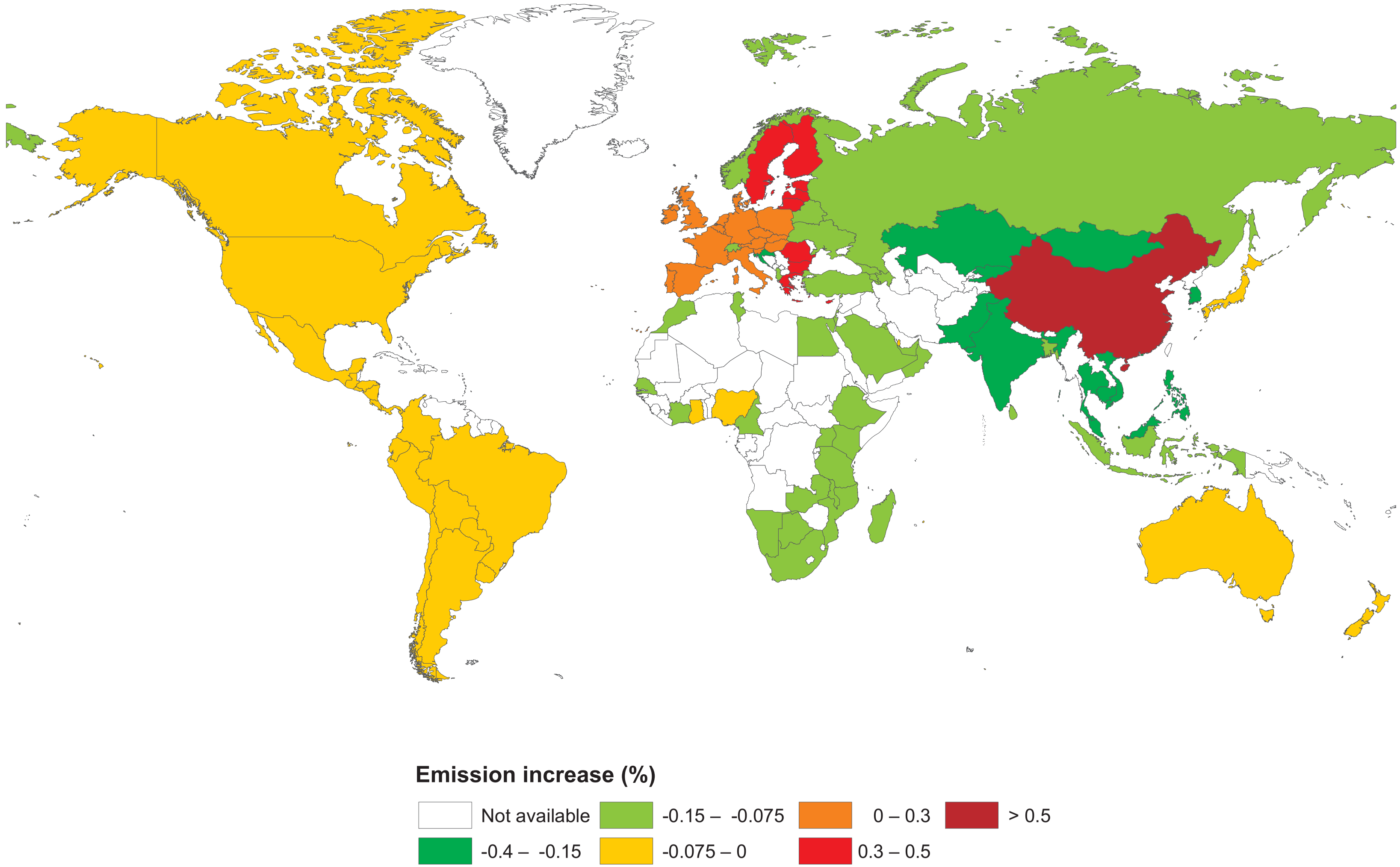
Note: The table shows counterfactual percentage changes in trade shares, i.e. $\Delta\frac{X_{ij}}{Y_j}$ where j is the importer, from two policy experiments: a hypothetical EU-USA free trade agreement and a EU-China FTA, respectively.

start to trade more amongst each other; at the cost of trade shares with the EU and US. The extent of trade diversion is largest within other North American countries.

EU-China free trade agreement. Last, we briefly discuss emission effects of trade liberalization between the EU and China. While the differences in carbon prices are large between EU countries and the US, they are even more pronounced between the EU and China. In our data, the implicit Chinese CO_2 price is about 100$ per t of CO_2 only.

With an EU-China free trade agreement, nominal world GDP rises by 0.16%. China benefits most from the opening up of the EU Single Market for Chinese products. Its gains from trade amount to 2.53% of real GDP. EU countries also experience a small increase of real GDP of 0.58% on average. Emission increases are quite substantial in China (1.23%) while moderate in the EU. They range from 0.07% in the Netherlands to 0.47% in Cyprus.

Figure 5.8: Country-specific emission increase from EU-China free trade agreement



Note: The graph visualizes country-specific emission increases (in %) due to the policy experiment of introducing an FTA between the EU and China using the default method; i.e. PPML estimation and $\hat{\sigma} = 5.259$.

Unlike in the EU-USA FTA scenario, emissions actually go down in other regions. Emissions (and GDP) fall most in other European and other Asian countries. The negative effect of the EU-China FTA on other regions' world market share is not overcompensated by the increase in world GDP. Figure 5.8 visualizes country-specific heterogeneity in emission effects in a world map. The predicted trade creation and trade diversion effects are as expected and heterogeneous across regions.

The most striking difference between the EU-USA and EU-China FTA policy experiment is certainly the predicted emission outcome. While the emission increase is quite similar in the US and the EU in the former experiment, the latter experiment predicts a large increase in Chinese emissions compared to moderate emission increases in the EU. Even though our model only features one sector, which effectively shuts down the composition effect of sectoral specialization, China attracts a lot of emission with trade liberalization. This is in line with the pollution haven hypothesis.

5.5 Conclusions

This paper is the first to quantify emission relocation effects of partial climate policy with a structural gravity model. This approach has two major advantages. First, important model parameters like bilateral trade costs and elasticity of substitution are structurally linked to empirical estimates. Second, previous empirical studies confirm that carbon leakage is empirically relevant. But they are not able to quantify carbon leakage in general equilibrium. Country-specific reactions to climate policy are absorbed in importer and exporter fixed effects in the empirical gravity estimation. A structural gravity model takes these general equilibrium effects via income and third country effects into account and enables quantification of leakage.

We find moderate leakage rates in the range of 2-10% depending on the size of the climate-active region. An EUA permit price of 15 US-$ allows the EU to fulfill its Kyoto target. EU countries increase their import shares from non-EU countries. The induced emission relocation amounts to 10%. The counterfactual emission increase through production relocation in non-EU countries is heterogeneous and governed by proximity to the EU, country size, and relative carbon prices. Results are robust to the econometric estimation procedure chosen. Not accounting for country-specific heterogeneity in factor use leads to a slight underestimation of the extent of leakage. Emission limitations as negotiated for the second period of the Kyoto Protocol lead to 8% of emission relocation. This leakage rate is smaller than in the ETS scenario because with Kyoto II a larger part of the world is constrained.

Carbon leakage has important implications for the design of a future climate policy architecture. Since the predicted leakage rate is moderate but non-negligible, it is even more important to strive for the first best: a global climate deal. Policy efforts should be headed in this direction. If this is politically not feasible, partial climate deals could be designed such that leakage is prevented. The literature discusses several options. Climate policy could target footprints, i.e. the GHG embodied in consumption, instead of domestic emissions (see proposals in Bastianoni et al., 2004; Eder and Narodoslawsky, 1999; Peters, 2008). But this system requires information about each good's country-specific emission intensity and the global production chain. It might be hard to administer. Equivalently, a CO_2 tax on domestic production could be accompanied by carbon-related border tax adjustments for imports and tax exemptions for exports. But the conformity of such measures with rules of the World Trade Organization is an open issue and depends on their exact design (see the discussions in Bhagwati and Mavroidis, 2007; Ismer and Neuhoff, 2007; Goh, 2004; Sindico, 2008).

Some limitations of our policy experiment simulations merit discussion. First, we look at emissions as input directly. In reality, climate policy often does not impose a single carbon price but affects different energy inputs differently. We abstract from fuel substitution in our model. Second, we assume a regionally disintegrated energy market with horizontal energy supply. Consequently, emissions react very flexibly to carbon price changes. Supply-side carbon leakage is ruled out by assumption. A model with vertical energy supply has a leakage rate of 100%. Still, it might be interesting to contrast the production relocation effects of such a world with our findings. Third, our analysis focuses on aggregate output. Sectoral adjustments in production are disregarded. However, due to sectoral differences in energy intensity, degree of product differentiation and trade costs one expects heterogeneous relocation and trade responses to climate policy at the sectoral level. This is an avenue for future research. Last, the estimated emission relocation effects do not factor in emissions at upstream parts of the production chain. There are no intermediate inputs. And free-riding and supply-side leakage are ruled out. Thus, the predicted leakage rates might be interpreted as a lower bound of the true effect.

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Appendix A

Appendix to Chapter 1

Table A.1: Country and sample information

ISO3	Country	Emission growth (in%)	Kyoto (year)	ICC (year)	Sample: Rich	Large	OPEC	EIT
AGO	Angola	91	n.a.	n.a.	0	1	1	0
ALB	Albania	66	n.a.	2003	0	0	0	0
ARE	United Arab Emirates	30	n.a.	n.a.	1	0	1	0
ARG	Argentina	18	n.a.	2001	1	1	0	0
ARM	Armenia	28	n.a.	n.a.	1	0	0	0
AUS	Australia	8	2007	2002	1	1	0	0
AUT	Austria	13	2002	2000	1	1	0	0
AZE	Azerbaijan	11	n.a.	n.a.	1	1	0	0
BDI	Burundi	-55	n.a.	2004	0	1	0	0
BEL	Belgium	-8	2002	2000	1	1	0	0
BEN	Benin	81	n.a.	2002	0	1	0	0
BFA	Burkina Faso	36	n.a.	2004	0	1	0	0
BGD	Bangladesh	44	n.a.	n.a.	0	1	0	0
BGR	Bulgaria	2	2002	2002	1	1	0	0
BLR	Belarus	10	2005	n.a.	1	1	0	0
BOL	Bolivia	19	n.a.	2002	0	1	0	0
BRA	Brazil	9	n.a.	2002	1	1	0	0
BTN	Bhutan	32	n.a.	n.a.	0	0	0	0
BWA	Botswana	23	n.a.	2000	1	0	0	0
CAN	Canada	11	2002	2000	1	1	0	0
CHL	Chile	12	n.a.	n.a.	1	1	0	0
CHN	China	55	n.a.	n.a.	1	1	0	0
CIV	Cote d'Ivoire	3	n.a.	n.a.	0	1	0	0
CMR	Cameroon	33	n.a.	n.a.	0	1	0	0
COG	Congo, Rep.	27	n.a.	2004	0	0	0	0
COL	Colombia	-3	n.a.	2002	1	1	0	0
COM	Comoros	40	n.a.	2006	0	0	0	0
CRI	Costa Rica	32	n.a.	2001	1	0	0	0
CUB	Cuba	3	n.a.	n.a.	1	1	0	0
CYP	Cyprus	16	n.a.	2002	1	0	0	0
CZE	Czech Republic	-1	2001	n.a.	1	1	0	1

continued...

Table A.1: Country and sample information continued

ISO3	Country	Emission growth (in%)	Kyoto (year)	ICC (year)	Sample Rich	Large	OPEC	EIT
DEU	Germany	-6	2002	2000	1	1	0	1
DJI	Djibouti	12	n.a.	2002	0	0	0	0
DNK	Denmark	-5	2002	2001	1	1	0	0
DOM	Dominican Republic	6	n.a.	2005	1	1	0	0
DZA	Algeria	22	n.a.	n.a.	0	1	1	0
EGY	Egypt, Arab Rep.	30	n.a.	n.a.	0	1	0	0
ERI	Eritrea	12	n.a.	n.a.	0	1	0	0
ESP	Spain	23	2002	2000	1	1	0	0
EST	Estonia	9	2002	2002	1	0	0	1
ETH	Ethiopia	16	n.a.	n.a.	0	1	0	0
FIN	Finland	11	2002	2000	1	1	0	0
FJI	Fiji	72	n.a.	1999	0	0	0	0
FRA	France	1	2002	2000	1	1	0	0
GAB	Gabon	-10	n.a.	2000	1	0	0	0
GBR	United Kingdom	-0	2002	2001	1	1	0	0
GEO	Georgia	10	n.a.	2003	1	0	0	0
GHA	Ghana	26	n.a.	1999	0	1	0	0
GIN	Guinea	7	n.a.	2003	0	1	0	0
GMB	Gambia, The	34	n.a.	2002	0	0	0	0
GNQ	Equatorial Guinea	261	n.a.	n.a.	1	0	0	0
GRC	Greece	11	2002	2002	1	1	0	0
GTM	Guatemala	32	n.a.	n.a.	0	1	0	0
GUY	Guyana	-9	n.a.	2004	0	0	0	0
HND	Honduras	51	n.a.	2002	0	1	0	0
HRV	Croatia	15	2007	2001	1	0	0	0
HUN	Hungary	-4	2002	2001	1	1	0	1
IDN	Indonesia	36	n.a.	n.a.	0	1	0	0
IND	India	28	n.a.	n.a.	0	1	0	0
IRL	Ireland	11	2002	2002	1	0	0	0
IRN	Iran, Islamic Rep.	37	n.a.	n.a.	1	1	1	0
ITA	Italy	5	2002	1999	1	1	0	0
JAM	Jamaica	15	n.a.	n.a.	1	0	0	0
JOR	Jordan	34	n.a.	2002	0	1	0	0
JPN	Japan	1	2002	2007	1	1	0	0
KAZ	Kazakhstan	43	n.a.	n.a.	1	1	0	0
KEN	Kenya	15	n.a.	2005	0	1	0	0
KGZ	Kyrgyz Republic	9	n.a.	n.a.	0	1	0	0
KHM	Cambodia	61	n.a.	2002	0	1	0	0
KOR	Korea, Rep.	16	n.a.	2002	1	1	0	0
LAO	Lao PDR	50	n.a.	n.a.	0	1	0	0
LBR	Liberia	59	n.a.	2006	0	0	0	0
LKA	Sri Lanka	34	n.a.	n.a.	0	1	0	0
LTU	Lithuania	0	2003	2003	1	0	0	1
LVA	Latvia	2	2002	2002	1	0	0	1
MAR	Morocco	29	n.a.	n.a.	0	1	0	0
MDA	Moldova	-11	n.a.	n.a.	0	0	0	0
MDG	Madagascar	7	n.a.	n.a.	0	1	0	0
MEX	Mexico	14	n.a.	2005	1	1	0	0
MKD	Macedonia, FYR	-5	n.a.	2002	0	0	0	0
MLI	Mali	7	n.a.	2000	0	1	0	0
MNG	Mongolia	20	n.a.	2002	0	0	0	0
MOZ	Mozambique	56	n.a.	n.a.	0	1	0	0
MRT	Mauritania	1	n.a.	n.a.	0	0	0	0

continued...

Table A.1: Country and sample information continued

ISO3	Country	Emission growth	Kyoto	ICC	Sample			
		(in%)	(year)	(year)	Rich	Large	OPEC	EIT
MUS	Mauritius	41	n.a.	2002	1	0	0	0
MWI	Malawi	14	n.a.	2002	0	1	0	0
MYS	Malaysia	43	n.a.	n.a.	1	1	0	0
NAM	Namibia	40	n.a.	2002	0	0	0	0
NIC	Nicaragua	19	n.a.	n.a.	0	1	0	0
NLD	Netherlands	1	2002	2001	1	1	0	0
NOR	Norway	22	2002	2000	1	0	0	0
NPL	Nepal	11	n.a.	n.a.	0	1	0	0
NZL	New Zealand	6	2002	2000	1	0	0	0
PAK	Pakistan	36	n.a.	n.a.	0	1	0	0
PAN	Panama	11	n.a.	2002	1	0	0	0
PER	Peru	25	n.a.	2001	0	1	0	0
PHL	Philippines	-2	n.a.	n.a.	0	1	0	0
PNG	Papua New Guinea	48	n.a.	n.a.	0	1	0	0
POL	Poland	-4	2002	2001	1	1	0	1
PRT	Portugal	2	2002	2002	1	1	0	0
PRY	Paraguay	-5	n.a.	2001	0	1	0	0
ROM	Romania	-4	2001	2002	1	1	0	1
RWA	Rwanda	5	n.a.	n.a.	0	1	0	0
SAU	Saudi Arabia	46	n.a.	n.a.	1	1	1	0
SDN	Sudan	75	n.a.	n.a.	0	1	0	0
SEN	Senegal	37	n.a.	1999	0	1	0	0
SGP	Singapore	-7	n.a.	n.a.	1	0	0	0
SLB	Solomon Islands	12	n.a.	n.a.	0	0	0	0
SLV	El Salvador	11	n.a.	n.a.	0	1	0	0
SVK	Slovak Republic	-5	2002	2002	1	1	0	1
SVN	Slovenia	-1	2002	2001	1	0	0	1
SWE	Sweden	-3	2002	2001	1	1	0	0
SWZ	Swaziland	-16	n.a.	n.a.	0	0	0	0
SYR	Syrian Arab Rep.	13	n.a.	n.a.	0	1	0	0
TCD	Chad	110	n.a.	2006	0	1	0	0
TGO	Togo	4	n.a.	n.a.	0	1	0	0
THA	Thailand	32	n.a.	n.a.	1	1	0	0
TJK	Tajikistan	20	n.a.	2000	0	1	0	0
TKM	Turkmenistan	30	n.a.	n.a.	1	0	0	0
TTO	Trinidad & Tobago	45	n.a.	1999	1	0	0	0
TUN	Tunisia	23	n.a.	n.a.	1	1	0	0
TUR	Turkey	21	n.a.	n.a.	1	1	0	0
TZA	Tanzania	67	n.a.	2002	0	1	0	0
UGA	Uganda	62	n.a.	2002	0	1	0	0
UKR	Ukraine	2	2004	n.a.	1	1	0	1
URY	Uruguay	7	n.a.	2002	1	0	0	0
USA	United States	4	n.a.	n.a.	1	1	0	0
UZB	Uzbekistan	1	n.a.	n.a.	0	1	0	0
VEN	Venezuela, RB	6	n.a.	2000	1	1	1	0
VNM	Vietnam	77	n.a.	n.a.	0	1	0	0
ZAF	South Africa	11	n.a.	2000	1	1	0	0
ZMB	Zambia	17	n.a.	2002	0	1	0	0
ZWE	Zimbabwe	-36	n.a.	n.a.	0	1	0	0

Note: Emission growth is difference between pre- and post-treatment (i.e. 1997-2000 and 2004-2007, respectively) average of log CO_2 emissions. Kyoto shows the ratification year of Kyoto commitment, ICC shows the ratification year of the Rome Statutes of the ICC. EIT is a dummy for economies in transition.

Table A.2: Robustness checks: IV estimates on CO_2 emissions – Full results

	(A) Alternative Samples			
Sample:	w/o EIT (A1)	w/o OPEC (A2)	rich only (A3)	large only (A4)
Kyoto (0,1)	-0.08	-0.10**	-0.11***	-0.09
	(0.06)	(0.05)	(0.04)	(0.06)
Kyoto, spatial lag	0.02***	0.02***	0.03	0.02***
	(0.00)	(0.00)	(0.02)	(0.00)
Ln GDP	0.62	0.55	0.75	-0.45
	(0.51)	(0.52)	(0.70)	(0.58)
Ln GDP, squared	-0.00	0.00	-0.00	0.03*
	(0.01)	(0.01)	(0.02)	(0.01)
Ln population	0.93***	1.02***	0.55*	1.38***
	(0.28)	(0.30)	(0.33)	(0.39)
Ln manufacturing (% of GDP)	0.13	0.12	0.32**	-0.03
	(0.10)	(0.10)	(0.15)	(0.07)
Ln agriculture (% of GDP)	-0.10**	-0.11**	-0.05	-0.12**
	(0.05)	(0.05)	(0.05)	(0.06)
Ln services (% of GDP)	-0.10	-0.07	-0.03	-0.14
	(0.09)	(0.11)	(0.21)	(0.12)
Ln stock of other IEA	-0.17**	-0.15**	-0.21***	-0.19**
	(0.08)	(0.07)	(0.06)	(0.08)
Government orientation (0.1,0.2,0.3)	0.16**	0.14**	0.06	0.13*
	(0.07)	(0.06)	(0.06)	(0.07)
Openness, (Exp+Imp)/GDP	-0.01	0.02	-0.25***	0.09
	(0.07)	(0.07)	(0.07)	(0.08)
WTO (0,1)	0.01	-0.00	-0.02	0.01
	(0.05)	(0.04)	(0.05)	(0.04)
Polity (-1 to 1)	-0.00	-0.00	-0.01*	-0.00
	(0.00)	(0.00)	(0.01)	(0.00)
No. of observations	1,298	1,354	720	1,007
No. of countries	122	127	67	94
First-stage diagnostics:				
Over-ID test (p-value)	0.42	0.67	0.10	0.07
Weak-ID test (F-stat)	22.56	18.18	8.30	16.28
Second-stage diagnostics:				
adj. R^2	0.43	0.41	0.43	0.51

Note: Dependent variable is ln CO_2 emissions. Fixed-effects estimation. All regressions use year dummies and constant (not shown). Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Instruments for Kyoto: membership in ICC and its spatial lag.

Table A.2: Robustness checks: IV estimates on CO_2 emissions – Full results cont.

	(B) Alternative IV Strategy		(C) Kyoto definition	(D) Alternative treatment window	
	LIML (B1)	GMM (B2)	Stringency (C)	narrow (D1)	broad (D2)
Kyoto (0,1)	-0.10**	-0.10**	-0.05**	-0.12**	-0.17**
	(0.05)	(0.04)	(0.02)	(0.06)	(0.07)
Kyoto, spatial lag	0.02***	0.02***	0.02***	0.04***	0.04***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Ln GDP	0.62	0.58	0.66	0.85	0.63
	(0.50)	(0.50)	(0.51)	(0.57)	(0.51)
Ln GDP, squared	-0.00	-0.00	-0.00	-0.01	-0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Ln population	0.95***	0.98***	0.99***	1.01***	1.04***
	(0.26)	(0.26)	(0.25)	(0.30)	(0.26)
Ln manufacturing (% of GDP)	0.12	0.12	0.12	0.15	0.11
	(0.09)	(0.09)	(0.09)	(0.11)	(0.08)
Ln agriculture (% of GDP)	-0.10**	-0.09**	-0.09**	-0.22***	-0.18**
	(0.04)	(0.04)	(0.04)	(0.08)	(0.09)
Ln services (% of GDP)	-0.10	-0.10	-0.10	-0.24	-0.09
	(0.09)	(0.09)	(0.09)	(0.16)	(0.14)
Ln stock of other IEA	-0.18***	-0.17**	-0.17**	-0.17*	-0.24**
	(0.07)	(0.07)	(0.07)	(0.10)	(0.10)
Government orientation	0.15**	0.14**	0.14**	0.19	0.26*
	(0.06)	(0.06)	(0.06)	(0.16)	(0.15)
Openness, (Exp+Imp)/GDP	-0.02	-0.02	-0.00	-0.12	-0.06
	(0.07)	(0.07)	(0.07)	(0.13)	(0.13)
WTO (0,1)	0.00	0.00	0.00	0.03	0.06
	(0.04)	(0.04)	(0.04)	(0.06)	(0.06)
Polity (-1 to 1)	-0.00	-0.00	-0.00	-0.01	-0.01
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
No. of observations	1,418	1,418	1,418	266	258
No. of countries	133	133	133	133	129
First-stage diagnostics:					
Over-ID test (p-value)	0.44	0.44	0.41	0.85	0.82
Weak-ID test (F-stat)	19.09	19.09	21.94	34.06	37.47
Second-stage diagnostics:					
adj. R^2	0.42	0.42	0.42	0.38	0.32

Note: Dependent variable is ln CO_2 emissions. Fixed-effects estimation. All regressions use year dummies and constant (not shown). In Panel (D) long differences-in-differences. Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Instruments for Kyoto: membership in ICC and its spatial lag.

Table A.3: Through which channels does Kyoto operate? Full results

Panel:	(A) Shares in energy use				(B) Shares in electricity production			
Dep. Var.:	Renewables		Fossil fuel		Coal		Alternative energy	
Method:	FE-OLS	FE-IV	FE-OLS	FE-IV	FE-OLS	FE-IV	FE-OLS	FE-IV
Kyoto (0,1)	1.38***	2.41***	-0.67	-2.46**	0.12	-1.43	1.07***	1.66***
	(0.53)	(0.93)	(0.60)	(1.16)	(0.87)	(1.76)	(0.25)	(0.56)
Kyoto, spatial lag	-0.58***	-0.62***	0.52**	0.61***	-0.22	-0.14	0.03	-0.00
	(0.17)	(0.15)	(0.21)	(0.16)	(0.18)	(0.11)	(0.05)	(0.03)
Ln GDP	-1.16	-0.02	-1.18	-3.15	-7.62	-9.33	-0.30	0.35
	(9.37)	(9.24)	(10.33)	(10.32)	(9.92)	(10.36)	(3.19)	(3.37)
Ln GDP, squared	-0.15	-0.17	0.25	0.29	0.36	0.40	-0.05	-0.06
	(0.26)	(0.26)	(0.28)	(0.28)	(0.27)	(0.28)	(0.09)	(0.10)
Ln population	-16.89**	-13.31*	16.78**	10.58	7.14	1.77	0.12	2.18
	(6.99)	(7.80)	(6.99)	(8.05)	(5.23)	(7.93)	(1.79)	(2.59)
Ln manufacturing (% of GDP)	-1.34	-1.35	-0.28	-0.26	2.41**	2.43**	0.39	0.39
	(1.17)	(1.14)	(1.36)	(1.30)	(1.22)	(1.21)	(0.48)	(0.46)
Ln agriculture (% of GDP)	1.58	1.76	-2.00	-2.31*	0.76	0.49	-0.26	-0.16
	(1.20)	(1.17)	(1.26)	(1.24)	(1.08)	(1.12)	(0.34)	(0.36)
Ln services (% of GDP)	-0.77	-0.74	1.69	1.64	-1.14	-1.19	0.15	0.17
	(2.37)	(2.40)	(2.57)	(2.62)	(1.09)	(1.13)	(0.49)	(0.49)
Ln stock of other IEA	1.47	1.40	-2.05	-1.94	-1.46	-1.36	-0.57	-0.61
	(1.08)	(1.06)	(1.36)	(1.37)	(1.27)	(1.29)	(0.44)	(0.43)
Government orientation (0.1,0.2,0.3)	0.01	0.01	3.60**	3.59**	0.17	0.16	-0.14	-0.13
	(1.60)	(1.61)	(1.80)	(1.82)	(2.41)	(2.43)	(1.25)	(1.24)
Openness, (Exp+Imp)/GDP	-0.18	0.07	0.14	-0.30	-1.94	-2.32*	0.19	0.34
	(1.21)	(1.17)	(1.25)	(1.25)	(1.23)	(1.40)	(0.36)	(0.42)
WTO (0,1)	-0.53	-0.36	0.31	0.01	-0.06	-0.33	-0.42**	-0.32
	(0.66)	(0.67)	(0.96)	(0.99)	(0.88)	(1.00)	(0.19)	(0.22)
Polity (-1 to 1)	-0.11*	-0.11*	0.07	0.07	0.17	0.17	0.06	0.06
	(0.06)	(0.06)	(0.07)	(0.07)	(0.11)	(0.11)	(0.05)	(0.05)
No. of observations (countries)	1,180 (110)	1,180 (110)	1,180 (110)	1,180 (110)	1,180 (110)	1,180 (110)	1,180 (110)	1,180 (110)
Over-ID test (p-value)		0.68		0.47		0.60		0.63
Weak-ID test (F-stat)		18.80		18.80		18.80		18.80
adj. R^2	0.22	0.21	0.08	0.06	-0.04	-0.05	0.09	0.08

Note: Fixed-effects estimation. All regressions use year dummies and constant (not shown). Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. * p<0.1,** p<0.05, *** p<0.01. Instruments for Kyoto: membership in ICC and its spatial lag.

Table A.3: Through which channels does Kyoto operate? Full results continued

Panel:	(C) Pump prices (USD/l)				(D) Log per capita use of			
Dep. Var.:	Diesel fuel		Gasoline		Energy		Electricity	
Method:	FE-OLS	FE-IV	FE-OLS	FE-IV	FE-OLS	FE-IV	FE-OLS	FE-IV
Kyoto (0,1)	0.10***	0.22***	0.13***	0.25***	-0.05***	-0.05*	-0.04	-0.08**
	(0.03)	(0.05)	(0.03)	(0.05)	(0.02)	(0.03)	(0.03)	(0.04)
Kyoto, spatial lag	0.01**	0.01	0.01	0.00	-0.00	-0.00	-0.00	0.00
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)
Ln GDP	1.23***	1.30***	0.55	0.62	-0.56*	-0.57*	0.04	-0.00
	(0.45)	(0.46)	(0.53)	(0.55)	(0.30)	(0.30)	(0.98)	(0.96)
Ln GDP, squared	-0.04***	-0.04***	-0.01	-0.02	0.02**	0.02**	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Ln population	-0.45*	-0.01	0.12	0.56	-0.33**	-0.34*	0.58*	0.44
	(0.24)	(0.28)	(0.29)	(0.35)	(0.16)	(0.19)	(0.30)	(0.29)
Ln manufacturing (% of GDP)	0.03	0.04	-0.02	-0.01	0.03	0.03	0.20*	0.20*
	(0.05)	(0.05)	(0.07)	(0.06)	(0.04)	(0.04)	(0.12)	(0.12)
Ln agriculture (% of GDP)	0.03	0.05	0.04	0.06	-0.05	-0.05	-0.01	-0.01
	(0.05)	(0.05)	(0.08)	(0.08)	(0.03)	(0.03)	(0.04)	(0.04)
Ln services (% of GDP)	0.22***	0.22***	0.28***	0.28***	-0.13*	-0.13*	0.07	0.07
	(0.08)	(0.08)	(0.10)	(0.09)	(0.07)	(0.07)	(0.08)	(0.08)
Ln stock of other IEA	0.07	0.06	0.11	0.10	-0.02	-0.02	-0.16	-0.16
	(0.06)	(0.06)	(0.13)	(0.13)	(0.05)	(0.05)	(0.11)	(0.11)
Government orientation (0.1,0.2,0.3)	-0.17	-0.17	-0.21	-0.20	-0.01	-0.01	0.10	0.10
	(0.11)	(0.11)	(0.14)	(0.14)	(0.05)	(0.05)	(0.08)	(0.08)
Openness, (Exp+Imp)/GDP	-0.03	-0.01	-0.10	-0.08	0.06*	0.06*	0.12	0.11
	(0.06)	(0.06)	(0.09)	(0.09)	(0.03)	(0.03)	(0.10)	(0.09)
WTO (0,1)	0.01	0.02	0.00	0.02	0.03	0.03	0.08*	0.08*
	(0.03)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)	(0.04)	(0.04)
Polity (-1 to 1)	0.00	0.00	0.00	0.00	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
No. of observations (countries)	608 (127)	608 (127)	608 (127)	608 (127)	1,180 (110)	1,180 (110)	1,169 (109)	1,169 (109)
Over-ID test (p-value)		0.61		0.32		0.36		0.21
Weak-ID test (F-stat)		20.30		20.30		18.80		18.84
adj. R^2	0.71	0.69	0.60	0.58	0.31	0.31	0.47	0.47

Note: Fixed-effects estimation. All regressions use year dummies and constant (not shown). Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Instruments for Kyoto: membership in ICC and its spatial lag.

Table A.4: Robustness checks: Channels. IV estimates – Full results

	(A) Shares in energy use					
Dep. Var.:	Renewables			Fossil fuel		
Method/Sample:	long-FE	w/o EIT	rich only	long-FE	w/o EIT	rich only
	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)
Kyoto (0,1)	3.43***	2.85***	2.05**	-3.62***	-2.16*	-0.58
	(1.14)	(1.10)	(0.85)	(1.37)	(1.17)	(1.13)
Kyoto, spatial lag	-0.81***	-0.66***	-0.07	0.81***	0.69***	-0.79**
	(0.21)	(0.14)	(0.22)	(0.22)	(0.11)	(0.40)
Ln GDP	4.40	1.28	5.33	-8.89	-5.87	0.06
	(9.35)	(9.45)	(16.52)	(9.74)	(10.55)	(18.08)
Ln GDP, squared	-0.29	-0.20	-0.24	0.41	0.35	0.16
	(0.26)	(0.26)	(0.43)	(0.28)	(0.29)	(0.45)
Ln population	-7.24	-12.56	-0.82	2.93	11.35	1.72
	(6.32)	(8.53)	(5.22)	(6.65)	(8.71)	(6.18)
Ln manufact. share	-3.06	-1.70	1.31	4.25	-0.03	-1.83
	(2.15)	(1.28)	(1.86)	(3.13)	(1.49)	(1.96)
Ln agricult. share	2.09	2.07	0.58	-3.55*	-2.57*	-0.08
	(1.79)	(1.27)	(0.78)	(1.90)	(1.33)	(0.89)
Ln services share	0.44	-0.67	-3.37	-0.44	1.67	2.30
	(4.47)	(2.45)	(2.45)	(4.67)	(2.68)	(2.75)
Ln IEA stock	1.71	1.43	1.20	-1.70	-1.76	-2.73
	(1.55)	(1.24)	(1.52)	(2.04)	(1.64)	(2.15)
Governm. orientation	1.11	0.79	-0.71	2.19	2.95	4.45***
	(4.85)	(1.90)	(1.31)	(5.19)	(2.09)	(1.70)
Openness	2.48	0.23	0.48	-4.66*	0.02	-0.17
	(2.45)	(1.27)	(0.83)	(2.57)	(1.29)	(1.04)
WTO (0,1)	-0.40	-0.47	-0.32	0.19	0.05	-0.15
	(1.09)	(0.81)	(0.45)	(1.34)	(1.20)	(0.54)
Polity (-1 to 1)	-0.16	-0.11*	-0.02	0.13	0.07	0.03
	(0.12)	(0.06)	(0.04)	(0.14)	(0.07)	(0.06)
No. of observations	220	1,060	701	220	1,060	701
No. of countries	110	99	65	110	99	65
Over-ID test (p-value)	0.32	0.67	0.72	0.18	0.81	0.58
Weak-ID test (F-stat)	36.49	22.07	8.39	36.49	22.07	8.39
adj. R^2	-0.32	0.21	0.05	-0.58	0.07	0.02

Note: Fixed-effects IV estimates. Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. All regressions use year dummies and constant (not shown). * p<0.1, ** p<0.05, *** p<0.01. Instruments for Kyoto: membership in ICC and its spatial lag.

Table A.4: Robustness checks: Channels. IV estimates – Full results continued

	(B) Shares in electricity production					
Dep. Var.:	Alternative			Coal		
Method/Sample:	long-FE	w/o EIT	rich only	long-FE	w/o EIT	rich only
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)
Kyoto (0,1)	3.06***	1.93***	1.69***	-0.96	-0.47	-0.74
	(0.58)	(0.53)	(0.54)	(1.41)	(2.46)	(1.30)
Kyoto, spatial lag	-0.05	-0.00	0.12	-0.14	-0.06	-1.32*
	(0.06)	(0.03)	(0.16)	(0.11)	(0.05)	(0.69)
Ln GDP	3.21	0.55	-2.09	-11.34	-9.68	-7.10
	(4.52)	(3.26)	(5.76)	(12.82)	(11.10)	(13.14)
Ln GDP, squared	-0.12	-0.06	-0.00	0.48	0.41	0.21
	(0.13)	(0.09)	(0.14)	(0.34)	(0.29)	(0.34)
Ln population	8.35***	1.62	1.16	1.89	4.26	2.29
	(2.18)	(2.53)	(3.56)	(4.27)	(7.71)	(8.60)
Ln manufact. share	-0.28	0.38	0.63	6.66**	2.46*	2.06
	(0.82)	(0.50)	(0.52)	(2.59)	(1.36)	(1.29)
Ln agricult. share	0.20	-0.06	-0.34	0.36	0.92	1.51
	(0.72)	(0.37)	(0.47)	(2.14)	(1.26)	(1.38)
Ln services share	0.16	0.34	-0.16	-2.98	-1.56	0.07
	(1.08)	(0.46)	(0.94)	(2.13)	(1.05)	(1.95)
Ln IEA stock	-1.19*	-0.22	-0.95	-1.07	-1.51	0.88
	(0.69)	(0.42)	(0.76)	(2.11)	(1.32)	(2.42)
Governm. orientation	-0.91	0.23	-0.26	2.62	-1.08	1.44
	(2.54)	(1.42)	(1.60)	(5.91)	(2.57)	(3.35)
Openness	1.19	0.36	0.03	-7.08**	-1.50	-2.28
	(0.87)	(0.43)	(0.66)	(3.00)	(1.44)	(1.62)
WTO (0,1)	0.19	-0.38	0.00	-0.92	-0.32	0.88
	(0.43)	(0.24)	(0.33)	(1.30)	(1.17)	(1.14)
Polity (-1 to 1)	0.09	0.06	-0.03	0.27	0.17	0.37
	(0.09)	(0.05)	(0.02)	(0.17)	(0.11)	(0.24)
No. of observations	220	1,060	701	220	1,060	701
No. of countries	110	99	65	110	99	65
Over-ID test (p-value)	0.60	0.55	0.30	0.62	0.74	0.92
Weak-ID test (F-stat)	36.49	22.07	8.39	36.49	22.07	8.39
adj. R^2	-0.47	0.06	0.21	-0.74	-0.05	-0.05

Note: Fixed-effects IV estimates. Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. All regressions use year dummies and constant (not shown). * p<0.1, ** p<0.05, *** p<0.01. Instruments for Kyoto: membership in ICC and its spatial lag.

Table A.4: Robustness checks: Channels. IV estimates – Full results continued

	(C) Pump prices					
Dep. Var.:		Diesel fuel			Gasoline	
Method/Sample:	long-FE	w/o EIT	rich only	long-FE	w/o EIT	rich only
	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)
Kyoto (0,1)	0.49***	0.16***	0.31***	0.46***	0.22***	0.32***
	(0.06)	(0.06)	(0.08)	(0.06)	(0.07)	(0.09)
Kyoto, spatial lag	0.01	0.01	-0.07*	0.00	0.00	-0.06*
	(0.01)	(0.00)	(0.04)	(0.01)	(0.01)	(0.03)
Ln GDP	0.98*	1.21**	0.67	0.55	0.50	0.28
	(0.54)	(0.48)	(0.71)	(0.60)	(0.55)	(0.78)
Ln GDP, squared	-0.02	-0.03**	-0.02	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
Ln population	1.17***	0.03	0.07	1.34***	0.56	0.44
	(0.23)	(0.28)	(0.45)	(0.26)	(0.35)	(0.59)
Ln manufact. share	-0.00	-0.01	0.00	-0.07	-0.05	-0.02
	(0.10)	(0.05)	(0.08)	(0.10)	(0.07)	(0.09)
Ln agricult. share	0.06	0.05	0.14**	0.03	0.06	0.05
	(0.08)	(0.05)	(0.06)	(0.10)	(0.08)	(0.10)
Ln services share	0.18	0.23***	0.48***	0.20	0.28***	0.51***
	(0.12)	(0.08)	(0.16)	(0.14)	(0.09)	(0.18)
Ln IEA stock	0.11	0.03	0.24*	0.05	0.09	0.12
	(0.10)	(0.07)	(0.13)	(0.13)	(0.14)	(0.12)
Governm. orientation	-0.25	-0.19	-0.21*	-0.24	-0.23	-0.26*
	(0.23)	(0.13)	(0.13)	(0.21)	(0.16)	(0.14)
Openness	0.13	-0.02	-0.06	0.05	-0.09	-0.11
	(0.09)	(0.06)	(0.08)	(0.13)	(0.09)	(0.08)
WTO (0,1)	0.14*	0.01	0.05	0.14*	0.02	0.06
	(0.07)	(0.04)	(0.04)	(0.08)	(0.05)	(0.05)
Polity (-1 to 1)	0.01**	0.00	0.01*	0.01	0.00	0.01
	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
No. of observations	252	554	316	252	554	316
No. of countries	126	116	65	126	116	65
Over-ID test (p-value)	0.04	0.37	0.39	0.47	0.47	0.04
Weak-ID test (F-stat)	39.06	21.99	8.83	39.06	21.99	8.83
adj. R^2	0.47	0.66	0.67	0.40	0.55	0.60

Note: Fixed-effects IV estimates. Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. All regressions use year dummies and constant (not shown). * p<0.1, ** p<0.05, *** p<0.01. Instruments for Kyoto: membership in ICC and its spatial lag.

Table A.4: Robustness checks: Channels. IV estimates – Full results continued

	(D) Per capita use of					
Dep. Var.:		Energy			Electricity	
Method/Sample:	long-FE (D1)	w/o EIT (D2)	rich only (D3)	long-FE (D4)	w/o EIT (D5)	rich only (D6)
Kyoto (0,1)	-0.06	-0.05	-0.06*	-0.02	-0.08*	-0.06
	(0.04)	(0.04)	(0.03)	(0.06)	(0.05)	(0.04)
Kyoto, spatial lag	0.00	-0.00	0.01	0.00	0.00	-0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Ln GDP	-0.73***	-0.64**	-0.47	-0.21	-0.07	-1.02
	(0.27)	(0.29)	(0.38)	(0.94)	(0.99)	(0.64)
Ln GDP, squared	0.03***	0.02***	0.02**	0.02	0.01	0.04**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Ln population	-0.25	-0.39*	-0.38	0.94***	0.49	0.26
	(0.20)	(0.21)	(0.24)	(0.25)	(0.32)	(0.24)
Ln manufact. share	0.03	0.04	0.05	0.23	0.22*	0.00
	(0.07)	(0.04)	(0.05)	(0.15)	(0.13)	(0.05)
Ln agricult. share	-0.10*	-0.05	-0.01	-0.10	-0.02	0.02
	(0.05)	(0.04)	(0.04)	(0.08)	(0.05)	(0.04)
Ln services share	-0.24**	-0.12*	-0.12	0.07	0.07	0.11
	(0.11)	(0.07)	(0.12)	(0.15)	(0.08)	(0.10)
Ln IEA stock	0.03	-0.01	-0.04	-0.16	-0.18	0.04
	(0.07)	(0.05)	(0.04)	(0.14)	(0.11)	(0.05)
Governm. orientation	-0.11	-0.03	-0.06	0.20	0.11	0.06
	(0.14)	(0.06)	(0.04)	(0.22)	(0.09)	(0.06)
Openness	0.13*	0.06*	-0.00	0.23	0.11	-0.05
	(0.07)	(0.04)	(0.04)	(0.20)	(0.10)	(0.05)
WTO (0,1)	0.08**	0.03	0.01	0.11	0.09*	0.03
	(0.03)	(0.02)	(0.03)	(0.08)	(0.05)	(0.03)
Polity (-1 to 1)	0.00	-0.00	-0.01	-0.01	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)	(0.01)
No. of observations	220	1,060	701	218	1,049	701
No. of countries	110	99	65	109	98	65
Over-ID test (p-value)	0.67	0.31	0.42	0.92	0.17	0.74
Weak-ID test (F-stat)	36.49	22.07	8.39	36.30	22.14	8.39
adj. R^2	-0.02	0.32	0.43	0.15	0.47	0.65

Note: Fixed-effects IV estimates. Standard errors in parentheses adjusted for within-group clustering and heteroskedasticity. All regressions use year dummies and constant (not shown). * p<0.1, ** p<0.05, *** p<0.01. Instruments for Kyoto: membership in ICC and its spatial lag.

Appendix B

Appendix to Chapter 2

B.1 Data Appendix

Input-output tables

The OECD collects input-output tables for its members and various other countries. Input-output (I-O) tables are observed around the years 1995, 2000 and 2005. We apply the 1995 I-O table for the years 1995-98, the 2000 table for 1999-2002 and the 2005 table for 2003-07. For 37 out of the 40 countries we have at least two I-O tables; Table B.2 gives an overview of availability for each country. For cases where no input-output table was available for the years under investigation we chose the I-O table of the nearest year possible. This implies the assumption that the economic structure (and specifically the relative prices) has not changed between these two points in time. The OECD I-O tables contain 48 industries, mostly on the two digit level of the International Standard Industrial Classification of All Economic Activities (ISIC) Revision 3. We aggregated these I-O industries to 15 sectors to match the emission data of the IEA (see Table B.1). Implicitly, we assume that all products within a sector are produced with the same CO_2 intensity. The high level of sectoral aggregation in our analysis gives rise to an aggregation bias when this assumption does not hold.[1]

[1] There is a trade-off between sectoral detail and having harmonized data for a large set of countries. Since we are interested in differences in the carbon footprints of Kyoto and non-Kyoto countries, we chose to include as many countries as possible at the cost of sectoral detail.

Table B.1: Sector classification

Sector	ISIC code	Sector description
1	1+2, 5	Agriculture, forestry, fishing
2	10-14,23,40	Electricity, gas and water supply, mining and quarrying
3	27	Basic metals
4	24	Chemicals and petrochemicals
5	26	Other non-metallic mineral products
6	34+35	Transport equipment
7	28-32	Machinery
8	15+16	Food products, beverages, tobacco
9	21+22	Paper, paper products, pulp and printing
10	20	Wood and wood products
11	17-19	Textile and leather
12	25,33,36,37	Non-specified industries
13	45	Construction
14	60-62	Transport
15	41,50-52, 55,63-99	Other services

Trade data

Bilateral trade data is obtained from the UN Comtrade database. It is translated from the Standard International Trade Classification (SITC) Rev. 3 to ISIC Rev. 3 with an industry concordance table provided by RAMON[2]. In the Comtrade database, imports are generally valued with CIF prices, exports with FOB prices. In order to have the same valuation for imports and exports, we use the FOB export price of the partner country as FOB price of imports. Thereby we ignore the carbon dioxide emissions caused by international transportation. For Russia, bilateral trade data is not available in the year 1995. Hence, we assume the trade relations in 1995 to be as in 1996 and use trade data of 1996 for the Russian Federation. Prior to 1999 bilateral trade data for Belgium and Luxembourg is reported jointly. Therefore trade, output and emissions data of both countries is aggregated in all years. It is assumed that both countries produce with Belgian technology, i.e. we apply the Belgian I-O table to the region Belgium-Luxembourg. Furthermore, service trade is assumed to be zero. This assumption is due to data limitations on bilateral service trade

[2] `http://ec.europa.eu/eurostat/ramon/`

flows and implies that all countries absorb CO_2 emissions embodied in final service goods domestically.

Sectoral CO_2 emissions

Sectoral CO_2 emissions are taken from the IEA CO_2 Emissions from Fuel Combustion (detailed estimates) Vol. 2009 database. The IEA estimates the CO_2 emissions from fossil fuel combustion with the default method and emission factors of different fuels suggested by the Intergovernmental Panel on Climate Change guidelines. Other sources of carbon dioxide emissions such as fugitive emissions, industrial processes or waste are disregarded. However, CO_2 emissions from fuel combustion make up around 80% of total CO_2 emissions. We also do not consider emissions from international bunker fuels.

Output data

In order to obtain emission coefficients, we need to divide sectoral emission levels by some measure of sectoral output. This is the most challenging part of constructing our carbon footprint database. Whenever possible, output data come from the OECD Structural Analysis Database (STAN).[3] STAN output data is available in current national currency only and was converted to current U.S. dollars with the period average exchange rates from the IMF IFS database. Even though the coverage of STAN data is excellent, some data points are missing. Yet, for a MRIO approach, a balanced sample is needed. We impute missing data points by applying overall growth rates of output or where not available of real GDP. This seems preferable to applying emission coefficients of the ROW aggregate since it would introduce structural breaks in the carbon footprint series. For Switzerland manufacturing sector's output (sectors 3-12) is missing in 1995 and 1996. We impute the data points with the growth rate of manufacturing output obtained from STAN. The Swiss transportation sector's output is also missing in those years and is calculated by applying the growth rate of the total economy's output from STAN. Output is missing for Canada in

[3] The 27 countries are Austria, Belgium-Luxembourg, Canada, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Japan, Korea, Netherlands, Norway, New Zealand, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, United Kingdom, and the United States.

2006 and 2007 and for New Zealand and Portugal in 2007. We use real GDP growth rates (data series rgdpch) from the PWT 6.3 database to obtain sectoral output levels.

For countries not covered by STAN, sectoral output of the manufacturing industries was taken from the INDSTAT2 2011 database which is given in ISIC Rev. 3.[4] We complement this with non-manufacturing output (sectors 1, 2, 13-15) obtained from the UN SNA database where available, exceptions see below. In the SNA database, transport (ISIC 60-62) and storage (ISIC 63) are reported jointly, therefore our industry category 14 contains part of category 15 in those countries. As in the STAN database, some countries are not covered in all sample years and missing data were imputed. Manufacturing output is interpolated for the years 1995 and 1997 for South Africa. Manufacturing output is not available in INDSTAT2 for Argentina from 2003-07, and Australia, Chile, Israel, Mexico and Turkey in 2007. This data was generated with the growth rate of the total economy's output from the UN SNA. In 2007, these growth rates are not available for Australia and Turkey and we use the growth rate of real GDP from PWT 6.3 instead. The SNA does not feature sectoral output data for Russia from 1995-2001 and we impute the missing data with growth rates of the total economy from the SNA database.

For Australia, China, Indonesia, and Turkey non-manufacturing sectoral output (sectors 1, 2, 13-15) was not available in the UN SNA. Instead, we interpolate output data from the OECD I-O tables. This gives imputed data for China and Indonesia from 1995-2005, Australia from 1998-2004, and Turkey from 1996-2002. I-O output data are again converted to U.S. dollars with period average exchange rates from the IFS database. Extrapolation for the remaining missing years is done by applying real GDP growth rates from PWT 6.3.

After matching all data, we end up with a data set spanning the years 1995 to 2007 comprising 15 sectors[5] on a two digit ISIC Rev. 3 level and 40 countries. 28 out of the investigated countries face binding emissions restrictions due to the Kyoto Protocol and 11 are non-OECD member countries (Argentina, Brazil, Chile, China, Estonia, India, Indonesia, Israel, Russia, Slovenia and South Africa). The sample countries are responsible for about 80% of worldwide carbon dioxide emissions in the sample years, see also Figure 1.

[4] The remaining 13 countries are Australia, Argentina, Brazil, Chile, China, India, Indonesia, Israel, Mexico, Romania, Russia, South Africa, and Turkey.

[5] 12 out of 15 sectors comprise internationally tradable goods.

Table B.2: Input-output table availability

Country	ISO code	Input-output table for period		
		mid 1990s	early 2000s	mid 2000s
Argentina	ARG	1997		
Australia	AUS		1998/99	2004/05
Austria	AUT	1995	2000	2005
Belgium	BEL	1995	2000	2004
Brazil	BRA	1995	2000	2005
Canada	CAN	1995	2000	2005
China	CHN	1995	2000	2005
Chile	CHL	1996		2003
Czech Republic	CZE		2000	2005
Denmark	DNK	1995	2000	2005
Estonia	EST	1997	2000	2005
Finland	FIN	1995	2000	2005
France	FRA	1995	2000	2005
Germany	DEU	1995	2000	2005
Greece	GRC	1995	2000	2005
Hungary	HUN		2000	2005
India	IND	1993/94	1998/99	2003/04
Indonesia	IDN	1995	2000	2005
Ireland	IRL	1995	2000	2005
Israel	ISR	1995		2005
Italy	ITA	1995	2000	2005
Japan	JPN	1995	2000	2005
Korea	KOR		2000	2005
Mexico	MEX			2003
Netherlands	NLD	1995	2000	2005
New Zealand	NZL	1995/96		2002/03
Norway	NOR	1995	2000	2005
Poland	POL	1995	2000	2005
Portugal	PRT	1995	2000	2005
Romania	ROM		2000	2005
Russia	RUS	1995	2000	
Slovakia	SVK	1995	2000	2005
Slovenia	SVN		2000	2005
South Africa	ZAF	1993	2000	2002
Spain	ESP	1995	2000	2005
Sweden	SWE	1995	2000	2005
Switzerland	CHE		2000	
Turkey	TUR	1996	1998	2002
United Kingdom	GBR	1995	2000	2005
United States	USA	1995	2000	2005

B.2 Detailed regression output

Table B.3: Detailed table – Robustness check on alternative country samples

	(A1)	(A2)	(A3)	(A4)	(A5)	(A6)	(A7)	(A8)	(A9)
Dep.var.:	Emissions	Footprints	Imports	Emissions	Footprints	Imports	Emissions	Footprints	Imports
Sample:	China excluded			DEU,ROU,POL,SVK excluded			Ex-communist countries excl.		
Kyoto (0,1)	-0.05*	0.07	0.13***	-0.07**	0.06	0.14***	0.01	0.12**	0.13**
	(0.03)	(0.05)	(0.04)	(0.03)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)
Log GDP	0.13	0.38***	0.23**	0.11	0.33**	0.20	0.61**	0.69***	0.10
	(0.08)	(0.11)	(0.10)	(0.10)	(0.12)	(0.13)	(0.26)	(0.15)	(0.16)
Polity (-10 to 10)	0.02***	0.03***	0.01**	0.01*	0.03***	0.01**	0.03***	0.04***	0.01
	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
Log MEA stock	0.18*	0.09	-0.13	0.02	-0.04	-0.11	0.34**	0.08	-0.31*
	(0.10)	(0.11)	(0.11)	(0.11)	(0.07)	(0.09)	(0.16)	(0.14)	(0.18)
EU (0,1)	-0.07*	-0.08	-0.01						
	(0.04)	(0.05)	(0.04)						
China (0,1)				0.29***	0.17**	-0.10			
				(0.09)	(0.06)	(0.07)			
2nd stage diagnostics									
adj. R^2	0.31	0.31	0.21	0.52	0.46	0.22	0.44	0.33	0.23
F-stat	13.17	9.94	2.41	8.93	24.72	3.21	25.79	21.11	2.17
RMSE	0.06	0.08	0.07	0.07	0.08	0.07	0.09	0.09	0.07
1st stage diagnostics									
Over-ID test (Hansen J, p-value)	0.23	0.17	0.25	0.41	0.47	0.13	0.25	0.07	0.14
Weak-ID test (F-stat)	35.55	35.55	35.55	39.07	39.07	39.07	13.80	13.80	13.80
Max IV F-test size bias[a]	10%	10%	10%	10%	10%	10%	15%	15%	15%
Max IV bias rel. to OLS[a]	5%	5%	5%	5%	5%	5%	10%	10%	10%

Notes: First-differenced (FD) models. N=40 countries, T=2: pre-treatment average (1997-2000), post-treatment average (2004-2007). Emissions and footprints in log per capita values. Net carbon imports as share of domestic carbon emissions. All regressions include a constant (not shown). Standard errors and 1st stage diagnostics are heteroskedasticity-robust and finite-sample adjusted. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Excluded instruments for Kyoto variable: ratification status of ICC treaty, spatial lag thereof, and lagged growth rate of population. IV (instrumental variables regression). Robust underidentification test (not reported) satisfied in every regression.

[a]Stock and Yogo (2005); critical values are for Cragg-Donalds F-statistic and i.i.d. errors.

Table B.4: Detailed table – Robustness check on alternative IV strategies

	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)	(B7)	(B8)	(B9)
Dep.var.:	Emissions	Footprints	Imports	Emissions	Footprints	Imports	Emissions	Footprints	Imports
IV strategy:	ICC variable only (LIML)			Lagged population growth			Selection probability		
Kyoto (0,1)	-0.14**	0.02	0.21**	-0.06*	0.06	0.13***	-0.08**	0.04	0.13***
	(0.07)	(0.06)	(0.10)	(0.03)	(0.05)	(0.04)	(0.03)	(0.04)	(0.03)
Log GDP	0.09	0.34***	0.25	0.13	0.36***	0.21*	0.12	0.34***	0.21**
	(0.12)	(0.11)	(0.15)	(0.09)	(0.12)	(0.11)	(0.08)	(0.10)	(0.10)
Polity (-10 to 10)	0.01	0.03***	0.02*	0.02*	0.03***	0.01**	0.01*	0.03***	0.01***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
Log MEA stock	0.13	0.06	-0.13	0.10	0.05	-0.10	0.11	0.06	-0.10
	(0.11)	(0.10)	(0.13)	(0.12)	(0.10)	(0.12)	(0.10)	(0.09)	(0.11)
EU	-0.05	-0.06	-0.03	-0.07**	-0.08	-0.01	-0.07**	-0.07*	-0.00
	(0.04)	(0.05)	(0.06)	(0.04)	(0.05)	(0.05)	(0.03)	(0.04)	(0.04)
China (0,1)	0.29***	0.17***	-0.08	0.31***	0.18***	-0.10	0.30***	0.18***	-0.10*
	(0.08)	(0.05)	(0.08)	(0.08)	(0.06)	(0.07)	(0.07)	(0.05)	(0.06)
2nd stage diagnostics									
adj. R^2	0.43	0.46	-0.19	0.55	0.44	0.20	0.54	0.46	0.21
F-stat	14.55	75.78	2.12	10.58	44.31	2.68	11.64	61.11	2.90
RMSE	0.08	0.08	0.09	0.07	0.08	0.07	0.06	0.07	0.06
1st stage diagnostics									
Over-ID test (Hansen J, p-value)	0.64	0.33	0.19						
Weak-ID test (F-stat)	4.33	4.33	4.33	95.57	95.57	95.57	54.73	54.73	54.73
Max IV F-test size bias[a]	25%	25%	25%	10%	10%	10%	10%	10%	10%
Max IV bias rel. to OLS[a]				5%	5%	5%	5%	5%	5%

Notes: First-differenced (FD) models. N=40 countries, T=2: pre-treatment average (1997-2000), post-treatment average (2004-2007). Emissions and footprints in log per capita values. Net carbon imports as share of domestic carbon emissions. All regressions include a constant (not shown). Standard errors and 1st stage diagnostics are heteroskedasticity-robust and finite-sample adjusted. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Excluded instruments for Kyoto variable: ratification status of ICC treaty, spatial lag thereof, and lagged growth rate of population. IV (instrumental variables regression). Robust underidentification test (not reported) satisfied in every regression.

[a] Stock and Yogo (2005); critical values are for Cragg-Donalds F-statistic and i.i.d. errors.

Table B.5: Detailed table – Robustness check on alternative dependent variables

	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)	(C7)	(C8)	(C9)
Dep.var.:	Emission[a]	Footprint[a]	Imports	Footprint	Footprint[a]	Imports	Footprint	Footprint[a]	Imports
Computation:				Fixed I-O tables (2000)			US I-O table for RoW		
Kyoto (0,1)	-0.07**	0.03	0.10***	0.04	0.01	0.11**	0.06	0.03	0.14***
	(0.03)	(0.04)	(0.03)	(0.05)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
Log GDP				0.33**		0.21*	0.35***		0.20*
				(0.13)		(0.10)	(0.11)		(0.11)
Log GDP per capita	-0.85***	-0.64***	0.22**		-0.66***			-0.65***	
	(0.11)	(0.13)	(0.09)		(0.15)			(0.13)	
Polity (-10 to 10)	0.01	0.03***	0.01***	0.03***	0.02**	0.01*	0.03***	0.02***	0.01**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)
Log MEA stock	0.09	0.04	-0.00	0.07	0.06	-0.07	0.04	0.04	-0.10
	(0.13)	(0.13)	(0.11)	(0.13)	(0.15)	(0.11)	(0.10)	(0.13)	(0.12)
EU (0,1)	-0.08*	-0.09	-0.01	-0.09*	-0.10	-0.02	-0.08	-0.09	-0.01
	(0.05)	(0.06)	(0.04)	(0.05)	(0.06)	(0.04)	(0.05)	(0.06)	(0.05)
China (0,1)	0.29***	0.17**	-0.15*	0.23***	0.22**	-0.07	0.19***	0.17**	-0.10
	(0.08)	(0.07)	(0.08)	(0.07)	(0.08)	(0.06)	(0.05)	(0.07)	(0.07)
2nd stage diagnostics									
adj. R^2	0.69	0.53	0.29	0.42	0.49	0.12	0.44	0.53	0.20
F-stat	25.74	33.06	3.72	20.44	13.40	1.70	50.14	34.39	2.73
RMSE	0.08	0.08	0.06	0.09	0.09	0.07	0.08	0.08	0.07
1st stage diagnostics									
Over-ID test (Hansen J, p-value)	0.30	0.41	0.32	0.16	0.32	0.18	0.22	0.43	0.18
Weak-ID test (F-stat)	36.00	36.00	36.00	34.39	36.00	34.39	34.39	36.00	34.39
Max IV F-test size bias[b]	10%	10%	10%	10%	10%	10%	10%	10%	10%
Max IV bias rel. to OLS[b]	5%	5%	5%	5%	5%	5%	5%	5%	5%

Notes: First-differenced (FD) models. N=40 countries, T=2: pre-treatment average (1997-2000), post-treatment average (2004-2007). Net carbon imports as share of domestic carbon emissions. All regressions include a constant (not shown). Standard errors and 1st stage diagnostics are heteroskedasticity-robust and finite-sample adjusted. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Excluded instruments for Kyoto variable: ratification status of ICC treaty, spatial lag thereof, and lagged growth rate of population. IV (instrumental variables regression). Robust underidentification test (not reported) satisfied in every regression.

[a] Log emissions and footprint per unit of GDP. [b]Stock and Yogo (2005); critical values are for Cragg-Donalds F-statistic and i.i.d. errors.

Table B.6: Detailed table – Robustness check on alternative treatment windows

	(D1)	(D2)	(D3)	(D4)	(D5)	(D6)	(D7)	(D8)	(D9)
Dep.var.:	Emissions	Footprints	Imports	Emissions	Footprints	Imports	Emissions	Footprints	Imports
Treatment window:	narrow: 2002 only			broad: 2001-04			treatment at start of 2005		
Kyoto (0,1)	-0.05*	0.05	0.11***	-0.08**	0.06	0.15***	-0.08**	0.03	0.12**
	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.05)
Log GDP	0.13	0.39***	0.23*	0.13	0.34***	0.18	0.13	0.34***	0.17
	(0.09)	(0.12)	(0.12)	(0.09)	(0.11)	(0.12)	(0.13)	(0.11)	(0.18)
Polity (-10 to 10)	0.02*	0.03***	0.01**	0.02*	0.03***	0.01*	0.03***	0.03**	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Log MEA stock	0.12	0.06	-0.09	0.11	0.04	-0.12	0.09	0.06	-0.06
	(0.11)	(0.10)	(0.11)	(0.12)	(0.10)	(0.13)	(0.17)	(0.11)	(0.22)
EU (0,1)	-0.08**	-0.09	-0.01	-0.07*	-0.07	-0.00	-0.03	-0.03	-0.02
	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.07)
China (0,1)	0.36***	0.20***	-0.13*	0.32***	0.18***	-0.11	0.75***	0.34***	-0.32**
	(0.09)	(0.06)	(0.07)	(0.09)	(0.06)	(0.07)	(0.10)	(0.07)	(0.12)
2nd stage diagnostics									
adj. R^2	0.58	0.48	0.20	0.57	0.43	0.19	0.69	0.34	0.06
F-stat	12.07	57.27	2.72	10.62	36.08	3.26	54.85	64.65	8.66
RMSE	0.06	0.07	0.06	0.08	0.08	0.08	0.05	0.06	0.08
1st stage diagnostics									
Over-ID test (Hansen J, p-value)	0.15	0.25	0.12	0.18	0.23	0.12	0.62	0.50	0.22
Weak-ID test (F-stat)	33.66	33.66	33.66	33.73	33.73	33.73	8.70	8.70	8.70
Max IV F-test size bias[a]	10%	10%	10%	10%	10%	10%	10%	10%	10%
Max IV bias rel. to OLS[a]	5%	5%	5%	5%	5%	5%			

Notes: First-differenced (FD) models. Columns (D7)-(D9) use limited-information maximum likelihood (LIML). N=40 countries, T=2: pre-treatment average, post-treatment average. Emissions and footprints in log per capita values. Net carbon imports as share of domestic carbon emissions. All regressions include a constant (not shown). Standard errors and 1st stage diagnostics are heteroskedasticity-robust and finite-sample adjusted. * p<0.1,** p<0.05, *** p<0.01. Excluded instruments for Kyoto variable: ratification status of ICC treaty, spatial lag thereof, and lagged growth rate of population. IV (instrumental variables regression). Robust underidentification test (not reported) satisfied in every regression.
[a]Stock and Yogo (2005); critical values are for Cragg-Donalds F-statistic and i.i.d. errors.

Table B.7: Detailed table – Robustness check: Fixed effects estimation on yearly data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Dep.var.:	Emissions	Footprints	Imports	Emissions	Footprints	Imports	Emissions	Footprints	Imports	Emissions	Footprints	Imports
Method:	FE-OLS	FE-OLS	FE-OLS	FE-IV	FE-IV	FE-IV	FD-OLS	FD-OLS	FD-OLS	FD-IV	FD-IV	FD-IV
Kyoto (0,1)	-0.03***	0.02	0.06***	-0.07***	0.05**	0.13***	-0.00	0.00	0.01	-0.57	0.17	0.78
	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.77)	(0.90)	(1.23)
Log GDP	0.46***	0.48***	0.02	0.44***	0.50***	0.05	0.46***	0.58***	0.11	0.69***	0.37*	-0.38
	(0.07)	(0.07)	(0.08)	(0.07)	(0.07)	(0.08)	(0.10)	(0.16)	(0.13)	(0.25)	(0.22)	(0.32)
Polity (-10 to 10)	0.01**	0.01**	0.00	0.01*	0.01**	0.00	0.00	0.00	-0.00	0.00	0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)
Log MEA	-0.05	-0.08*	-0.06	-0.03	-0.10**	-0.11**	-0.05	-0.11	-0.07	-0.07	-0.05	0.02
	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.07)	(0.05)	(0.12)	(0.20)	(0.21)
EU (0,1)	-0.04***	-0.02	0.02	-0.04**	-0.02	0.01	-0.01	-0.00	0.01	-0.03	-0.00	0.03
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
China (0,1)	0.04	0.08**	0.04	0.03	0.09**	0.06	-0.04***	0.06	0.09*			
	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.01)	(0.05)	(0.05)			
2nd stage diagnostics												
adj. R^2	0.32	0.35	0.05	0.30	0.34	0.01	0.11	0.07	0.01	-1.92	-0.01	-0.79
F-stat	9.48	19.85	4.94	10.48	19.48	6.33	4.52	4.20	1.86	4.04	3.42	0.83
RMSE	0.05	0.07	0.07	0.06	0.07	0.08	0.04	0.09	0.09	0.08	0.10	0.14
1st stage diagnostics												
Over-ID test (Hansen J, p-value)				0.32	0.19	0.04				0.38	0.24	0.26
Weak-ID test (F-stat)				82.01	82.01	82.01				0.78	0.78	0.78
Max IV F-test size bias[a]				10%	10%	10%				>30%	>30%	>30%
Max IV bias rel. to OLS[a]				5%	5%	5%				>25%	>25%	>25%

Notes: Fixed effects (FE, within) or first-differenced (FD) models. N=40 countries, T=11 (1997-2007). Emissions and footprints in log per capita values. Net carbon imports as share of domestic carbon emissions. All regressions include a constant and a comprehensive set of year dummies (not shown). Standard errors and 1st stage diagnostics are heteroskedasticity-robust and finite-sample adjusted. * p<0.1,** p<0.05, *** p<0.01. Excluded instruments for Kyoto variable: ratification status of ICC treaty, spatial lag thereof, and lagged growth rate of population. IV (instrumental variables regression). Robust underidentification test (not reported) satisfied in every regression.
[a]Stock and Yogo (2005); critical values are for Cragg-Donalds F-statistic and i.i.d. errors.

Appendix C

Appendix to Chapter 3

Table C.1: Sectoral ATTs of Kyoto commitment - reduced sample

SITC	Sector label	ATT	SITC	Sector label	ATT
11	Beverages	-0.166 (0.174)	61	Leather, leather manufactures, n.e.s., and dressed furskins	-0.264 (0.244)
12	Tobacco and tobacco manufactures	-0.339 (0.689)	62	Rubber manufactures, n.e.s.	0.013 (0.142)
21	Hides, skins and furskins, raw	0.262 (0.436)	63	Cork and wood manufactures	0.042 (0.165)
22	Oil-seeds and oleaginous fruits	0.202 (0.545)	64[a]	Paper, paperboard and articles of paper pulp, of paper or of paperboard	-0.186 (0.147)
23	Crude rubber	-1.102*** (0.296)	65	Textile yarn, fabrics, made-up articles, n.e.s., and related products	-0.022 (0.117)
24	Cork and wood	0.170 (0.480)	66[a]	Non-metallic mineral manufactures, n.e.s.	-0.229* (0.128)
25	Pulp and waste paper	0.102 (0.333)	67[a]	Iron and steel	-0.419** (0.174)
26	Textile fibres and their wastes	-0.159 (0.194)	68[a]	Non-ferrous metals	-0.787*** (0.192)
27	Crude fertilizers, and crude minerals	0.195 (0.224)	69[a]	Manufactures of metals, n.e.s.	-0.200* (0.110)
28	Metalliferous ores and metal scrap	0.287 (0.343)	71	Power-generating machinery and equipment	-0.429*** (0.146)
29	Crude animal and vegetable materials, n.e.s.	0.045 (0.151)	72	Machinery specialized for particular industries	-0.276** (0.123)
32	Coal, coke and briquettes	-0.182 (0.779)	73	Metalworking machinery	-0.119 (0.164)
33[a]	Petroleum, petroleum products and related materials	-0.445 (0.344)	74	General industrial machinery, n.e.s., machine parts, n.e.s.	-0.062 (0.113)
41	Animal oils and fats	0.408 (0.326)	75	Office machines and automatic data-processing machines	0.024 (0.130)
42	Fixed vegetable fats and oils, crude, refined or fractionated	0.078 (0.383)	76	Telecommunications and sound-recording and reproducing equip.	-0.449*** (0.136)
43	Animal or vegetable fats and oils, processed; waxes	-0.679* (0.384)	77	Electrical machinery, apparatus and appliances, n.e.s.	-0.203* (0.114)
51[a]	Organic chemicals	-0.213 (0.161)	78	Road vehicles (including air-cushion vehicles)	-0.070 (0.138)
52[a]	Inorganic chemicals	-0.330** (0.158)	79	Other transport equipment	-0.264 (0.265)
53[a]	Dyeing, tanning and coloring materials	-0.419*** (0.138)	81	Prefabricated buildings; sanitary, plumbing, heating, lighting fixtures	-0.306** (0.154)
54	Medicinal and pharmaceutical products materials, n.e.s.	-0.166 (0.128)	82	Furniture, and parts thereof	-0.013 (0.128)
55	Essential oils, resinoids, perfume materials; toilet, cleansing preparations	-0.112 (0.146)	83	Travel goods, handbags and similar containers	0.391** (0.161)
56[a]	Fertilizers	-0.189 (0.306)	84	Articles of apparel and clothing accessories	-0.226 (0.147)
57[a]	Plastics in primary forms	-0.537*** (0.168)	85	Footwear	-0.053 (0.195)
58[a]	Plastics in non-primary forms	-0.252 (0.164)	87	Professional, scientific and controlling instruments and apparatus, n.e.s.	-0.027 (0.125)
59[a]	Chemical materials and products, n.e.s.	-0.160 (0.134)	88	Photographic apparatus, optical goods, n.e.s.; watches and clocks	0.176 (0.140)
			89	Miscellaneous manufactured articles, n.e.s	-0.285*** (0.105)

Note: The table displays sector-specific ATTs from regression-adjusted DID kernel propensity score matching (Epanechnikov kernel, bandwidth 0.06) in reduced sample. Weights obtained sector-by-sector. Dependent variable is log of bilateral sectoral exports. Controls not shown. Heteroskedasticity-robust standard errors in parentheses. Significance at 1%, 5% and 10% indicated by ***, ** and * respectively.
[a] Goods category considered to be energy-intensive.

Table C.2: Robustness checks sectoral ATTs - reduced sample

SITC	Sector label	(1) Baseline	(2) Logit	(3) w/o China	(4) Policy
23	Crude rubber	-1.102***	-1.105***	-1.203***	-0.722
		(0.296)	(0.302)	(0.351)	(0.441)
43	Animal or vegetable fats	-0.679*	-0.700*	-0.666	-0.476
	and oils	(0.384)	(0.389)	(0.425)	(0.499)
52[a]	Inorganic chemicals	-0.330**	-0.298*	-0.344*	-0.457**
		(0.158)	(0.165)	(0.176)	(0.205)
53[a]	Dyeing, tanning and coloring	-0.419***	-0.450***	-0.423***	-0.493**
	materials	(0.138)	(0.139)	(0.146)	(0.225)
57[a]	Plastics (in primary form)	-0.537***	-0.617***	-0.472**	-0.846***
		(0.168)	(0.173)	(0.201)	(0.277)
66[a]	Non-metallic mineral	-0.229*	-0.110	-0.095	-0.218
	manufactures	(0.128)	(0.136)	(0.149)	(0.211)
67[a]	Iron and steel	-0.419**	-0.461**	-0.337	-0.516**
		(0.174)	(0.192)	(0.205)	(0.256)
68[a]	Non-ferrous metals	-0.787***	-0.701***	-0.665***	-0.619**
		(0.192)	(0.201)	(0.222)	(0.296)
69[a]	Manufactures of metals, n.e.s.	-0.200*	-0.165	-0.182	-0.417**
		(0.110)	(0.119)	(0.140)	(0.181)
71	Power-generating machinery	-0.429***	-0.339**	-0.256	0.701
	and equipment	(0.146)	(0.159)	(0.162)	(0.666)
72	Machinery specialized for	-0.276**	-0.336***	-0.332**	-0.414**
	particular industries	(0.123)	(0.127)	(0.142)	(0.201)
76	Telecommunications equipment	-0.449***	-0.503***	-0.468***	-0.466**
		(0.136)	(0.141)	(0.149)	(0.217)
77	Electrical machinery, apparatus	-0.203*	-0.268**	-0.243*	-0.373**
	and appliances, n.e.s.	(0.114)	(0.118)	(0.129)	(0.177)
81	Prefabricated buildings	-0.306**	-0.299*	-0.183	-0.332
		(0.154)	(0.160)	(0.173)	(0.236)
89	Miscellaneous manufactured	-0.285***	-0.256**	-0.229*	-0.390***
	articles	(0.105)	(0.112)	(0.133)	(0.140)
83	Travel goods, handbags	0.391**	0.376**	0.432**	0.352
		(0.161)	(0.172)	(0.189)	(0.336)

Note: The table displays sector-specific ATTs from regression-adjusted DID kernel matching (Epanechnikov kernel, bandwidth 0.06) in reduced sample. Weights obtained sector-by-sector. Dependent variable is log of bilateral sectoral exports. Heteroskedasticity-robust standard errors in parentheses. Significance at 1%, 5% and 10% indicated by ***, ** and * respectively. Results only shown for sectors with significant effects in Table C.1. Logit uses a Logit selection model. w/o China excludes China from sample. Policy includes political variables. [a] Goods category considered to be energy-intensive.

Appendix D

Appendix to Chapter 4

D.1 Information on dataset

The empirical analysis in this paper draws on bilateral virtual CO_2 trade flows as constructed for the dataset used in Aichele and Felbermayr (2012). A detailed description of data sources and methods is found in Appendix B. In contrast to the construction of footprints, country pairs with no information on the exporter's emission coefficients are dropped from the virtual CO_2 sample; which implies our database is unbalanced. In the STAN database, Swiss data is missing in 1995-96, Canadian data in 2006-07 and New Zealand and Portuguese data in 2007. Likewise, in the INDSTAT4 dataset some countries are not covered in all sample years and therefore dropped as exporters. This is so for Argentina from 2003-07, and Australia, Chile, Israel, Mexico and Turkey in 2007. Table D.1 summarizes information on dropped exporters.

Country list. The 40 sample countries are: Australia, Austria, Argentina, Belgium-Luxembourg, Brazil, Canada, Czech Republic, Chile, China, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, India, Indonesia, Ireland, Israel, Italy, Japan, Korea, Mexico, Netherlands, Norway, New Zealand, Poland, Portugal, Romania, Russia, Slovakia, Slovenia, Spain, South Africa, Sweden, Switzerland, United Kingdom, Turkey, and the United States.

Table D.1: Sample information

Country	Missing years	Kyoto country
Argentina	2003-2007	No
Australia	2007	Yes
Canada	2006, 2007	Yes
Chile	2007	No
Israel	2007	No
Mexico	2007	No
New Zealand	2007	Yes
Portugal	2007	Yes
Switzerland	1995, 1996	Yes
Turkey	2007	No

Note: Due to data availability, we do not have information on some country's sectoral emission intensities for a given year. These countries are dropped from the sample as exporters; however, they still appear as importers. The table displays all exporter-year combinations not in the dataset. Consequently, our estimations are based on an unbalanced sample.

D.2 Proofs

Result 1. Intermediate demand by firms

Each sector ℓ demands the output of every sector as an input via the final output good. Assuming that the cost function (2) is Cobb-Douglas with $c_m^\ell[.] = \Pi_m^{\alpha^\ell} t_m^{\beta^\ell} w_m^{(1-\alpha^\ell-\beta^\ell)}$, substituting the expression for Π_m, recognizing that $w_m = 1$ by choice of numeraire, and applying *Shephard's Lemma*, one obtains the unit input requirement for sector-s varieties from country x for the use of intermediate inputs in sector ℓ in country m,

$$\frac{\partial c_m^\ell [\cdot]}{\partial p_{mx}^s} = \alpha^\ell \mu^s c_m^\ell [\cdot] \frac{N_x^s (p_{mx}^s)^{-\sigma^s}}{(P_m^s)^{1-\sigma^s}}. \tag{D.1}$$

Sector-s intermediate goods trade between countries m and x, ι_{mx}^s, is the respective unit input requirement times total output of all demanding sectors $\ell = 1, \dots, S$. Hence,

$$\begin{aligned}
\iota_{mx}^s &= \sum_{\ell=1}^{S} \frac{\partial c_m^\ell [\cdot]}{\partial p_{mx}^s} N_m^\ell y_m^\ell \\
&= \mu^s N_x^s (p_{mx}^s)^{-\sigma_s} (P_m^s)^{\sigma_s - 1} \sum_{\ell=1}^{S} \alpha^\ell (\sigma^\ell - 1) N_m^\ell f^\ell \\
&= \mu^s \omega L_m N_x^s (p_{mx}^s)^{-\sigma_s} (P_m^s)^{\sigma_s - 1} \sum_{\ell=1}^{S} \frac{\alpha^\ell}{\omega} (\sigma^\ell - 1) \frac{L_{m,HQ}^\ell}{L_m} \\
&= g_m N_x^s \frac{\mu^s \omega L_m}{P_m^s} (\frac{p_{mx}^s}{P_m^s})^{-\sigma^s},
\end{aligned} \tag{D.2}$$

where the second line follows from using equation (D.1), recognizing that $y_m^\ell = \frac{(\sigma^\ell - 1) f^\ell}{c_m^\ell}$. The third line follows from multiplying by $\frac{\omega L_m}{\omega L_m}$ and noting that $N_m^\ell f^\ell = L_{m,HQ}^\ell$ is the total amount of headquarter services used in a sector. For the fourth line we factor out the term

$$g_m \equiv \sum_{\ell=1}^{S} \frac{\alpha^\ell}{\omega} (\sigma^\ell - 1) \frac{L_{m,HQ}^\ell}{L_m}, \tag{D.3}$$

with the remaining term in the equation being isomorphic to the expression for trade in final goods. Note first that $g_m > 0$ if intermediate input linkages exist (i.e., if $\alpha^\ell \neq 0$). Clearly, the amount of intermediates trade rises with the intermediates input requirement α. Intermediates trade is higher, when the share of headquarter services in the labor force in the importer m is high. I.e. if a country has a comparative disadvantage in the homogeneous goods sector and focuses more on the manufacturing varieties (e.g. due to a lax climate policy) it will have a higher trade volume.

Result 2. The carbon content of trade

The inter-industry demand for sector ℓ varieties in sector s is found by applying Shephard's lemma to sector-s's unit cost function, $\frac{\partial c_i^s[\cdot]}{\partial p_i^\ell}$. Those direct inter-industry demands for all ℓ and s combinations can be summarized in an $S \times S$ input-output table:

$$\boldsymbol{B_i} = \begin{pmatrix} \frac{\partial c_i^1}{\partial p_i^1} & \cdots & \frac{\partial c_i^S}{\partial p_i^1} \\ \vdots & \ddots & \vdots \\ \frac{\partial c_i^1}{\partial p_i^S} & \cdots & \frac{\partial c_i^S}{\partial p_i^S} \end{pmatrix}.$$

The Leontief inverse of this I-O table, $\boldsymbol{A_i} = (\boldsymbol{I} - \boldsymbol{B_i})^{-1}$, gives the total input requirement of all sector pairs along the domestic production chain. That is, the sth column of $\boldsymbol{A_i}$ is the total demand of sector s for the different varieties available. In order to translate this into the corresponding emissions of a good, premultiply with the vector of direct emission intensities of all varieties. The domestic carbon content of a sector-s variety is thus the vector product of the national carbon emission vector and the vector of unit input requirements of that sector, $\eta_i^s = \boldsymbol{e_i A_i^s}$.

The same logic applies for the MRIO accounting method. However, in the MRIO framework the I-O table captures the input-output relations between all sector pairs in all country pairs, see Trefler and Zhu (2010). That is, the I-O table is now a $KS \times KS$ matrix with

$$\boldsymbol{B} = \begin{pmatrix} \boldsymbol{B_{11}} & \boldsymbol{B_{12}} & \cdots & \boldsymbol{B_{1K}} \\ \boldsymbol{B_{21}} & \boldsymbol{B_{22}} & \cdots & \boldsymbol{B_{2K}} \\ \vdots & \vdots & \ddots & \vdots \\ \boldsymbol{B_{K1}} & \boldsymbol{B_{K2}} & \cdots & \boldsymbol{B_{KK}} \end{pmatrix},$$

where $\boldsymbol{B_{ji}}$ is the matrix of intermediate usage of country i sourced by country j. $\boldsymbol{B_{ji}}$ is again found by Shephard's Lemma,

$$\boldsymbol{B_{ji}} = \begin{pmatrix} \frac{\partial c_i^1}{\partial p_{ij}^1} & \cdots & \frac{\partial c_i^S}{\partial p_{ij}^1} \\ \vdots & \ddots & \vdots \\ \frac{\partial c_i^1}{\partial p_{ij}^S} & \cdots & \frac{\partial c_i^S}{\partial p_{ij}^S} \end{pmatrix}.$$

Going through the same steps as for the SRIO method, the total carbon content of a sector-s variety of country i is $\tilde{\eta}_i^s = \boldsymbol{e}\tilde{\boldsymbol{A}}_{\boldsymbol{i}}^{\boldsymbol{s}}$, where $\boldsymbol{e} = (e_1 \dots e_K)$ is the world-wide emission vector and $\tilde{\boldsymbol{A}}_{\boldsymbol{i}}^{\boldsymbol{s}}$ is the vector of world-wide input requirements of sector-s in country i – i.e. column $(S(i-1)+s)$ of the world-wide Leontief inverse $\boldsymbol{A} = (\boldsymbol{I} - \boldsymbol{B})^{-1}$.

Result 3. Special case: No intermediates trade

This section draws on results presented in Behrens et al. (2009). Let's assume that a firm's cost share of intermediates is zero. Then, the Cobb-Douglas unit cost functions and market clearing conditions are (using that $w_i = 1$ by choice of numeraire).

$$c_i\left[t_i\right] = (t_i)^{\beta} \qquad \forall\, i = 1, \dots, K,$$

$$\frac{\sigma f}{\mu\omega t_i^{\beta(1-\sigma)}} = \sum_{m=1}^{K} \frac{\phi_{mi} L_m}{\sum_{k=1}^{K} \phi_{mk} N_k (t_k)^{\beta(1-\sigma)}} \qquad \forall\, i = 1, \dots, K, \tag{D.4}$$

where $\phi_{ij} \equiv \tau_{ij}^{1-\sigma}$. We assume symmetric transportation costs without loss of generality. Define the matrices respectively vectors

$$\mathbf{t} \equiv \text{diag}\begin{pmatrix} t_1 \\ \vdots \\ t_K \end{pmatrix},\ \boldsymbol{\Phi} \equiv \begin{pmatrix} 1 & \cdots & \phi_{1K} \\ \vdots & \ddots & \vdots \\ \phi_{1K} & \cdots & 1 \end{pmatrix},\ \mathbf{L} \equiv \begin{pmatrix} L_1 \\ \vdots \\ L_K \end{pmatrix},\ \mathbf{N} \equiv \begin{pmatrix} N_1 \\ \vdots \\ N_K \end{pmatrix},$$

the scalar $r \equiv \frac{\mu\omega}{\sigma f}$, and let $\mathbf{1}$ be a vector of ones. Then, we can rewrite the free entry-and-exit condition in matrix notation as

$$r\mathbf{t}^{\beta(1-\sigma)}\boldsymbol{\Phi}\text{diag}(\boldsymbol{\Phi}\mathbf{t}^{\beta(1-\sigma)}\mathbf{N})^{-1}\mathbf{L} = \mathbf{1}.$$

Behrens et al. (2009) show how to solve this for $\mathbf{N}$ with simple matrix algebra:

$$\begin{aligned}\mathbf{L} &= \frac{1}{r}\mathrm{diag}(\mathbf{\Phi}\mathbf{t}^{\beta(1-\sigma)}\mathbf{N})\mathbf{\Phi}^{-1}\mathbf{t}^{\beta(\sigma-1)}\mathbf{1}\\ \Leftrightarrow \mathbf{L} &= \frac{1}{r}\mathrm{diag}(\mathbf{\Phi}^{-1}\mathbf{t}^{\beta(\sigma-1)}\mathbf{1})\mathbf{\Phi}\mathbf{t}^{\beta(1-\sigma)}\mathbf{N}\\ \Leftrightarrow \mathbf{N}^* &= r\mathbf{t}^{\beta(\sigma-1)}\mathbf{\Phi}^{-1}\mathrm{diag}(\mathbf{\Phi}^{-1}\mathbf{t}^{\beta(\sigma-1)}\mathbf{1})^{-1}\mathbf{L}.\end{aligned}$$

Assuming an interior solution, the number of varieties is given by

$$N_i^* = \frac{\mu\omega}{\sigma f}(t_i)^{\beta(\sigma-1)}\sum_{j=1}^{K}\frac{\varphi_{ij}L_j}{\sum_{k=1}^{K}\varphi_{jk}(t_k)^{\beta(\sigma-1)}} = \frac{\mu\omega}{\sigma f}(t_i)^{\beta(\sigma-1)}\sum_{j=1}^{K}\frac{\varphi_{ij}L_j}{\varphi_j}, \tag{D.5}$$

where φ_{ij} is an entry of $\mathbf{\Phi}^{-1}$. $\varphi_j \equiv \sum_{k=1}^{K}\varphi_{jk}(t_k)^{\beta(\sigma-1)}$ is a cost-weighted measure of a country j's inverse centrality (proximity to trade partners). The φ_{ij}'s and L_j's are exogenous variables. So the number of varieties a country produces depends on its carbon tax and the size of all trading partners weighted with a relative measure of their proximity – and thus on the carbon tax of all other countries as well.

As in Behrens et al. (2009), it is useful to express the number of varieties in shares. Multiplying (D.4) by N_i and summing over all countries, we can show that the number of varieties available worldwide is fixed[1] and depends on world size, fixed costs and taste and technology parameters:

$$\begin{aligned}\sum_i \frac{\sigma f}{\mu\omega}N_i &= \sum_i N_i \sum_{m=1}^{K}\frac{\phi_{mi}L_m t_i^{\beta(1-\sigma)}}{\sum_{k=1}^{K}\phi_{mk}N_k(t_k)^{\beta(1-\sigma)}}\\ \frac{\sigma f}{\mu\omega}\sum_i N_i &= \sum_{m=1}^{K}L_m\frac{\sum_i N_i\phi_{mi}t_i^{\beta(1-\sigma)}}{\sum_{k=1}^{K}\phi_{mk}N_k(t_k)^{\beta(1-\sigma)}}\\ \frac{\sigma f}{\mu\omega}\bar{N} &= \sum_{m=1}^{K}L_m = L\\ \bar{N} &= \frac{\mu\omega L}{\sigma f}.\end{aligned}$$

Since $\bar{N}$ is exogenously given, changes in the cost structure across countries (like changes in climate policy) will shift the shares in varieties across the globe. Let $\lambda_i \equiv \frac{N_i}{\bar{N}}$ be country i's share in world varieties and $\theta_i \equiv \frac{L_i}{L}$ the country's share in world income. From (D.5) λ_i is

[1] Note that this result stems from assuming that fixed costs are expressed as headquarter services and not in an input bundle. This implies that firm size is not fix and depends on marginal costs, i.e. climate policy. Otherwise, the number of varieties available worldwide would depend on marginal costs.

given by

$$\lambda_i \equiv \frac{N_i}{\bar{N}} = \frac{\mu\omega}{\sigma f \frac{\mu\omega L}{\sigma f}}(t_i)^{\beta(\sigma-1)} \sum_{j=1}^{K} \frac{\varphi_{ij} L_j}{\varphi_j} = (t_i)^{\beta(\sigma-1)} \sum_{j=1}^{K} \frac{\varphi_{ij}\theta_j}{\varphi_j}. \tag{D.6}$$

Scale effects Rearranging (D.6) such that $F_j \equiv \sum_{j=1}^{K} \frac{\varphi_{ij}\theta_j}{\varphi_j} = \lambda_i (t_i)^{\beta(1-\sigma)}$ and plugging into equation (4), we get an alternative useful expression for bilateral import volumes:

$$Q_{mx} = (\frac{\sigma-1}{\sigma})^{\sigma+1} \mu\omega L_m (\tau_{mx})^{-\sigma} (t_x)^{-\sigma\beta} (\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \frac{\lambda_j}{\lambda_x})^{-1}.$$

Differentiating with respect to the exporter's climate policy yields:

$$\begin{aligned}
\frac{\partial Q_{mx}}{\partial t_x} &= -\sigma\beta(\frac{\sigma-1}{\sigma})^{\sigma+1} \mu\omega L_m (\tau_{mx})^{-\sigma} (t_x)^{-\sigma\beta-1} (\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \frac{\lambda_j}{\lambda_x})^{-1} \\
&\quad -(\frac{\sigma-1}{\sigma})^{\sigma+1} \mu\omega L_m (\tau_{mx})^{-\sigma} (t_x)^{-\sigma\beta} \frac{\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \frac{\partial(\frac{\lambda_j}{\lambda_x})}{\partial t_x}}{(\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \frac{\lambda_j}{\lambda_x})^2}. \\
\Leftrightarrow \frac{\partial Q_{mx}}{\partial t_x} \frac{t_x}{Q_{mx}} &= -\sigma\beta - \frac{\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \frac{\partial(\frac{\lambda_j}{\lambda_x})}{\partial t_x} t_x \frac{\lambda_j/\lambda_x}{\lambda_j/\lambda_x}}{\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \frac{\lambda_j}{\lambda_x}} \\
&= -\sigma\beta - \frac{\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \lambda_j \kappa_{\lambda,x}}{\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \lambda_j} = -\sigma\beta - \frac{\sum_j \phi_{mj} F_j \kappa_{\lambda,x}}{\sum_j \phi_{mj} F_j},
\end{aligned}$$

where the last line again makes use of equation (D.6) and $\kappa_{\lambda,x} \equiv \frac{\partial \lambda_j/\lambda_x}{\partial t_x} \frac{t_x}{\lambda_j/\lambda_x}$. If the exporter increases its carbon tax, the import volume directly reacts with an elasticity of $-\sigma\beta$. The second term reflects how varieties are shifted across the globe in response to x's higher carbon tax. This indirect effect depends on trade costs between some country j and the importer, j's share in varieties, and how this share changes relative to the exporter's share in varieties.

The importer's scale effect can be calculated accordingly as:

$$\begin{aligned}
\frac{\partial Q_{mx}}{\partial t_m} &= -Q_{mx} \frac{\beta(1-\sigma)(t_m)^{\beta(1-\sigma)-1} \frac{\lambda_m}{\lambda_x} + \sum_j \phi_{mj} (t_m)^{\beta(1-\sigma)} \frac{\partial(\frac{\lambda_j}{\lambda_x})}{\partial t_m}}{\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \frac{\lambda_j}{\lambda_x}}. \\
\Leftrightarrow \frac{\partial Q_{mx}}{\partial t_m} \frac{t_m}{Q_{mx}} &= \frac{\beta(\sigma-1)(t_m)^{\beta(1-\sigma)} \lambda_m - t_m \lambda_x \sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \frac{\partial(\frac{\lambda_j}{\lambda_x})}{\partial t_m}}{\sum_j \phi_{mj} (t_j)^{\beta(1-\sigma)} \lambda_j} \\
&= \frac{\beta(\sigma-1) F_m - \sum_j \phi_{mj} F_j \kappa_{\lambda,m}}{\sum_j \phi_{mj} F_j},
\end{aligned}$$

So, the importer's scale effect is driven by the varieties (price index) channel only.

Special case: Two country world Let's assume we are in a two country world. The trade cost matrix is $\Phi = \begin{pmatrix} 1 & \phi \\ \phi & 1 \end{pmatrix}$ and its inverse is $\Phi^{-1} = \frac{1}{1-\phi^2} \begin{pmatrix} 1 & -\phi \\ -\phi & 1 \end{pmatrix}$. We investigate the trade flow Q_{12}, i.e. country 1 is the importer, country 2 the exporter. The exporter's scale effect is given by:

$$\kappa_{Q,2} = -\sigma\beta - \frac{\kappa_{\lambda,2} F_1}{F_1 + \phi F_2}.$$

For an interior solution, all F_i's have to be positive; otherwise the respective N_i's are non-positive. Thus, the sign of the second term depends on how the worldwide number of varieties shifts between country 1 and 2. Since all terms in

$$\frac{\partial \lambda_1}{\partial t_2} = \frac{\phi\beta(\sigma-1)(t_1 t_2)^{\beta(\sigma-1)}}{t_2} \left(\frac{\theta_1}{(\varphi_1)^2} + \frac{\theta_2}{(\varphi_2)^2}\right) > 0$$

are positive, country 1's share in varieties rises when country 2 strengthens its climate policy. Having only two countries, this implies that country 2's share in varieties falls. I.e. $\kappa_{\lambda,2} \equiv \frac{\partial(\lambda_1/\lambda_2)}{\partial t_2} \frac{t_2}{\lambda_1/\lambda_2}$ is positive. Thus the exporter's scale effect $\kappa_{Q,2} < 0$. If the exporter imposes a stricter climate policy, the import volume falls.

In the two country case, the importer's scale effect is

$$\kappa_{Q,1} = \frac{\beta(\sigma-1)F_1 - \kappa_{\lambda,1} F_1}{F_1 + \phi F_2}.$$

We have already shown, that the own share of varieties falls with a stricter climate policy. So $\kappa_{\lambda,1} \equiv \frac{\partial(\lambda_1/\lambda_2)}{\partial t_1} \frac{t_1}{\lambda_1/\lambda_2}$ is negative. Thus, the importer's scale effect $\kappa_{Q,1} > 0$. If the importer imposes a stricter climate policy, the import volume rises.

D.3 Detailed Regression Results

Table D.2: First-differenced regressions on pooled data

	(1)	(2)	(3)	(4)
Dep. variable:	Ln imports, Q_{mx}	Ln CO_2 intensity of imports, η_x	Ln CTT, η_i/η_j	Ln CO_2 imports, E_{mx}
Kyoto_m-Kyoto_x	0.005	0.016***	-0.028***	0.021**
	(0.009)	(0.001)	(0.002)	(0.009)
Joint FTA membership	0.045	0.001		0.046
	(0.029)	(0.004)		(0.029)
Joint WTO membership	-0.095	0.002		-0.093
	(0.166)	(0.018)		(0.171)
Joint EU membership	0.004	0.003		0.008
	(0.032)	(0.003)		(0.032)
Country × year effects	YES	YES	YES	YES
Country-pair sector effects	YES	YES	YES	YES
Observations	202,420	202,420	194,121	202,420
No. of countrypair-sectors	18,352	18,352	18,059	18,352
Adj. R^2	0.022	0.423	0.006	0.018
F-stat	20.655	707.682	7.396	13.429
RMSE	0.853	0.130	0.213	0.864

Note: First-differenced (FD) panel regressions on pooled sectoral data. Heteroskedasticity-robust standard errors (in brackets) are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. CTT: sectoral carbon terms of trade a la Antweiler (1996); computed as emission intensity of exporter over emission intensity of importer in a given sector.

Table D.3: First stage to column (4) Table 4.4

Dependent variable: Kyoto_m-Kyoto_x	(1)
ICC_m-ICC_x	0.258***
	(0.004)
ln GDP_m	-0.075***
	(0.026)
ln GDP_x	0.106***
	(0.025)
Joint FTA membership	-0.053***
	(0.012)
Joint WTO membership	-0.236***
	(0.057)
Joint EU membership	0.029***
	(0.011)
Observations	223,387
No. of countrypair-sectors	18,476
Adj. R^2	0.048
F-statistic	391.813

Note: First stage regression to column (4) Table 4.4 on pooled sectoral data. Year dummies and all relevant multilateral resistance (MR) control variables of the second stage are also included (i.e., FTA, WTO, EU, distance, contiguity, common language); see Baier and Bergstrand (2009a). Heteroskedasticity-robust standard errors (in brackets) are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Table D.4: Placebo tests: fictitious Kyoto ratification dates

Fictitious treatment date:	1997 FE	1998 FE	1997 Long DiD
Ln imports			
Kyoto$_m$-Kyoto$_x$	-0.014	-0.053***	0.023
	(0.020)	(0.020)	(0.019)
Joint FTA membership	0.111**	0.104**	0.099
	(0.046)	(0.046)	(0.103)
Joint WTO membership	-0.056	-0.053	0.343
	(0.230)	(0.230)	(0.664)
Adj. R^2	0.040	0.040	0.091
Ln CO_2 intensity			
Kyoto$_m$-Kyoto$_x$	0.002	0.005	0.004
	(0.004)	(0.004)	(0.019)
Joint FTA membership	0.004	0.005	0.009
	(0.008)	(0.008)	(0.017)
Joint WTO membership	0.003	0.003	-0.297**
	(0.068)	(0.068)	(0.144)
Adj. R^2	0.333	0.333	0.342
Ln CO_2 imports			
Kyoto$_m$-Kyoto$_x$	-0.012	-0.048**	0.026
	(0.021)	(0.021)	(0.003)
Joint FTA membership	0.116**	0.109**	0.104
	(0.046)	(0.046)	(0.105)
Joint WTO membership	-0.053	-0.051	0.021
	(0.251)	(0.251)	(0.649)
Adj. R^2	0.036	0.036	0.089

Note: Regressions on pooled sectoral data (observations: 103,782). (3) Long differences-in-differences (DiD): Estimation on pre- and post-treatment averages. The pre- and post-period are 1995-1996 and 1997-2000, i.e. 35,350 observations. Heteroskedasticity-robust standard errors (in brackets) are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. All models include a full set of country-and-year dummies, controls for trade policy (FTA and WTO membership) and a constant (not shown). Note that joint EU membership is dropped because there are no changes in EU status between 1995 and 1999.

Table D.5: Placebo tests: comparison of Kyoto and USA

	(1) Ln imports	(2) Ln CO_2 intensity	(3) Ln CTT	(4) Ln CO_2 imports
Kyoto_m-Kyoto_x	0.055***	0.034***	-0.053***	0.088***
	(0.012)	(0.003)	(0.005)	(0.012)
Joint FTA membership	0.076*	0.013		0.089**
	(0.041)	(0.009)		(0.042)
Joint WTO membership	-0.421*	-0.013		-0.434*
	(0.244)	(0.038)		(0.245)
Observations	101,020	101,020	96,180	101,020
No. of countrypair-sectors	8,620	8,620	8,492	8,620
Adj. R^2	0.175	0.685	0.056	0.074
F-stat	19.846	415.013	10.223	7.614
RMSE	0.928	0.180	0.307	0.948

Note: Trade between two Kyoto countries and trade between non-Kyoto countries unless one trade partner is the USA is dropped from sample. Fixed-effects (FE) panel regressions on pooled sectoral data. Heteroskedasticity-robust standard errors (in brackets) are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. All models include a full set of country-and-year dummies, controls for trade policy (joint FTA, and WTO membership) and a constant (not shown). Note that the EU dummy is dropped because there are no EU countries in the control group.

Table D.6: Placebo tests: comparison of Kyoto and developed non-Kyoto countries

	(1) Ln imports	(2) Ln CO_2 intensity	(3) Ln CTT	(4) Ln CO_2 imports
Kyoto_m-Kyoto_x	0.055***	0.034***	-0.053***	0.089***
	(0.012)	(0.003)	(0.005)	(0.012)
Joint FTA membership	0.061	0.011		0.072*
	(0.038)	(0.009)		(0.039)
Joint WTO membership	-0.421*	-0.013		-0.434*
	(0.243)	(0.038)		(0.244)
Observations	109,373	109,373	104,247	109,373
No. of countrypair-sectors	9,336	9,336	9,197	9,336
Adj. R^2	0.175	0.684	0.057	0.075
F-stat	20.923	442.897	10.869	8.232
RMSE	0.924	0.179	0.306	0.944

Note: Trade between two Kyoto countries and trade between non-Kyoto countries unless one trade partner is developed non-Kyoto country (i.e. Australia, Israel, Korea, USA) is dropped from sample. Fixed-effects (FE) panel regressions on pooled sectoral data. Heteroskedasticity-robust standard errors (in brackets) are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. All models include a full set of country-and-year dummies, controls for trade policy (joint FTA, WTO, and EU membership) and a constant (not shown).

Table D.7: Placebo tests: USA as additional Kyoto country

	(1) Ln imports	(2) Ln CO_2 intensity	(3) Ln CTT	(4) Ln CO_2 imports
$Kyoto_m$-$Kyoto_x$	0.046***	0.029***	-0.045***	0.075***
	(0.011)	(0.003)	(0.005)	(0.011)
Joint FTA membership	0.102***	0.010		0.113***
	(0.031)	(0.008)		(0.032)
Joint WTO membership	-0.144	-0.001		-0.145
	(0.163)	(0.036)		(0.165)
Joint EU membership	0.019	0.019**		0.037
	(0.035)	(0.009)		(0.035)
Observations	223,499	223,499	215,917	223,499
No. of countrypair-sectors	18,588	18,588	18,387	18,588
Adj. R^2	0.206	0.709	0.036	0.074
F-stat	46.310	878.755	11.703	15.238
RMSE	0.829	0.179	0.305	0.849

Note: Treatment group includes the USA as Kyoto ratifier with an emission cap; the hypothetical treatment date is 2002. Fixed-effects (FE) panel regressions on pooled sectoral data. Heteroskedasticity-robust standard errors (in brackets) are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. All models include a full set of country-and-year dummies, controls for trade policy (joint FTA, WTO, and EU membership) and a constant (not shown).

Table D.8: Regression on ln(imp+1), ln(beim+1)

	(1) Ln imports	(2) Ln CO_2 imports
$Kyoto_m$-$Kyoto_x$	0.086***	0.081***
	(0.019)	(0.012)
Joint FTA membership	0.086	0.103***
	(0.055)	(0.034)
Joint WTO membership	-0.308	-0.301
	(0.303)	(0.218)
Joint EU membership	-0.652***	-0.206***
	(0.059)	(0.037)
Observations	235,833	235,833
No. of countrypair-sectors	18,720	18,720
adj. R^2	0.129	0.090
F-stat	36.136	15.787
RMSE	1.596	0.945

Note: Fixed-effects (FE) panel regressions on pooled sectoral data. Heteroskedasticity-robust standard errors (in brackets) are corrected for clustering within country-pair and sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. All models include a full set of country-and-year dummies, controls for trade policy (joint FTA, WTO, and EU membership) and a constant (not shown).

Table D.9: Zero trade flows: Heckman selection model (maximum likelihood)

	Imports		CO_2 imports	
Dependent variable:	$\Pr(Q_{mxst}) > 0$	$\ln Q_{mxst}$	$\Pr(E_{mxst}) > 0$	$\ln E_{mxst}$
Equation:	Selection	Outcome	Selection	Outcome
Kyoto_m-Kyoto_x	0.033	0.035*	0.001	0.348***
	(0.022)	(0.019)	(0.021)	(0.021)
Joint FTA membership	0.062	0.266***	0.102**	0.277***
	(0.043)	(0.043)	(0.043)	(0.047)
Joint WTO membership	-0.392**	1.123***	0.051	1.063***
	(0.194)	(0.253)	(0.218)	(0.256)
Joint EU membership	-0.283***	-0.235***	-0.216***	-0.228***
	(0.080)	(0.044)	(0.077)	(0.048)
Ln distance	-1.198***	-1.441***	-1.128***	-1.436***
	(0.047)	(0.024)	(0.048)	(0.027)
Contiguity (0,1)	7.777***	0.383***	0.275	0.377***
	(0.240)	(0.056)	(0.288)	(0.061)
Common language (0,1)	0.683***	0.654***	0.656***	0.653***
	(0.097)	(0.047)	(0.095)	(0.052)
Common religion index	-0.362***		-0.336***	
	(0.069)		(0.068)	
Observations	235,833		235,833	
No. of countrypair-sectors	18,720		18,720	
Log likelihood	-474,541		-492,198	
Estimated correlation (rho)	-0.089***		-0.085***	
Estimated selection (lambda)	-0.162***		-0.167***	

Note: Heckman selection model (maximum likelihood). Pooled OLS estimation. All regressions include full set of country $\times$ year effects, sector dummies and constant (not shown), but no country-pair-sector fixed effects. An index of a country pair's similarity in religion as in Helpman et al. (2008) is the selection variable. Heteroskedasticity-robust standard errors (in brackets) corrected for clustering within country-pair(-sector); ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

Table D.10: Regressions on Pooled Data, Robustness Checks - Detailed Table

	Panel A: Alternative measures of CO_2 imports				
	(A1)	(A2)	(A3)	(A4)	(A5)
Measure:		MRIO		Technique fixed	MRIO I-O fixed
Dependent variable:	Ln CO_2 intensity	Ln CTT	Ln CO_2 imports	Ln CO_2 imports	Ln CO_2 imports
Kyoto_m-Kyoto_x	0.023***	-0.031***	0.072***	0.050***	0.073***
	(0.002)	(0.004)	(0.011)	(0.011)	(0.011)
FTA (0,1)	0.009		0.112***	0.103***	0.110***
	(0.006)		(0.032)	(0.031)	(0.032)
Joint WTO (0,1)	-0.001		-0.144	-0.144	-0.144
	(0.030)		(0.164)	(0.163)	(0.161)
Joint EU (0,1)	0.014*		0.033	0.019	0.032
	(0.007)		(0.036)	(0.035)	(0.035)
Observations	223,460	215,879	223,460	223,384	223,499
Adj. R^2	0.708	0.034	0.085	0.206	0.064

Note: Country-pair-sector fixed effects estimation on pooled sectoral data. All regressions include country-and-time dummies and constant (not shown). Heteroskedasticity-robust standard errors (in brackets) corrected for clustering within country-pair; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. MRIO: multi-region input-output tables to compute carbon content of trade. Technique fixed: I-O tables and emission coefficients fixed in year 2000. MRIO I-O fixed: only I-O table fixed in 2000.

Table D.11: Regressions on Pooled Data, Robustness Checks - Detailed Table cont.

	Panel B: Alternative samples and estimators					
	(B1)	(B2)	(B3)	(B4)	(B5)	(B6)
Sample:	w/o China & transition countries[a]			Aggregate data		
Dependent variable:	Ln imports	Ln CTT	Ln CO_2 imports	Ln imports	Ln CTT	Ln CO_2 imports
$Kyoto_m$-$Kyoto_x$	0.048***	0.032***	0.033***	0.056***	-0.172***	0.143***
	(0.012)	(0.006)	(0.013)	(0.019)	(0.020)	(0.022)
FTA (0,1)	0.087**		0.095**	0.065		0.065
	(0.040)		(0.043)	(0.049)		(0.058)
Joint WTO (0,1)				0.138		0.035
				(0.333)		(0.465)
Joint EU (0,1)				-0.054		-0.024
				(0.055)		(0.061)
Observations	136,392	134,046	136,392	19,623	19,588	19,623
Adj. R^2	0.136	0.043	0.058	0.603	0.034	0.233

Note: Country-pair fixed effects estimation. All models include country-and-time dummies and constant (not shown). Heteroskedasticity-robust standard errors (in brackets) corrected for clustering within country-pair; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively.

[a] Transition countries are CZE, EST, HUN, POL, ROU, RUS, SVN, SVK.

Table D.12: Regressions on Pooled Data, Robustness Checks - Detailed Table cont.

	Panel C: Alternative estimators					
	(C1)	(C2)	(C3)	(C4)	(C5)	(C6)
Method:	Long differences-in-differences			Poisson pseudo maximum likelihood		
Dependent variable:	Ln imports	Ln CTT	Ln CO_2 imports	Imports > 0	Imports ≥ 0	CO_2 imports ≥ 0
$Kyoto_m$-$Kyoto_x$	0.054**	-0.077***	0.103***	-0.014	-0.013	0.050*
	(0.022)	(0.009)	(0.023)	(0.017)	(0.017)	(0.027)
Ln GDP_m				1.286***	1.282***	1.307***
				(0.085)	(0.085)	(0.150)
Ln GDP_x				1.515***	1.508***	0.856***
				(0.092)	(0.092)	(0.145)
Joint FTA (0,1)	0.105	-0.021	0.115	0.035	0.036	0.079
	(0.078)	(0.035)	(0.080)	(0.057)	(0.057)	(0.059)
Joint WTO (0,1)	-0.416	0.004	-0.487	0.051	0.092	0.475
	(0.402)	(0.110)	(0.411)	(0.279)	(0.273)	(0.388)
Joint EU (0,1)	0.125*	-0.027	0.130*	-0.055	-0.055	-0.123*
	(0.067)	(0.028)	(0.067)	(0.041)	(0.041)	(0.065)
MR terms[a]				YES	YES	YES
Year effects				YES	YES	YES
Country × year FE	YES	YES	YES			
Observations	36,269	35,612	36,269	223,387	234,278	234,278
No. of countrypair-sectors	18,545	18,328	18545	18,476	18,588	18,588
Adj. R^2	0.333	0.051	0.112			
Log likelihood				-32,066.300	-32,093.527	-4,902.334

Note: Country-pair-sector fixed effects estimation on pooled sectoral data. Heteroskedasticity-robust standard errors (in brackets) corrected for clustering within country-pair-sector; ***,** and * denote statistical significance at the 1%, 5% and 10% level, respectively. Long differences-in-differences: Estimation on pre- and post-treatment averages. The pre- and post-period are 1995-2000 and 2004-2007.

[a] Multilateral resistance (MR) control variables (i.e., FTA, WTO, EU, distance, contiguity, common language) constructed according to Baier and Bergstrand (2009a).

Table D.13: Sector-by-sector regressions: differential commitment - Detailed table

Dependent variable:	Ln imports		Ln CO_2 intensity		Ln CTT		Ln CO_2 imports	
Method:	FE	long FE	FE	long FE	FE	long FE	FE	long FE
Sector:	Agriculture, forestry, fishing							
Kyoto$_m$-Kyoto$_x$	-0.04	-0.02	0.02***	0.06***	-0.03**	-0.11***	-0.02	0.05
	(0.04)	(0.08)	(0.01)	(0.01)	(0.01)	(0.03)	(0.04)	(0.08)
Joint FTA (0,1)	0.03	0.12	-0.01	-0.01	-0.01	-0.02	0.02	0.11
	(0.09)	(0.22)	(0.02)	(0.04)	(0.04)	(0.09)	(0.09)	(0.23)
Joint WTO (0,1)	-0.31	-0.25	0.02	0.02	0.01	-0.03	-0.29	-0.21
	(0.40)	(1.17)	(0.08)	(0.12)	(0.18)	(0.32)	(0.42)	(1.20)
Joint EU (0,1)	0.66***	0.79***	0.00	0.01	-0.02	-0.01	0.66***	0.79***
	(0.14)	(0.27)	(0.03)	(0.05)	(0.06)	(0.10)	(0.14)	(0.27)
Observations	18148	2992	18148	2992	17254	2900	18148	2992
Adj. R^2	0.16	0.24	0.74	0.79	0.01	0.03	0.08	0.10
Sector:	Electricity, energy, mining, quarrying							
Kyoto$_m$-Kyoto$_x$	0.08	0.14	0.05***	0.10***	-0.08***	-0.20***	0.13**	0.24**
	(0.06)	(0.12)	(0.01)	(0.02)	(0.02)	(0.04)	(0.06)	(0.12)
Joint FTA (0,1)	0.08	-0.22	0.01	0.04	-0.01	-0.03	0.09	-0.17
	(0.16)	(0.43)	(0.03)	(0.07)	(0.06)	(0.16)	(0.16)	(0.43)
Joint WTO (0,1)	-0.02	-1.22	0.01	-0.05	0.01	0.08	-0.01	-1.31
	(0.43)	(1.36)	(0.08)	(0.18)	(0.17)	(0.38)	(0.43)	(1.36)
Joint EU (0,1)	-0.41**	-0.37	0.00	-0.01	-0.01	-0.01	-0.41**	-0.37
	(0.17)	(0.34)	(0.03)	(0.05)	(0.06)	(0.11)	(0.17)	(0.34)
Observations	16209	2818	16209	2818	14577	2604	16209	2818
Adj. R^2	0.10	0.15	0.78	0.82	0.03	0.08	0.08	0.15
Sector:	Basic metals							
Kyoto$_m$-Kyoto$_x$	0.20***	0.21**	-0.00	0.01	0.01	-0.01	0.20***	0.21**
	(0.04)	(0.08)	(0.01)	(0.01)	(0.01)	(0.02)	(0.04)	(0.08)
Joint FTA (0,1)	0.14	-0.00	0.02	0.04	-0.01	-0.01	0.16	0.04
	(0.11)	(0.28)	(0.02)	(0.04)	(0.03)	(0.07)	(0.12)	(0.29)
Joint WTO (0,1)	0.05	0.22	0.03	-0.37	0.02	-0.20	0.08	-0.13
	(0.73)	(1.07)	(0.17)	(0.29)	(0.36)	(0.45)	(0.76)	(1.18)
Joint EU (0,1)	0.04	0.13	0.02	0.03	-0.02	-0.03	0.06	0.11
	(0.12)	(0.24)	(0.02)	(0.05)	(0.05)	(0.09)	(0.13)	(0.25)
Observations	18515	3033	18515	3033	17774	2981	18515	3033
Adj. R^2	0.29	0.47	0.84	0.88	0.04	0.06	0.13	0.21

Note: Country-pair fixed effect regression (sector-by-sector). All regressions include country-and-time effects and constant (not shown). Differential Kyoto commitment takes values (-1,0,1). The method of estimation is either fixed effects (within, FE) or long fixed-effects estimation on pre- and post-treatment averages (long FE). Heteroskedacticity-robust standard errors (in brackets) are adjusted for within country-pair clustering. ***, ** and * denote statistical significance at the 1%, 5% and the 10% levels, respectively.

Table D.14: Sector-by-sector regressions: differential commitment - Detailed table cont.

Dependent variable:	Ln imports		Ln CO_2 intensity		Ln CTT		Ln CO_2 imports	
Method:	FE	long FE	FE	long FE	FE	long FE	FE	long FE
Sector:	Chemicals and petrochemicals							
Kyoto$_m$-Kyoto$_x$	0.02	0.02	0.06***	0.07***	-0.11***	-0.14***	0.08***	0.09*
	(0.03)	(0.05)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.06)
Joint FTA (0,1)	0.04	0.25	0.01	0.02	-0.01	-0.01	0.05	0.27
	(0.08)	(0.18)	(0.03)	(0.06)	(0.05)	(0.12)	(0.09)	(0.20)
Joint WTO (0,1)	-0.13	-0.21	-0.02	-0.01	0.02	-0.12	-0.14	-0.21
	(0.45)	(0.99)	(0.14)	(0.18)	(0.29)	(0.33)	(0.50)	(1.07)
Joint EU (0,1)	0.12	0.22	0.02	0.01	-0.02	-0.02	0.14	0.20
	(0.08)	(0.14)	(0.03)	(0.05)	(0.05)	(0.10)	(0.09)	(0.15)
Observations	19356	3067	19356	3067	19111	3052	19356	3067
Adj. R^2	0.36	0.58	0.77	0.82	0.03	0.06	0.15	0.22
Sector:	Other non-metallic mineral products							
Kyoto$_m$-Kyoto$_x$	0.14***	0.17***	-0.00	0.00	0.02	0.01	0.14***	0.18**
	(0.03)	(0.07)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.07)
Joint FTA (0,1)	-0.02	-0.10	0.02	0.01	-0.01	-0.02	-0.01	-0.10
	(0.10)	(0.25)	(0.03)	(0.07)	(0.06)	(0.14)	(0.11)	(0.27)
Joint WTO (0,1)	0.11	1.25	-0.02	-0.05	0.03	-0.11	0.10	1.21
	(0.65)	(1.45)	(0.13)	(0.19)	(0.28)	(0.43)	(0.61)	(1.45)
Joint EU (0,1)	-0.21**	-0.27	0.02	0.00	-0.02	-0.01	-0.20**	-0.27
	(0.09)	(0.18)	(0.02)	(0.05)	(0.05)	(0.10)	(0.10)	(0.19)
Observations	18751	3028	18751	3028	18115	2982	18751	3028
Adj. R^2	0.24	0.40	0.78	0.81	0.04	0.06	0.10	0.17
Sector:	Transport equipment							
Kyoto$_m$-Kyoto$_x$	0.15***	0.18**	0.01	0.01	-0.01	-0.01	0.16***	0.21**
	(0.04)	(0.08)	(0.01)	(0.02)	(0.02)	(0.04)	(0.04)	(0.09)
Joint FTA (0,1)	0.03	0.04	0.02	0.02	-0.02	-0.04	0.05	0.02
	(0.11)	(0.26)	(0.03)	(0.09)	(0.07)	(0.19)	(0.12)	(0.28)
Joint WTO (0,1)	-0.14	-0.72	0.02	-0.11	0.00	-0.26	-0.12	-0.92
	(0.66)	(1.30)	(0.03)	(0.23)	(0.08)	(0.65)	(0.66)	(1.29)
Joint EU (0,1)	-0.04	0.06	0.03	0.03	-0.04	-0.05	-0.01	0.10
	(0.12)	(0.23)	(0.03)	(0.06)	(0.07)	(0.12)	(0.12)	(0.23)
Observations	18407	3003	18407	3003	17498	2926	18407	3003
Adj. R^2	0.32	0.53	0.77	0.80	0.00	0.02	0.17	0.29

Note: Country-pair fixed effect regression (sector-by-sector). All regressions include country-and-time effects and constant (not shown). Differential Kyoto commitment takes values (-1,0,1). The method of estimation is either fixed effects (within, FE) or long fixed-effects estimation on pre- and post-treatment averages (long FE). Heteroskedacticity-robust standard errors (in brackets) are adjusted for within country-pair clustering. ***, ** and * denote statistical significance at the 1%, 5% and the 10% levels, respectively.

Table D.15: Sector-by-sector regressions: differential commitment - Detailed table cont.

Dependent variable:	Ln imports		Ln CO_2 intensity		Ln CTT		Ln CO_2 imports	
Method:	FE	long FE	FE	long FE	FE	long FE	FE	long FE
Sector:	Machinery							
Kyoto$_m$-Kyoto$_x$	0.13***	0.10**	0.01	0.00	-0.02	-0.01	0.15***	0.11**
	(0.02)	(0.05)	(0.01)	(0.02)	(0.02)	(0.04)	(0.03)	(0.05)
Joint FTA (0,1)	0.11*	0.12	0.01	0.01	-0.02	-0.03	0.13*	0.13
	(0.07)	(0.18)	(0.03)	(0.07)	(0.06)	(0.14)	(0.07)	(0.19)
Joint WTO (0,1)	-0.16	-0.48	0.00	-0.08	0.02	-0.05	-0.16	-0.47
	(0.56)	(1.41)	(0.09)	(0.22)	(0.19)	(0.41)	(0.62)	(1.55)
Joint EU (0,1)	-0.15**	0.01	0.02	0.00	-0.03	-0.04	-0.13*	0.01
	(0.07)	(0.14)	(0.03)	(0.05)	(0.05)	(0.10)	(0.07)	(0.14)
Observations	19488	3074	19488	3074	19354	3067	19488	3074
Adj. R^2	0.51	0.70	0.80	0.83	0.02	0.03	0.28	0.40
Sector:	Food products, beverages, tobacco							
Kyoto$_m$-Kyoto$_x$	0.01	0.06	0.01**	0.03**	-0.02	-0.05**	0.02	0.10
	(0.03)	(0.07)	(0.01)	(0.01)	(0.01)	(0.02)	(0.04)	(0.08)
Joint FTA (0,1)	0.17**	0.26	0.01	0.01	-0.01	-0.01	0.18*	0.27
	(0.09)	(0.21)	(0.02)	(0.04)	(0.04)	(0.09)	(0.09)	(0.22)
Joint WTO (0,1)	-1.09**	-2.40	-0.01	-0.13	0.02	0.19	-1.09**	-2.48*
	(0.44)	(1.47)	(0.05)	(0.16)	(0.10)	(0.31)	(0.44)	(1.51)
Joint EU (0,1)	0.76***	1.11***	0.01	0.01	-0.03	-0.02	0.77***	1.10***
	(0.12)	(0.24)	(0.02)	(0.04)	(0.04)	(0.08)	(0.12)	(0.24)
Observations	19134	3061	19134	3061	18767	3044	19134	3061
Adj. R^2	0.21	0.33	0.82	0.85	0.03	0.05	0.10	0.18
Sector:	Paper, paper products, pulp and printing							
Kyoto$_m$-Kyoto$_x$	0.15***	0.16**	0.02***	0.04***	-0.03**	-0.06**	0.17***	0.19***
	(0.04)	(0.07)	(0.01)	(0.01)	(0.01)	(0.03)	(0.04)	(0.07)
Joint FTA (0,1)	0.08	0.02	0.01	0.00	-0.01	-0.01	0.08	0.02
	(0.12)	(0.27)	(0.02)	(0.04)	(0.04)	(0.09)	(0.12)	(0.27)
Joint WTO (0,1)	0.21	0.08	-0.01	-0.07	0.02	0.08	0.20	0.13
	(0.24)	(0.93)	(0.16)	(0.12)	(0.31)	(0.21)	(0.22)	(1.00)
Joint EU (0,1)	-0.10	-0.05	0.01	0.00	-0.02	-0.02	-0.08	-0.05
	(0.11)	(0.19)	(0.02)	(0.04)	(0.05)	(0.08)	(0.11)	(0.19)
Observations	19045	3057	19045	3057	18636	3033	19045	3057
Adj. R^2	0.20	0.36	0.76	0.83	0.02	0.06	0.11	0.18

Note: Country-pair fixed effect regression (sector-by-sector). All regressions include country-and-time effects and constant (not shown). Differential Kyoto commitment takes values (-1,0,1). The method of estimation is either fixed effects (within, FE) or long fixed-effects estimation on pre- and post-treatment averages (long FE). Heteroskedacticity-robust standard errors (in brackets) are adjusted for within country-pair clustering. ***, ** and * denote statistical significance at the 1%, 5% and the 10% levels, respectively.

Table D.16: Sector-by-sector regressions: differential commitment - Detailed table cont.

Dependent variable:	Ln imports		Ln CO_2 intensity		Ln CTT		Ln CO_2 imports	
Method:	FE	long FE	FE	long FE	FE	long FE	FE	long FE
Sector:	Wood and wood products							
$\text{Kyoto}_m\text{-Kyoto}_x$	-0.10**	-0.15	0.02**	0.05***	-0.02	-0.08**	-0.08*	-0.09
	(0.05)	(0.09)	(0.01)	(0.02)	(0.02)	(0.04)	(0.05)	(0.09)
Joint FTA (0,1)	0.26*	0.33	0.02	0.03	-0.02	-0.05	0.28*	0.35
	(0.14)	(0.35)	(0.02)	(0.06)	(0.05)	(0.12)	(0.15)	(0.36)
Joint WTO (0,1)	-0.90	-1.48	-0.03	-0.11	0.03	-0.07	-0.93	-1.59
	(0.72)	(1.28)	(0.06)	(0.15)	(0.12)	(0.39)	(0.71)	(1.25)
Joint EU (0,1)	0.03	0.20	0.02	0.01	-0.02	-0.04	0.05	0.22
	(0.13)	(0.24)	(0.03)	(0.05)	(0.06)	(0.12)	(0.13)	(0.24)
Observations	17717	2992	17717	2992	16564	2901	17717	2992
Adj. R^2	0.21	0.36	0.76	0.78	0.01	0.01	0.12	0.21
Sector:	Textile and leather							
$\text{Kyoto}_m\text{-Kyoto}_x$	-0.12***	-0.19***	0.02***	0.03*	-0.04**	-0.06*	-0.09***	-0.15**
	(0.03)	(0.06)	(0.01)	(0.02)	(0.02)	(0.03)	(0.03)	(0.06)
Joint FTA (0,1)	0.24**	0.34	0.01	0.01	-0.01	-0.01	0.25**	0.34
	(0.09)	(0.20)	(0.02)	(0.06)	(0.05)	(0.12)	(0.10)	(0.23)
Joint WTO (0,1)	0.34	-0.33	-0.02	-0.11	0.02	0.13	0.33	-0.39
	(0.34)	(1.15)	(0.08)	(0.17)	(0.16)	(0.35)	(0.37)	(1.23)
Joint EU (0,1)	-0.05	0.16	0.01	0.00	-0.03	-0.04	-0.04	0.17
	(0.09)	(0.16)	(0.03)	(0.05)	(0.05)	(0.10)	(0.10)	(0.17)
Observations	19266	3070	19266	3070	18977	3056	19266	3070
Adj. R^2	0.19	0.34	0.78	0.81	0.02	0.04	0.12	0.26
Sector:	Non-specified industries							
$\text{Kyoto}_m\text{-Kyoto}_x$	-0.01	-0.02	0.09***	0.11***	-0.18***	-0.24***	0.09***	0.10**
	(0.02)	(0.04)	(0.01)	(0.02)	(0.02)	(0.04)	(0.03)	(0.05)
Joint FTA (0,1)	0.04	0.02	0.01	0.00	-0.01	-0.01	0.05	0.03
	(0.07)	(0.16)	(0.02)	(0.04)	(0.04)	(0.09)	(0.07)	(0.16)
Joint WTO (0,1)	-0.02	0.14	-0.02	-0.12	0.02	0.13	-0.04	0.05
	(0.38)	(0.96)	(0.09)	(0.23)	(0.18)	(0.47)	(0.43)	(1.03)
Joint EU (0,1)	-0.28***	-0.23*	0.02	0.01	-0.04	-0.04	-0.26***	-0.21
	(0.08)	(0.13)	(0.03)	(0.05)	(0.06)	(0.11)	(0.08)	(0.14)
Observations	19463	3074	19463	3074	19290	3066	19463	3074
Adj. R^2	0.52	0.73	0.78	0.83	0.04	0.10	0.28	0.41

Note: Country-pair fixed effect regression (sector-by-sector). All regressions include country-and-time effects and constant (not shown). Differential Kyoto commitment takes values (-1,0,1). The method of estimation is either fixed effects (within, FE) or long fixed-effects estimation on pre- and post-treatment averages (long FE). Heteroskedacticity-robust standard errors (in brackets) are adjusted for within country-pair clustering. ***, ** and * denote statistical significance at the 1%, 5% and the 10% levels, respectively.

Appendix E

Appendix to Chapter 5

Data

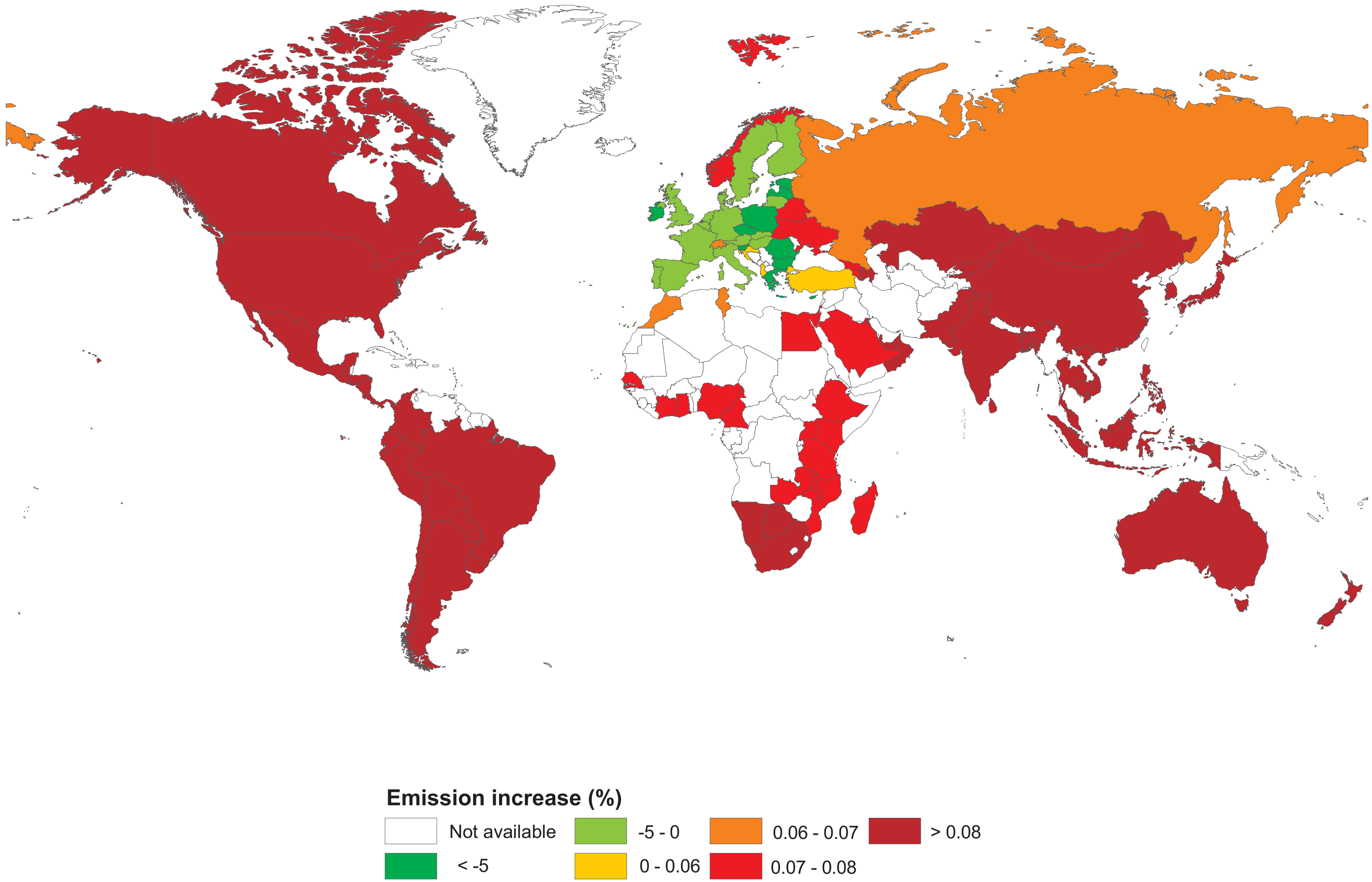
The 103 countries are: Albania, Argentina, Armenia, Australia, Austria, Azerbaijan, Bahrain, Bangladesh, Belarus, Belgium, Bolivia, Botswana, Brazil, Bulgaria, Cambodia, Cameroon, Canada, Chile, China, Colombia, Costa Rica, Croatia, Cyprus, Czech Republic, Cote d'Ivoire, Denmark, Ecuador, Egypt, El Salvador, Estonia, Ethiopia, Finland, France, Georgia, Germany, Ghana, Greece, Guatemala, Honduras, Hong Kong, Hungary, India, Indonesia, Ireland, Israel, Italy, Japan, Kazakhstan, Kenya, Kuwait, Kyrgyzstan, Latvia, Lithuania, Luxembourg, Madagascar, Malawi, Malaysia, Malta, Mauritius, Mexico, Mongolia, Morocco, Mozambique, Namibia, Netherlands, New Zealand, Nicaragua, Nigeria, Norway, Oman, Pakistan, Panama, Paraguay, Peru, Philippines, Poland, Portugal, Qatar, Republic of Korea, Romania, Russia, Saudi Arabia, Senegal, Singapore, Slovakia, Slovenia, South Africa, Spain, Sri Lanka, Sweden, Switzerland, Thailand, Tunisia, Turkey, Uganda, Ukraine, United States of America, United Arab Emirates, United Kingdom, United Republic of Tanzania, Uruguay, Vietnam, Zambia

Table E.1: Sensitivity: Heterogeneity of country effects for EUA permit price increase with OLS

Country	Emission effects			Trade share with non-EU countries				Real
	Total	Tech-nique	Scale	Goods		Emissions		GDP
				Import	Export	Import	Export	
	ΔE_i	$\Delta \eta_i$	Δq_i	$\Delta \frac{X_{ji}}{Y_i}$	$\Delta \frac{X_{ij}}{Y_j}$	$\Delta \frac{\eta_j' X_{ji}}{E_i}$	$\Delta \frac{\eta_i' X_{ij}}{E_j}$	$\Delta \frac{Y_i}{P_i}$
Estonia	-12.07	-10.86	-1.28	0.09	-0.94	12.49	-1.04	-1.08
Poland	-8.53	-7.69	-0.87	0.31	-0.53	8.65	-0.60	-0.89
Czech Republic	-8.32	-7.49	-0.86	0.35	-0.53	8.50	-0.60	-0.93
Slovenia	-8.20	-7.80	-0.41	0.16	-0.23	9.07	-0.30	-0.42
Bulgaria	-7.76	-6.04	-1.77	0.18	-1.33	6.90	-1.39	-1.54
Latvia	-6.84	-6.38	-0.47	0.01	-0.28	7.03	-0.34	-0.32
Cyprus	-6.42	-5.53	-0.92	0.48	-0.55	6.00	-0.58	-1.08
Malta	-6.15	-5.36	-0.81	0.26	-0.42	6.09	-0.48	-0.77
Romania	-5.68	-5.14	-0.55	0.35	-0.30	5.75	-0.34	-0.70
Ireland	-5.57	-5.35	-0.23	-0.01	-0.10	5.88	-0.15	-0.12
Greece	-5.34	-4.31	-1.05	0.30	-0.59	5.28	-0.63	-0.97
Luxembourg	-4.92	-4.61	-0.32	0.08	-0.15	5.30	-0.20	-0.27
Denmark	-4.64	-4.34	-0.30	0.05	-0.13	4.75	-0.17	-0.21
Spain	-4.10	-3.79	-0.32	0.19	-0.14	4.21	-0.18	-0.37
Hungary	-3.69	-3.23	-0.47	0.00	-0.24	3.64	-0.26	-0.28
Portugal	-3.59	-3.24	-0.36	0.13	-0.16	3.56	-0.19	-0.33
Slovak Republic	-3.56	-3.07	-0.50	0.20	-0.27	3.31	-0.30	-0.52
Germany	-3.47	-3.22	-0.25	0.09	-0.10	3.51	-0.13	-0.23
Finland	-3.35	-2.99	-0.37	0.10	-0.20	3.28	-0.22	-0.32
United Kingdom	-3.24	-3.04	-0.20	0.07	-0.08	3.37	-0.10	-0.17
Italy	-3.24	-2.99	-0.25	-0.06	-0.10	3.26	-0.13	-0.07
France	-2.77	-2.59	-0.18	0.26	-0.06	2.88	-0.08	-0.34
Austria	-2.75	-2.59	-0.17	0.23	-0.01	2.71	-0.03	-0.28
Lithuania	-2.74	-2.30	-0.44	0.17	-0.23	2.59	-0.24	-0.43
Belgium	-2.31	-2.01	-0.30	0.03	-0.15	2.21	-0.16	-0.21
Netherlands	-1.95	-1.74	-0.21	0.16	-0.07	1.53	-0.08	-0.26
Sweden	-1.91	-1.78	-0.13	-0.02	-0.03	1.79	-0.04	-0.04
EU average	-4.93	-4.43	-0.52	0.15	-0.29	4.95	-0.33	-0.49

Note: The policy experiment is an EUA permit price increase of 15 US-$. Results based on OLS estimations. The table shows percentage changes of the respective variables. Index $i \in$ EU refers to the respective EU country. For trade effects, $j \notin$ EU indexes non-EU countries.

Figure E.1: Country-specific emission increase from EUA permit price increase with OLS estimation



Note: The graph shows countries' emission increase (in %) due to emission relocation, i.e. $\Delta \bar{E}_i$, resulting from an EUA permit price increase of 15 US-$ using the default method; i.e. OLS estimation and $\hat{\sigma} = 4.801$.

Curriculum Vitae

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