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Working Papers

What a Difference Kyoto Made: Evidence from Instrumental Variables Estimation

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Ifo Working Paper No. 102

June 2011

An electronic version of the paper may be downloaded from the Ifo website
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Abstract

The Kyoto Protocol's success or failure should be evaluated against the unobserved counterfactual of no treatment. This requires instrumental variables. We find that countries' membership in the International Criminal Court (ICC) predicts Kyoto ratification in a panel model. Both multilateral policy initiatives triggered concerns about national sovereignty in many countries. We argue that ICC membership can be excluded from second-stage regressions explaining emissions and other outcomes. This is supported by first-stage diagnostics. Our results suggest that Kyoto had measurable beneficial effects on the average Kyoto country's energy mix, fuel prices, energy use and emissions, but may have speeded up deindustrialization.

JEL Code: C26, Q48, Q54.
Keywords: CO_2 emissions, energy, evaluation model, instrumental variables, Kyoto Protocol.

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*We are grateful to Peter Egger, Mario Larch, Mary Lovely, Devashish Mitra, David Popp, and M. Scott Taylor for comments, and to the German Science Foundation (DFG) for financial support (grant no. 583467).
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1. Introduction

For many observers the Kyoto Protocol has been a failure. In 2011, many countries are still far from achieving their promised carbon dioxide (CO_2) emission reductions. This does, however, not imply that the Kyoto Protocol has been completely futile. In order to evaluate the effect of Kyoto commitments on environmental or economic outcomes, one would need to compare the status quo to the *counterfactual* situation of no climate deal. This counterfactual is, of course, not observable. And selection of countries into the Protocol is most likely non-random. But using instrumental variables (IV) to model the selection of countries into commitments under Kyoto, it is possible to approximate the causal effect of Kyoto. This paper presents such an instrument and uses it to evaluate the effect of Kyoto on countries' emission levels, energy variables, and macroeconomic outcomes.

The objective of the Kyoto Protocol is to limit anthropogenic greenhouse gas (GHG) emissions. 37 industrialized nations and the European Union (EU) have agreed to cap their levels of overall GHG emissions to an average of 94.8% of their 1990 emissions by the period 2008-12.[1] In 2007, only four out of the 24 committed non-transition countries have emissions smaller than their 2008-12 objectives. These countries are Germany, Great Britain, France, and Sweden. 12 out of the 14 transition countries will easily reach their targets.[2] This is due to industrial restructuring prior to signing the Protocol in 1997. After 1997, in seven transition countries, emissions have been increasing again. In 2007, all committed countries together have emissions standing at 98.1% of 1990 levels (3.3 percentage points above target), despite large reductions in transition countries.

The Kyoto agreement lacks a convincing *formal* enforcement mechanism. This may explain why achieved reductions have so far been disappointing. Yet, to the extent that Kyoto has established a floor to the price of CO_2 emissions, or has given rise to the expectation of such a floor, it has added incentives to save on the use of fossil fuel. This is so for the EU Emissions Trading System but holds more generally also for the flexible mechanisms under Kyoto. Moreover, informal enforcement – for example through *naming and shaming* – may still have an effect. The United Nations Framework Convention on Climate Change (e.g. UNFCCC, 2009) summarizes

[1]The regulated GHGs are CO_2, CH_4, N_2O, PFCs, HFCs and SF6.

[2]For instance, in 2007, the Ukraine has emitted only 47% of its target level. Countries with similar slack are Belarus, Estonia, Latvia and Slovakia.

emission reduction achievements in annual reports. Non-achievers are criticized in the press and by international organizations. For example UNDP (2007, p. 10) points out Canada as a negative example of missing its target in a Human Development Report. And in a recent report for the Canadian International Council, the authors argue: *"[...] That said, the successful implementation of the Copenhagen Accord is arguably more critical for Canada than for any other country, offering a potential opportunity to shift the focus from Canada's not meeting its Kyoto obligations to an interpretation where Canada plays a role as a constructive contributor to the new accord."* (Drexhage and Murphy, 2010, p. 4). The lack of formal sanctions has 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.[3]

In this paper, we argue 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) explain the parallels between 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.[4] 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. However, to the extent that ICC membership proxies general preferences for multilateralism, it is essential to account for the economic component by controlling for membership in the World Trade Organization (WTO).

Our approach consists of two major steps. First, we use a panel of about 150 independent countries to model the selection of countries into Kyoto commitments. While GDP per capita

[3]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.

[4]Multilateral environmental agreements cannot be used as instrumental variables since they are likely to represent 'green' preferences and so directly influence environmental outcome variables. Other multilateral agreements such as the Anti-Personnel Land Mines Convention also correlate to Kyoto status, but cannot predict the timing of ratification.

is a strong predictor, there is no automatic link: for example, rich countries such as the U.S., Singapore, South Korea, or Israel have chosen not to commit to an emission target. We find that ICC membership robustly correlates to ratification of Kyoto commitments. Also, the spatial lag of ICC membership makes Kyoto commitments more likely. Other exogenous variables such as geographical remoteness reduce the likelihood of Kyoto commitment. These variables explain about 50% of the variation in commitment.

In the second step, we use our instruments to estimate the effect of Kyoto commitment on a host of environmental and economic outcome variables. Using a fixed-effects panel approach on yearly data, or, alternatively, a long fixed-effects model on pre- and post-treatment averages, we find that, in the long-run, Kyoto commitment has indeed reduced CO_2 emissions by about 10%, but statistical significance of the effect is marginal in some of the estimates. Kyoto has also led to restructuring of the energy mix: committed countries increase the share of alternative energy sources, and shift away from fossil fuels. We find that Kyoto commitment causes an increase in the prices of gasoline and diesel fuel. We do not detect any robust and significant effect on GDP per capita, net exports, or energy imports. However, there is some evidence that Kyoto commitment appears to accelerate deindustrialization: the share of manufacturing in GDP falls by about 2 percentage points.

Related literature The environmental and public economics literature addresses the question why countries form multilateral environmental agreements (MEA) and why some countries choose not to join (for examples, see Carraro and Siniscalco, 1998; Beron et al., 2003). The focus lies on models of strategic interactions, coalition formation and free-riding. Beron et al. (2003) empirically test for free-riding and spillover effects in the ratification process of the Montreal Protocol which regulates ozone depleting substances. In their interdependent Probit model they include GDP per capita, initial emissions, development status and political freedom to explain ratification. This list of explanatory variables is similar to ours.

Copeland and Taylor (2005) focus attention to implications from international trade. In their theoretical model, an emission cap in an open Heckscher-Ohlin economy affects other countries' emissions via free-riding, income and terms-of-trade effects. In contrast to previous work, the authors find that, in the presence of trade, climate policy can also be a strategic complement instead of a strategic substitute. Note that, within their framework, membership in Kyoto is assumed to be given. Egger et al. (2011) empirically test the relevance of international trade relations as determinant for MEAs. Conversely, Rose and Spiegel (2009) argue that economic outcomes are influenced by MEA memberships. A country's memberships in MEAs signal its discount rate and will therefore be beneficial for fostering international economic relations (i.e., the conclusion of free trade agreements). They use the Polity score from the Polity IV Project as an instrument for MEA membership and find a positive effect on FDI stocks and banking claims.

The paper most strongly related to ours is probably Aakvik and Tjøtta (2011) who empirically estimate the effect of the Helsinki and Oslo Protocols on the reduction of sulfur dioxide emissions. In contrast to us, they focus on emissions alone but do not find statistically significant effects. This may be due to the fact that they do not instrument ratification of the Protocols. Also, the Kyoto Protocol goes farther than the other Protocols in setting savings incentives. While carbon dioxide emission savings are equivalent to real cost savings for firms as they burn less fuel, this is not true for sulfur emissions.

Finally, our paper is loosely related to the carbon Kuznets curve literature, for a survey see Dinda (2004) or Galeotti et al. (2006). This literature estimates a dynamic relationship between development (measured by GDP per capita) and CO_2 emissions per capita. The purpose of those papers is to estimate the 'turning point' beyond which further GDP per capita growth lowers emissions per capita. The mechanism is driven by structural change and non-homothetic preferences. However, this is not the focus of our work. We are interested in the *causal effect* of a specific policy (commitment to the Kyoto Protocol) on outcomes and not in *explaining* emissions. 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 obligations reduces CO_2 emissions. However, they treat Kyoto commitment as an exogenous variable so that their results cannot necessarily be interpreted as causal.[5]

The rest of the paper proceeds as follows. Chapter 2. econometrically analyzes the determinants of a country's commitment to the Kyoto Protocol, thereby describing the first-stage regression in our IV approach. Chapter 3. presents the second-stage IV results of the effect of

[5]It turns out that their results on emissions are not too different from ours, despite our use of IV methods. A major difference, we look at a host of dependent variables rather than just on emissions.

Kyoto commitment on several environmental and economic outcome variables. The last chapter contains concluding remarks.

2. Who made commitments under Kyoto?

2.1. Measurement

The Kyoto Protocol was signed in 1997 and its ratification process started in the early 2000s. It is reasonable to assume that the Protocol starts to matter once ratification through the parliament has occurred. Ratification involves political parties, the media, and the general public, while the signature of the Protocol directly following negotiation in 1997 had no immediate political relevance. The Protocol entered into force only in 2005 (after the ratification of Russia), but it is ratification that sets the relevant domestic policy parameters. Hence, we define Kyoto commitment as a dummy variable 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.[6] It takes the value of zero otherwise.

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

This definition does not attempt to measure the stringency of commitment but has the advantage of simplicity. Uncertainty as to the exact timing of the Kyoto constraints inserts measurement error into the Kyoto dummy variable and biases coefficients towards zero.

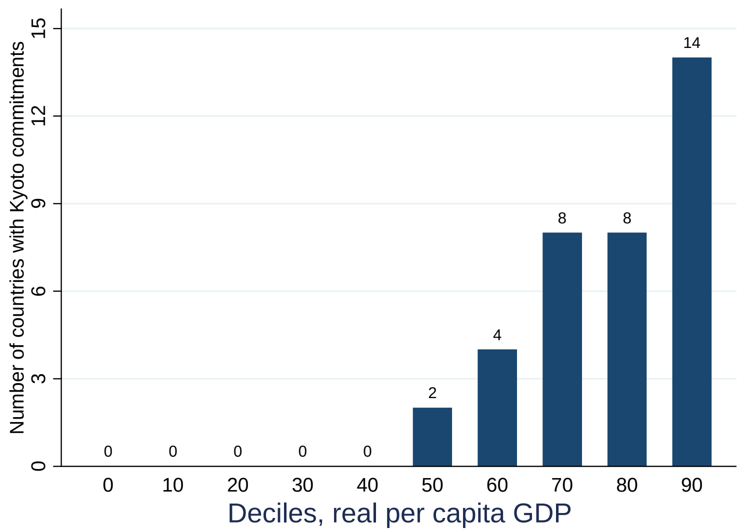
2.2. Selection into Kyoto: descriptives

Common but differentiated responsibilities In our sample of 151 independent countries, 36 have commitments under the Kyoto Protocol.[7] Arguing that rich developed countries are principally responsible for the current levels of CO_2 concentration in the atmosphere due to more than 150 years of industrial activity, the Protocol places a heavier burden on developed nations under the principle of *'common but differentiated responsibilities'*. However, in practice, this does not at all mean that there is a direct link between per capita GDP and Kyoto status. Fig-

[6] We refer to these Annex B countries which have ratified Kyoto as 'Kyoto countries'.
[7] One Kyoto country (Liechtenstein) is not included in our sample due to data availability.

ure 1 counts the number of 151 independent non-OPEC countries with commitments (ratified by national parliaments) as of the year 2007 in different per capita income groups. Out of the 16 countries with real per capita income above the 90% percentile, 14 countries have commitments (Singapore, and USA have none). Out of the 15 countries with per capita income above the 80% percentile but below the 90% percentile, 8 countries have commitments (Bahamas, Barbados, Cyprus, Israel, South Korea, Trinidad and Tobago, and Taiwan have none). Interestingly, Cyprus and Malta, two EU member states, have no Kyoto commitments.

Figure 1: Kyoto countries and income groups



Data source: Penn World Table 6.3, series rgdpch. Real GDP per capita in PPP-adjusted dollars.

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 and Switzerland (2003), Russia and Ukraine (2004), Belarus (2005) and finally Australia and Croatia (2007) followed.

Unconditional correlations These simple observations suggest that there is non-trivial heterogeneity across countries with respect to their Kyoto status. In this section, we draw on the theoretical literature (in particular Copeland and Taylor, 2005; Beron et al., 2003) to explain the observed variation. The analysis is deliberately exploratory. Ultimately, the aim is to identify determinants of Kyoto commitments that are exogenous and not directly related to outcomes such as the level of CO_2 emissions per capita.

The sample comprises a varying number of independent non-OPEC countries from 1997 to 2007.[8] Table 1 reports summary statistics and data sources. In a first step we look at countries in the year of 2007 and explain their Kyoto status using their characteristics as of 2007 or lagged versions thereof. Figure 2 plots beta coefficients[9] of these unconditional correlations for various explanatory variables. Important determinants of Kyoto membership are, amongst others, a country's green preferences proxied by the number of ratified MEAs, EU membership, being in the neighborhood of other (large) Kyoto countries (i.e. having a high value of the spatial Kyoto lag), lagged GDP per capita growth, and lagged emissions per capita growth. Note that the lagged log of emissions per capita is positively correlated with Kyoto commitment. At a first take this seems implausible. But given that emissions are correlated with GDP, this unconditional correlation partly picks up that Kyoto countries are economically large, industrialized nations as well as that countries with low initial per-capita emissions, such as developing countries, might expect emission increases in the future. Also, countries that have ratified other international agreements such as the Rome Statute governing the International Criminal Court, the Anti-Personnel Landmine Convention (APLC) or the Comprehensive Nuclear Test-Ban-Treaty (CTBT) are more likely to be Kyoto countries.

2.3. Selection into Kyoto: Regression analysis

Model The analysis in Figure 2 is suggestive. However, for the purpose of understanding the effect of Kyoto commitment on economic and environmental outcome variables, one requires an understanding of the selection process in a panel setup. Therefore, we estimate the following *linear probability fixed-effects model*[10] for Kyoto commitments for the time span 1997-2007:

$$Kyoto_{it} = \alpha + \boldsymbol{\gamma}\mathbf{X}_{it}^{'} + \boldsymbol{\zeta}\mathbf{Z}_{it}^{'} + \boldsymbol{\nu}_t + \boldsymbol{\nu}_i + \upsilon_{it}, \tag{2}$$

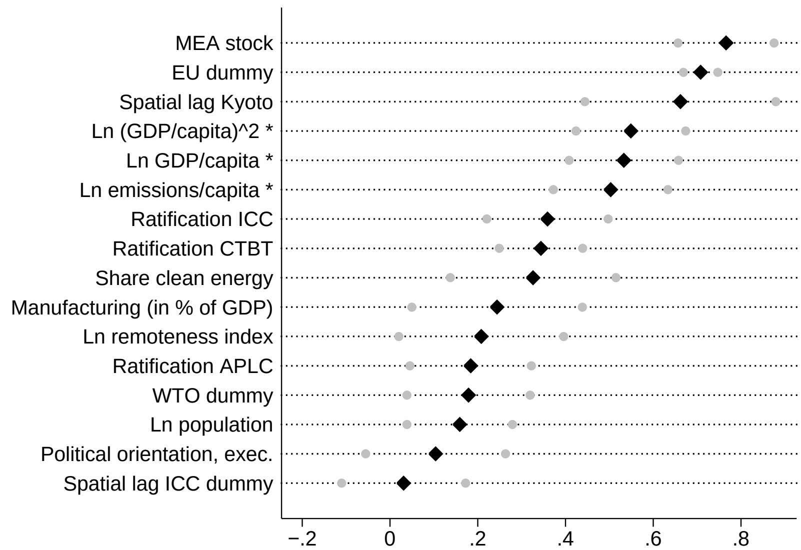
where $\mathbf{X}_{it}$ is a vector of controls which are likely to matter for Kyoto commitment and outcomes alike and $\mathbf{Z}_{it}$ is a vector of instruments that do not directly affect outcomes. To cap-

[8] We take all available countries into account.

[9] Beta coefficients give the standard deviation change in the dependent variable per standard deviation increase in the explanatory variable. The larger the beta coefficient the greater is the effect of the explanatory variable.

[10] Note that there is no fixed-effects estimator for the Probit model.

Figure 2: Simple correlations of Kyoto status and its determinants



Note: The graph shows beta coefficients (diamonds) $\pm$ 1.96 standard deviations (circles) of an OLS regression of Kyoto status on the respective variable and a constant in a cross-section of 2007. The constant is not reported. Varying sample sizes. * indicates variable is lagged 5 years.

ture cyclical elements (such as the oil price or the world business cycle) we include a full set of year dummies ν_t into all regressions. ν_i is a full set of country dummies (fixed effects) which we eliminate through within-transformation of the data and υ_{it} is the error term. We use a heteroskedasticity-robust estimator of the variance-covariance matrix and allow for clustering at the country level in order to deal with serial correlation in error terms. The advantage of using a panel fixed-effects estimator is that one can make use of country and time variation simultaneously. Moreover, country-specific time-invariant determinants of Kyoto commitments are fully controlled for.[11]

GDP per capita and carbon emissions Table 2 reports our main results on selection. Column (1) uses *GDP per capita, GDP per capita squared, and the level of carbon emissions per capita* to explain Kyoto status. The effect of GDP per capita is non-monotonic. Doubling GDP per capita (from the median level) increases the likelihood of Kyoto commitment by about 48 percentage points.[12] At the same time, higher CO_2 emissions per capita reduce the odds. The first finding

[11] We have also worked with a cross-section; results are comparable and available upon request.
[12] Median log GDP per capita is 8.75, so that the effect of doubling log GDP per capita is $-3.54 + 2 \times 0.23 \times 8.75$.

relates to the common but differentiated responsibilities principle. It can also be rationalized by mitigation policy being a superior good in voters' utility functions. This is the case in the model of Copeland and Taylor (2005). The second suggests that – for given income levels – countries with high emissions per capita probably shy away from higher costs of bringing per capita emissions down to average levels. These two variables (together with the time trend variables) explain almost 30% of total within variation. The two variables are, of course, potentially endogenous to Kyoto status so that the obtained coefficients cannot be interpreted as causal effects.

Geography and country size Time-invariant geographical controls such as climate or resource endowment are accounted for by the within-transformation of the data. In contrast, *geographical remoteness*, calculated as the population weighted average geographical distance of a country to all the other countries, varies across time. More remote countries trade less and are less integrated in the world economy. This measure is strongly negatively correlated to standard openness measures (exports plus imports over GDP). Column (2) shows that more remote countries are less likely to have Kyoto commitments. Own population size also negatively affects the odds, but is not significant. Since we identify all effects in Table 2 by time variation at the country-level, our results suggest that countries with higher own population growth or higher population growth in nearby countries are less likely to have commitments.

Preferences Next, we include two variables to proxy for *green preferences*. The first, the country's chief executive party's political orientation (taken from the World Bank's Database of Political Institutions 2010) takes the value of 0.1 if the executive is from the right, the value of 0.2 if it is from the center, and the value of 0.3 if it is from the left. One would think that left-leaning governments are more likely to accept commitments, but this does not show up in our regression. The second variable, the log stock of other (than Kyoto) MEAs is also expected to affect the likelihood of commitment positively, but does not show up significantly in the analysis.[13]

International Criminal Court membership Column (4) adds countries' membership in the *International Criminal Court* and the spatial lag thereof to the list of explanatory variables.

[13] These variables show up with the expected sign in the cross-section of countries.

These variables fill the vector $\mathbf{Z}_{it}$. 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).[14] The coefficient to this dummy suggests that countries having ratified the Rome Statute have a 13 percentage points higher likelihood to have a commitment under Kyoto, given their development status and current level of CO_2 emissions per capita. An increase by one standard deviation of the spatial lag of ICC membership boosts the odds of Kyoto commitments by about 3.2 percentage points.[15]

WTO and EU Column (5) adds a dummy for membership in the WTO as another proxy for trade openness and the country's degree of multilateralism. That dummy does not feature much variation over the time period; 13 countries have joined the WTO from 1997-2007; only three out of them also have Kyoto commitments (Croatia, Lithuania, Latvia).[16] It is therefore not surprising that WTO membership negatively correlates to Kyoto commitment. The EU dummy has the opposite sign: ceteris paribus, joining the EU increases the odds of Kyoto membership by about 19 percentage points. This estimate is driven by the Eastern enlargement of the Union. Note, however, that not all new EU members are Kyoto members as well (Cyprus and Malta are not).

Industrial structure and energy mix Column (6) adds the share of renewable energy in total energy production of a country. This variable is available only for a subsample of countries. It is positively related to the propensity of Kyoto commitments, but the statistical precision of estimation is very low. Other indicators of the energy mix of countries fare similarly. Also, adding the share of manufacturing in total GDP has a negative effect – as one may expect – but the effect is not statistically significant. Using time lags of these variables does not change the situation.

Preferred specification Column (7) is our preferred specification. It is similar to column (5) but adds the spatial lag of Kyoto commitment. The more nearby countries have commitments,

[14]The exact calculation of the spatial lag does not make a significant difference.
[15]Using the ICC variables without further controls yields comparable results.
[16]Other new members are former USSR states (Armenia, Georgia, Moldova), Asian countries such as China, Vietnam, Cambodia, Nepal, as well as Jordan and Oman.

the higher the likelihood that a country has a commitment, too. Since unilateral mitigation policies contribute to a public good, other countries' efforts may decrease own efforts. On the other hand, other countries' commitments lower negative competitiveness effects of own climate policies. The variable turns out to be positively associated to Kyoto status: when large nearby countries commit to climate targets, commitment of a specific single country becomes more likely. However, the effect is not overly large. With a mean of 0.13, the spatial lag adds on average about 5 percentage points to the likelihood of Kyoto commitment. Column (7) is rather successful in explaining the variation in Kyoto commitments, the within-R^2 is almost 60%.

First-stage regression Specification (7) is the nucleus of the first-stage instrumental variables regression that we employ in the next section. The regressions in Table 2 show that Kyoto and ICC membership are highly correlated. This is probably due to the fact that both treaties pose a threat to national sovereignty.[17] So, the ICC status can be seen as a proxy for a country's stance on multilateralism. And indeed, prominent non-Kyoto countries such as China, USA, Israel, Singapore or Thailand have not ratified the Rome Statute.

Moreover, the size of the country and its geographical position are likely to have direct implications for outcome variables such as CO_2 emissions per capita, their carbon taxes or their share of manufacturing in GDP, but a *country's own and its neighbors' membership to the International Criminal Court* can be taken as exogenous and excludable from regressions that aim at explaining emission outcomes. Countries do not sign up to the ICC because of their CO_2 emissions per capita, their carbon taxes or their share of manufacturing in GDP. Ratification of the Statute of the ICC does not directly affect outcome variables neither. We further argue that ICC membership is uncorrelated with omitted variables in the outcome equation (e.g. technology or development status), once we control for a country's institutional environment e.g. through the Polity index.

Hence, it appears that the two Rome Statute variables in specification (7) (i.e., own ICC membership and that of close countries) are good candidates for instrumental variables: they can be excluded from the second-stage regression, they do not cause the outcome, are uncorrelated with the error term, and they correlate to Kyoto status.[18] An F-test of joint significance of

[17]For example, Groves (2009, p. 1) likens the Kyoto Protocol to the Rome Statute and argues that *"[...] the proposed "Kyoto II" successor agreement [...] poses a clear threat to American sovereignty. This threat is primarily due to the nature of the proposed treaty – a complex, comprehensive, legally binding multilateral convention."*

[18]We have also experimented with other multilateral agreements such as the CTBT or the APLC. Ratification of those

the excluded variables yields a test statistic of 11.65 and a p-value of 0.00, thereby meeting the rule of thumb by which an F-statistic larger than 10 (for a single endogenous regressor) avoids the weak instrument problem (Stock and Yogo, 2005).

Robustness checks Columns (8) to (11) perform some robustness checks. So far, in the regressions of this table we have excluded the OPEC countries from the sample. Column (8) adds those 12 countries; point estimates and levels of statistical significance do not change much. Column (9) uses 5 year time lags of variables that are potentially endogenous to Kyoto commitment: log GDP per capita and its square, and the log emissions per capita. Relative to the benchmark regression of column (7), this affects the point estimate of the spatial lag of the ICC variable, but has otherwise very little effect. Column (10) excludes all countries with income below the median, none of which has a Kyoto commitment. With this smaller sample, we still find a statistically significant influence of ICC membership. Not surprisingly, GDP per capita ceases to be important. Column (11) excludes all countries with populations below 5 million. This halves the sample size, but the ICC variables remain significant. Finally, column (12) defines the dependent variable differently: it codes as a zero a situation where a country has no obligations under Kyoto, as 1 if the country has a slack cap, and as 2 if the country has a cap that has been non-slack in the year of 1990. Relative to the benchmark regression, this model with *'Kyoto stringency'* as dependent variable has a slightly better R^2, but otherwise fares very similarly. Most importantly, the ICC variables continue to work.

3. The effects of Kyoto commitments on outcomes

3.1. Econometric issues

We are interested in understanding the effect of legally binding Kyoto commitments on a series of outcome variables such as countries' carbon emissions, energy mix, fuel prices, and macroeconomic variables. To this end, we estimate fixed-effects models to control for unobserved heterogeneity across countries, such as endowments with fossil fuels, patterns of comparative advantage, or climatic and geographic conditions. We also include a full set of year dummies to

texts also tends to make Kyoto commitments more likely; however, the effects are weaker and less statistically significant.

control for the world business cycle and the price of oil. The baseline estimating equation takes the following form:

$$Y_{it} = \beta_0 + \beta_1 Kyoto_{it} + \boldsymbol{\beta}_2 \mathbf{X}'_{it} + \boldsymbol{\alpha}_t + \boldsymbol{\alpha}_i + \varepsilon_{it}, \tag{3}$$

where Y_{it} is the outcome variable. α_i denotes a vector of country-specific fixed effects which we eliminate by within-transformation of the data. α_t is a vector of year dummies and $\mathbf{X}_{it}$ is a vector of controls. In most regressions, X_{it} contains the spatial lag of Kyoto commitment, the log of GDP and its square, the log of population, economic openness, a country's political orientation measured by the chief executive's party affiliation, dummy variables for WTO and EU membership, and the Polity index. In some regressions, we add the log of the nominal exchange rate to the list of covariates. Whenever sensible, we use 5th time lags of GDP, openness and the exchange rate to avoid spurious contemporaneous correlations. We adjust the variance-covariance matrix for heteroskedasticity and for clustering of standard errors within countries (for example, serial correlation).[19] The latter adjustment strongly increases standard errors and makes it much harder to find statistically significant coefficients. Note that equation (3) is not meant to investigate the importance of specific explanatory variables for special left-hand-side variables. Rather, the focus lies on β_1, the Kyoto coefficient; the other included variables are controls. Compared to Aakvik and Tjøtta (2011) study on sulphur emissions, we include more controls but do not use linear and quadratic country-specific time trends.[20]

One complication that arises when estimating equation (3) is that ordinary least squares (OLS) estimates could be biased for several reasons: (i) reverse causality and closely related selection into treatment, (ii) omitted variables and (iii) measurement error. 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. Depending on the correlation of the omitted variable with Kyoto status, the resulting bias could be positive or

[19] Stock and Watson (2008) recommend that, if serial correlation is suspected, correcting for clustering is essential to ensure consistency of the estimated variance-covariance matrix.

[20] Their data set has a much longer time dimension ($T = 43$) and a much smaller cross-sectional dimension ($N = 30$). In our work, with $T = 11$ and $N = 150$, including these trends is very costly in terms of degrees of freedom. Including region-specific time trends leaves our main results unchanged.

negative. Third, ratification of the Kyoto Protocol is only a proxy for a country's climate policy activities. The resulting *measurement error* will bias the estimates toward zero. With these considerations in mind, we do not know *a priori* the direction of the bias in OLS estimates. Instrumenting for Kyoto status can cure these biases. In chapter 2. we argued that membership to the International Criminal Court and its spatial lag correlate with Kyoto commitments and are likely to be exogenous to the variables of interest such as emissions, energy consumption, fuel prices or macroeconomic outcomes. In the following we use these instruments in an array of regressions.

In the following we study the effect of Kyoto commitment on different outcome variables in two steps. For each area of interest, we start with the simplest possible differences-in-differences (diff-in-diff) setup, where the period average 1997-2000 is compared to the average for 2004-07 (with ratification of Kyoto between these intervals) for the group of Kyoto and non-Kyoto countries. We make the results visible in simple pictures. In the second step, we show fixed-effects regressions of (3) based on yearly data and apply our instrumentation strategy.

3.2. CO_2 emissions

The evidence in a diagram We start with investigating the role of Kyoto commitments for CO_2 emissions in the simplest possible unconditional diff-in-diff framework.[21] The left-most diagram in Figure 3 shows the change in the log of 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 2004-07 (following the ratification of Russia and Ukraine in 2004). Ratification occurred later only in Belarus (2005), Australia and Croatia (2007). Between the two periods, emissions have increased on average by 23.8% in the group of non-committed countries while they have increased on average by 4.3% 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 Afghanistan or Burundi), and increased strongly in countries recovering from crises (such as Chad or Angola). In the group of committed countries, Luxembourg, Norway, and Spain have increased emissions by more than 20%, while they fell slightly in Belgium or Germany. Ob-

[21] CO_2 emissions data is taken from the World Bank's World Development Indicators 2010 and comprises emissions stemming from the burning of fossil fuels and the manufacture of cement. They include carbon dioxide produced during consumption of solid, liquid, and gas fuels and gas flaring.

servations cluster strongly around the means (marked by the end points of the line). Fitting a linear regression $\Delta \ln EM_{it} = const. + \beta Kyoto_{it} + \upsilon_{it}$ into the cloud reveals a coefficient of -0.19, statistically different from zero at the one percent level of significance.[22]

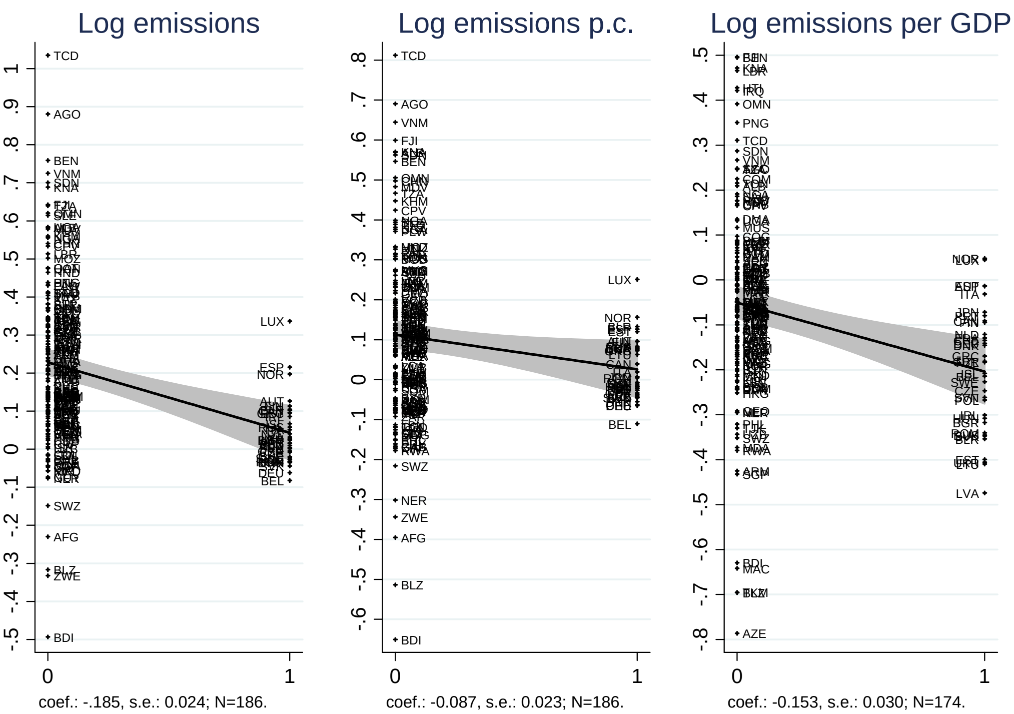
The middle diagram in Figure 3 repeats this exercise for log emissions per capita while the right-most diagram looks at log emissions per unit of GDP (emission intensity). Both scatter plots hint toward a downward-sloping relationship between emissions and Kyoto status. Interestingly, adjustment for population change does not majorly change the ranking of countries in both groups. Emission intensity has been falling in most committed countries, most strongly in formerly communist transition countries. Note, however, that this is driven not by falling emissions but by fast rising levels of GDP. In 1997, the year at which we start the analysis, most of the emission-saving industrial restructuring away from old carbon-intensive technologies and heavy industries toward cleaner technologies and services had already happened.

OLS regression results Table 3 presents more complete econometric results. It runs versions of equation (3) to generalize the analysis of Figure 3. The regressions differ from the figure by using yearly data and within-transforming the data rather than first-differencing it.[23] Columns (1) and (2) present OLS estimates. They differ with respect to the time span covered: column (1) spans 1990-2007 while column (2) covers our preferred window 1997-2007 (symmetric around most countries' ratification date). Column (1) shows that, holding population (and time-invariant country characteristics) fixed, emissions are positively correlated to log GDP but negatively to its square. Evaluated at the mean of log GDP, the elasticity of emissions with respect to GDP is 0.63. The elasticity remains positive for all observed GDP levels. The elasticity of emissions with respect to population is statistically indistinguishable from unity. Turning to column (2), which draws on a shorter time series, squared GDP is no longer statistically significant. Also the population elasticity is somewhat smaller, while still statistically identical to unity (p-value of the Chi^2-test is 0.31). These findings square well with the literature.[24]

Interestingly, Kyoto status correlates negatively to log emissions in both columns. The effect is somewhat more pronounced in the longer panel than in the shorter: Kyoto commitment is

[22]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 coefficients are typically smaller. The same holds also true for the analyses in Figures 4 to 6.
[23]Results on long first-differences are presented in Section 3.6..
[24]See, e.g., Dinda (2004) and Cole and Neumayer (2004).

Figure 3: Change in log emissions



Note: The diagrams show the change between pre- (1997-2000) and post-treatment (2004-07) averages for non-Kyoto (0) and Kyoto countries (1).

associated to a reduction of emissions by 9.26 and 6.08%, respectively. In the longer panel, the spatial lag of Kyoto commitment is insignificant, while in the longer panel, there is evidence that neighboring countries' Kyoto commitment actually drives up own emissions. Yet, the effect is small: evaluated at the mean, foreign commitment drives up emissions by 0.63%.[25] This is, of course, an average, and may hide substantial cross-country heterogeneity.

The OLS regressions in columns (1) and (2) also suggest that green preferences – proxied by the log of the stock of other MEAs – affect emissions negatively. Again, somewhat unexpectedly, the political orientation of the chief government executive matters for emissions: right-wing governments have 3.5% lower emissions than left-wing governments.[26] EU membership correlates negatively to emissions: in the short panel (column (2)), EU countries' emissions are on average 5% lower. Trade openness, as proxied by the sum of exports plus imports over GDP and

[25] $100 \times 0.0234 \times 0.270$.

[26] -0.002×17.33. This surprising finding is not driven by correlation between the political and the business cycle, since we control for GDP. It is more likely driven by policy implementation lags.

the WTO dummy does not appear to affect emissions.[27] The degree of democracy of a country, as captured by the Polity measure, does not directly matter, neither.

IV regression results To get the *treatment effect* of Kyoto commitment, we instrument for the Kyoto dummy with the ICC membership dummy and its spatial lag. Column (3) of Table 3 reports the results for the 1997-2007 panel. Kyoto appears to reduce emissions by 11% on average. This is slightly more than the OLS estimates have suggested. Hence, measurement error overcompensates simultaneity bias. This is probably not surprising, given the fact that our Kyoto dummy is only a very imprecise proxy for countries' true emission mitigation policies. Compared to the OLS estimates in column (2), the coefficients on the other covariates are not very different and the pattern of statistical significance is the same. Column (4) repeats column (3) but drops insignificant covariates. The point estimate of the Kyoto dummy barely changes. Since GDP may be endogenous to Kyoto commitment if costly climate policy reduces GDP per capita, reverse causation would bias our results. Since the coefficient on log GDP in column (4) is statistically identical to unity (p-value of the Chi^2 test is 0.51), one can subtract log GDP from the right- and left-hand-sides so that log GDP disappears from the list of covariates in the equation. Column (5) does this and finds a Kyoto effect of again -10%, albeit with somewhat reduced statistical significance (p-value 0.09).

For the IV regressions (3) to (5), the first-stage diagnostics signal instrument validity. Tests for overidentifying restrictions yield p-values of the Hansen j-statistic between 0.19 and 0.36. Those tests do not reject the null that the instruments are exogenous. The F-statistics for weak identification range between 17.15 and 19.90, well above the Staiger and Stock (1997) rule of thumb of 10. According to Stock and Yogo (2005), the implied true size of the F-test is 10%, so that we do not face a weak instrument problem. In the face of weak instruments, those authors recommend the use of limited information maximum likelihood (LIML). In the context of specifications (3) to (5), LIML does not change the point estimates of the Kyoto effect, but improves the power of the weak identification test.

Robustness checks Columns (6) to (12) perform robustness checks. Columns (6) to (8) use Kyoto *stringency*[28] instead of the Kyoto dummy as the dependent variable. The IV strategy con-

[27] Using the log of remoteness instead of openness yields comparable results.
[28] Kyoto stringency is defined in subsection 2.3. and used in column (12) of Table 2.

tinues to work fine with weak identification tests yielding F-statistics on excluded instruments well above 20, and the overidentification restrictions are satisfied. Since stringency has support over the interval [0,2], it is not surprising that the point estimates are smaller than the ones for the commitment dummy. The stringency measure is statistically significant and negative. The point estimate in column (8) suggests that countries with a slack cap (as of 1990) have 5% lower emissions, while countries with a non-slack cap have 10% lower emissions.

Columns (9) to (12) repeat regression (3) for different subsamples. Column (9) excludes the 10 OPEC countries included in column (3). The IV strategy continues to work, and the point estimate on the Kyoto dummy is almost exactly identical to the one found in column (3). Column (10) uses only countries with GDP per capita levels higher than the median (as of 2007). Major emerging economies such as China, Brazil, Russia and South Africa remain in the sample, but India or Indonesia drop out. With this much smaller sample, the F-statistic on excluded instruments is 8.38 which signals a potential weak instrument problem. The results suggest that Kyoto reduces emissions in this sample by about 9%. Using LIML estimation to avoid the weak instrument problem yields very similar point estimates of the Kyoto dummy. Column (11) eliminates transition countries from the sample. Interestingly, this reduction of the sample undoes the statistical significance of the Kyoto dummy, but the magnitude of the effect remains comparable to earlier results. However, dropping insignificant covariates restores the statistical significance of the Kyoto effect, albeit at a point estimate that appears implausibly large.

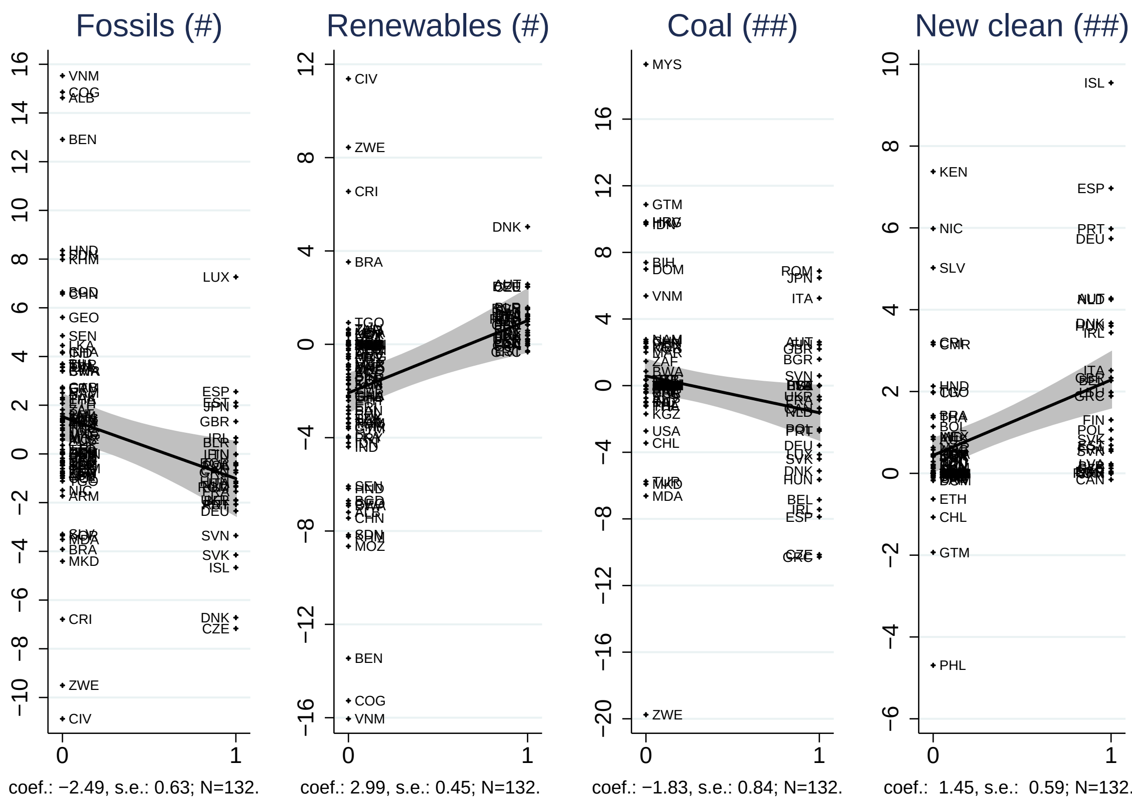
Summarizing OLS estimates of the effect of Kyoto on CO_2 emissions do not appear to be biased away from zero. Using ICC membership dummies as instruments, our IV regressions suggest that, compared to the counterfactual, Kyoto commitment reduces carbon emissions by about 10%. While the IV strategy generally works very well, in most regressions, the Kyoto effect is statistically significant only at the 5 or 10% level. Doubts about a measurable Kyoto effect are largest when transition countries are excluded from the sample. In the following subsections, we present evidence that shed light on the channels through which Kyoto may have led to lower emissions.

3.3. Energy mix

The evidence in a diagram We start with the effect of Kyoto commitments on countries' energy mix. The left-most diagram in Figure 4 plots changes in the share of fossil fuels in total energy consumption. That share has increased substantially in some countries such as Vietnam or China (by 15.5 and 6.6 percentage points, respectively), but has fallen in crisis-stricken countries such as Zimbabwe. In the group of committed countries, the share of fossil fuel has fallen in countries such as Denmark, Iceland, or the Czech Republic, but for different reasons. While the former two countries expanded the share of wind and hydro power, the latter increased the share of nuclear energy. The importance of fossil fuel has increased by a surprising amount in Luxembourg, but also gained ground in Norway (from a very low level). On average, committed countries reduced the share of fossil fuels by almost exactly one percentage point, while it increased by about 1.5 percentage points in the sample of non-committed countries. The difference of 2.49 percentage points is statistically significant at the 1% level. The next diagram shows changes in the share of renewables in total energy consumption. Within the group of committed countries, that share increased most in Denmark (by about 5 percentage points), but fell slightly in Norway. It increased by 1.05 percentage points on average in committed countries. Cross-country variation in the sample of non-committed countries is large, but clusters around -2 percentage points. The difference between the group averages is 2.99, statistically significant at the 1% level. It appears, thus, that changes in the share of renewables in total energy use correlates positively with Kyoto commitment.

The third diagram refers to the share of coal in electricity production, while the right-most diagram studies the share of new clean forms of energy (wind and solar). On average, the share of coal has decreased slightly in the group of committed countries (-1.28 percentage points), while it has minimally increased in the sample of non-committed countries (+0.55 percentage points). The difference is 1.83 percentage points and statistically significant at the 5% level (p-value 0.03). Interestingly, there are a couple of non-committed countries, where wind and solar energy expanded substantially (Kenya, Nicaragua, San Salvador), but in the sample of non-committed countries as a whole, the share increased by 0.33 percentage points only. In the sample of committed countries, Iceland, and Germany as well as Spain and Portugal have considerably increased their shares of new clean energy sources. The group average is 1.77. The growth difference between the two groups is 1.45 percentage points, significant at the 5% level (p-value 0.02).

Figure 4: Change in energy mix



Note: The diagrams show the change between pre- (1997-2000) and post-treatment (2004-07) averages for non-Kyoto (0) and Kyoto countries (1). New clean refers to wind and solar energy. #: share in total energy consumption. ##: share in electricity production.

Regression results Table 4 follows Table 3 and provides regression results on yearly data of the effect of Kyoto commitment. To save space, Table 4 only reports the Kyoto estimates and first- and second-stage diagnostics. Full results are delegated to the web appendix but discussed in the text. Columns (A1) to (A3) investigate the effect of Kyoto on the share of renewables in total energy use. The uninstrumented effect implies that Kyoto commitment increases that share by 1.45 percentage points, while the spatial lag of Kyoto is negative. Instrumentation doubles the effect of Kyoto on the share of renewables, and excluding transition countries from the sample increases it further. Columns (A4) to (A6) investigate the effect of Kyoto on the share of fossil fuel in total energy consumption. The first column presents a non-instrumented regression. The EU membership dummy appears to negatively correlate with the share of fossil fuels, while log population and the spatial lag of Kyoto commitment is positively associated, the latter signaling a possible free-rider effect. Own Kyoto commitment bears a negative sign, but the coefficient fails to be statistically significant (p-value 0.16). 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 and to the fact that the Kyoto dummy is an imprecise proxy for climate policies. Using IV estimation remedies the latter problem. Column (A5) shows that ICC membership and the lag thereof continue to be good instruments: both, the overidentification and the weak instrument tests suggest instrument validity. The resulting point estimate is negative and statistically significant at the 1% level. It implies that Kyoto commitment results in a 2.67 percentage points reduction in the share of fossil fuel in total energy consumption. Excluding transition countries (column (A6)) does not change this finding. Similarly, using the Kyoto stringency variable has no substantial effect on results either (not shown).

Columns (B1) to (B3) analyze the share of coal in the *production of electricity*. While the analysis in Figure 4 suggests a statistically significant negative relationship, controlling for a host of variables such as the log of population or an EU dummy, and applying our IV strategy, there is no statistical evidence in favor of a negative effect of Kyoto on the share of coal. This is despite the fact that our instruments continue to do fine. Finally, columns (B4) to (B6) study the share of new clean energy sources (such as wind and solar power) in the electricity production. Both, the uninstrumented and the IV regressions suggest that Kyoto commitments have increased that share by almost 2 percentage points. In line with other results in this paper, we find that measurement error in the Kyoto variable dwarfs the possible bias from reverse causation. Interestingly, the IV estimates do not show that more democratic countries (higher Polity index) have seen larger increases in the share of new clean energies, nor is the EU dummy significant. There is also no evidence for free-riding, as other countries' commitment has no effect on the share of new clean energies.

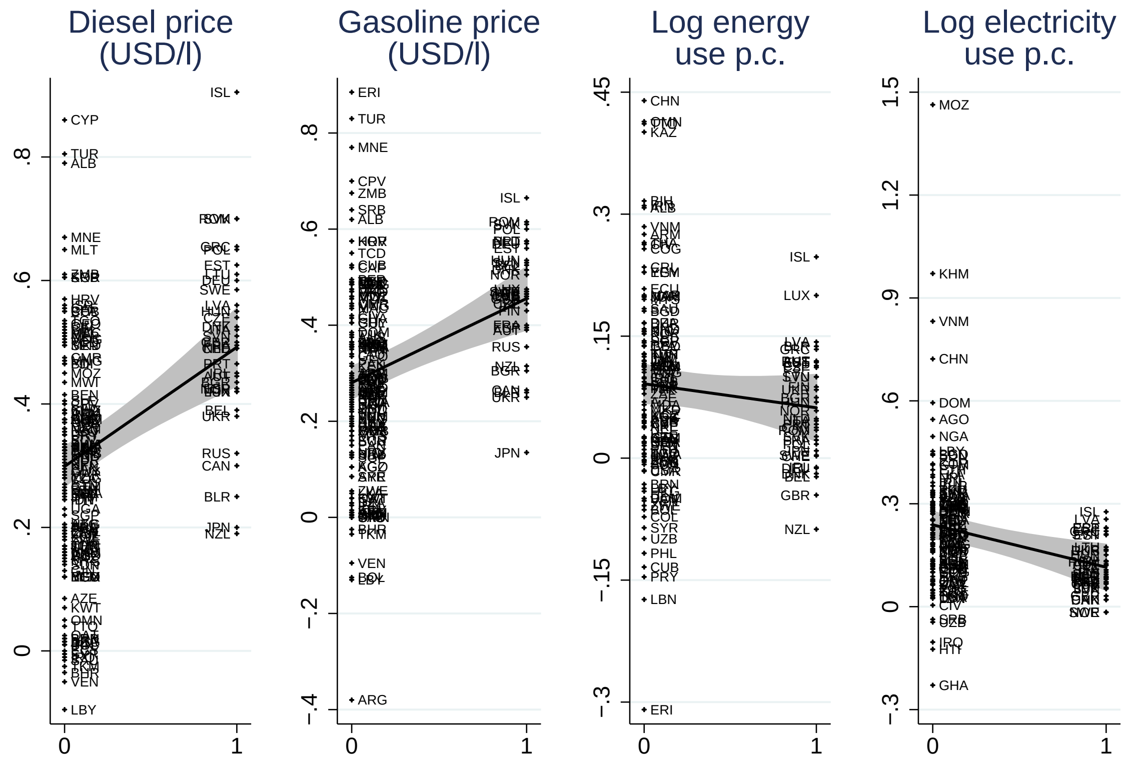
3.4. Fuel prices and energy and electricity use

The evidence in a diagram We continue with the effect of Kyoto on fuel prices. The two left-most panels of Figure 5 plot the (absolute) changes in fuel prices, expressed in U.S. dollars per liter, across the groups of committed and non-committed countries.[29] Overall inflation in the price of oil leads to rising prices in both groups. Since the price of oil, the key input in production of diesel fuel or gasoline, is quoted in U.S. dollars on world markets, and fuels are very strongly traded internationally, it is reasonable to assume the law of one price to hold. Deviations due to country-specific factors such as refinery capacity, distribution systems, or geographical situation are taken care of by time-differencing. We interpret the data in the figures as deviations from the world price caused by fuel subsidies or taxes. The figures do not, however, inform about the price of those fuels *relative* to other goods. The price of diesel fuel has increased by about 30 cents in the group of non-committed countries and by 49 cents in the group of committed countries. The differential increase across the two groups is 19 cents. It is significant at the 1% level. A similar picture emerges when looking at gasoline. The average increase in non-committed countries was 28 cents and in committed countries 46. The difference, 18 cents, is again statistically significant at the 1% level.

In the next step we investigate the role of Kyoto commitment for the per capita use of energy and electricity. The unconditional diff-in-diff exercise is visualized in the two panels on the right in Figure 5. The log of energy use per capita (in kg of oil equivalent) has increased in most countries; most notably in China where it has increased by almost 45 percent. It has fallen in some industrialized countries such as New Zealand, Great Britain or Germany. In the group of non-committed countries the average rate of change is 9%, while it is 6% in the sample of committed countries. The difference, about 3 percentage points, is only marginally statistically significant (p-value 0.10). Turning to the log electricity per capita (in kWh) consumption 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 by 83 and 72% in Vietnam and China, respectively. The average growth rate of the two observed periods is 24% in the sample of non-committed countries and 12% in the group of committed countries. The difference, 12%, is statistically significant at the 1% level.[30]

[29]In our more comprehensive regression analysis we include the exchange rate as a control.
[30]Robust estimation lowers the point estimate to 9%, still significant at the 1% level.

Figure 5: Change in fuel prices and energy/electricity use



coef.: 0.19, s.e.: 0.03; N=160. coef.: 0.18, s.e.: 0.03; N=160. coef.: −0.03, s.e.: 0.02; N=132. coef.: −0.12, s.e.: 0.03; N=132.

Note: The diagrams show the change between pre- (1997-2000) and post-treatment (2004-07) averages for non-Kyoto (0) and Kyoto countries (1).

Regression results Panels (C) and (D) in Table 4 present regression results based on yearly data. Column (C1) reports the uninstrumented fixed-effects estimator. It suggests that Kyoto status is positively associated to the price of diesel fuel. Also the spatial lag of Kyoto commitment correlates positively. Contrary to what one may conclude from the theoretical literature, the price of fuel increases with GDP, but that relationship is non-monotonic.[31] Left-leaning chief executives of governments are associated to lower fuel prices, but this effect stops to be statistically significant when Latin American countries are dropped from the sample (not shown). WTO and EU dummies lead to higher diesel prices (by 5 and 16 cents, respectively). Higher degrees of democracy do not influence fuel prices significantly. With an adjusted (within) R^2 of 70%, the specification is surprisingly successful in predicting the diesel price. Instrument-

[31] Using GDP per capita in the regression instead of GDP leads to exactly the same result.

ing Kyoto commitment leaves the controls virtually unchanged, but the Kyoto effect more than doubles to 18 cents per liter. The IV strategy turns out to work reasonably well: the weak identification test yields an F-statistic of 18.12, and the overidentification test does not reject instrument validity. Excluding transition countries (column (C3)) does not change the picture. The same is true when Kyoto stringency is used instead of the Kyoto dummy (not shown). The results for the price of gasoline (columns (C4) to (C6)) look very similar. Again, the instrumented estimation yields a point estimate of Kyoto that is about double the uninstrumented one.

We now turn to the effect of Kyoto on the log energy use per capita. The uninstrumented regression yields a negative, statistically significant effect of -5%. Population growth exerts a strong negative effect while trade openness has a weak positive one. Higher GDP leads to higher energy use per capita, but only through the squared term. The spatial lag of the Kyoto variable is negative but not significant. Turning to IV estimations, the negative effect of Kyoto remains but is no longer statistically significant at standard levels (e.g., the p-value in column (D2) is 0.12). Interestingly, the evidence is different for electricity consumption per capita. Kyoto commitment decreases consumption by 5% without instrumentation and by about 9% with instrumentation. Comparison with the lack of effects on energy consumption suggests that Kyoto commitment may have caused a substitution away from electricity to other forms of energy (e.g., in heating systems). Also interesting, and reminiscent of the results for emissions, excluding the transition countries from the sample lowers statistical significance (p-value 0.05).

Summarizing we find evidence that Kyoto led to some restructuring toward cleaner energy sources such as wind and hydro power. Also, fuel prices went up and there is some evidence of less electricity use per capita. This might explain part of the emission reductions in Kyoto countries. In the last step, we now investigate the effect of Kyoto commitment on macroeconomic variables such as per capita income, the share of manufacturing in total output, and the trade balance.

3.5. Macroeconomic outcomes and international trade

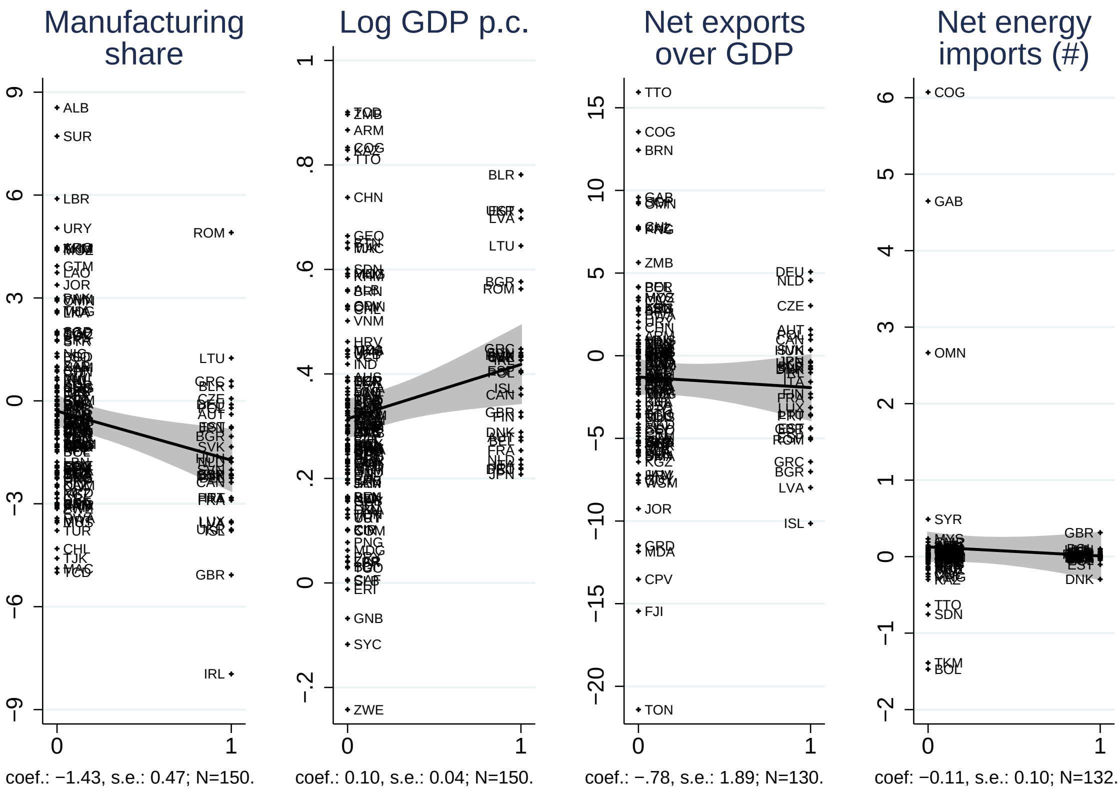
The evidence in a diagram The left-most panel in Figure 6 plots the change in the share of manufacturing. On average, in the sample of non-committed countries that share fell very slightly by -0.32 percentage points between the two periods. In most countries, it remained fairly constant (China, Indonesia, India), whereas it increased substantially in some (Argentina, Brazil). Idiosyncracies play an important role, see the outlier of Albania or Suriname. In the sample of committed countries, the manufacturing share fell on average by -1.73 percentage points. It fell most in Ireland, where the share shrank from 33 to 25 percent, but increased in Romania from 20 to 25. The difference between sample averages is equal to -1.43, statistically significant at the 1% level.[32] The next diagram plots changes in the log of GDP per capita. Non-Kyoto countries had, on average, a growth rate of 32% between the two periods. Some countries experienced a dramatic decline in GDP per capita (e.g. the crisis-stricken Zimbabwe -24%) whereas other countries had a boom (e.g. China with a growth rate of 74%). In the group of countries with Kyoto commitment, the average growth rate between the two periods was 42%. Most striking is that the countries with above average growth rates are mostly economies in transition (such as Belarus, Estonia or Ukraine). The difference between the group averages, 10 percentage points, is statistically significant at the 1% level.[33]

Finally, the last two diagrams in Figure 6 turn to net exports over GDP and net energy imports relative to energy use. On average, net exports fell by 1.2 and 2.0 percentage points, respectively, in the group of non-committed and committed countries. The difference is -0.78 and not statistically significant. In the sample of non-committed countries, the change in the energy import share is, on average, almost identical to zero. The difference across the two groups of countries is statistically not different from zero, either. Changes in net energy imports are potentially explained by economic growth, substitution away from traditional domestically available fossil fuel sources (coal) towards imported ones (gas), and increases in domestic oil production. Given the importance of idiosyncratic factors, it is not surprising that the figure does not reveal a discernable pattern across Kyoto and non-Kyoto countries.[34]

Regression results Table 4, Panel (E) and (F) report results of fixed-effects regressions based on yearly data. We apply the standard model (3), but use 5th lags of potentially endogenous variables such as the log of GDP, the log of the nominal exchange rate and openness.[35] Columns (E1) to (E3) analyze the effect of Kyoto on the share of manufacturing in GDP. The results con-

[32] Using robust regression to penalize outliers, the difference is -1.13, still significant at the 1% level.
[33] Using robust regression techniques does not change the result.
[34] The finding does not change when only energy importers are considered. Robust regression techniques show that the finding of no relationship is not due to outliers.
[35] Generally, results do not depend on taking these lags. They are also insensitive to using the real exchange rate instead of the nominal one or of using some measure of geographical remoteness instead of openness.

Figure 6: Change in macroeconomic outcomes and trade



Note: The diagrams show the change between pre- (1997-2000) and post-treatment (2004-07) averages for non-Kyoto (0) and Kyoto countries (1). Sample excludes OPEC and other major oil producing countries. # Net energy imports as share of energy consumption.

firm the ones obtained in Figure 6: Kyoto commitment reduces the manufacturing share by about 0.82 percentage points in the non-instrumented case and by about 1.22 percentage points in the IV regression. Excluding transition countries makes the effect stronger (-1.9 percentage points) and enables more accurate estimation (p-value 0.03). Columns (E4) to (E6) look at real GDP per capita. Log population is negatively related to real GDP while WTO membership appears to enter positively. The Polity index is insignificant. More importantly, Kyoto commitment does not appear to have any measurable impact on GDP per capita, neither in the OLS nor in the IV regressions.[36] For all regressions the IV strategy works well.

Columns (F1) to (F3) regress net exports over GDP (the trade balance) on the Kyoto dummy

[36] These regressions are not to be interpreted as growth regressions. With the fairly short time period under analysis (1997-2007), time variation in most independent variables is fairly low. This is particularly so for variables that turn out significant in standard growth regressions such as Polity IV or measures of human capital. The latter is captured by country effects.

and controls. Log GDP (the 5th time lag thereof) correlates positively with net exports, the nominal exchange rate does not appear to matter. The Kyoto effect is negative, but statistically insignificant. Finally, turning to the share of net energy imports in countries' total energy use, we add the log nominal exchange rate with the U.S. dollar as an additional independent variable to the regression model. The uninstrumented regression in column (F4) shows that a 10% increase in GDP appears to reduce energy imports as a share of total energy use by about 10 percentage points. An increase in the size of the population by 10% is associated to a 17 percentage points increase in the share of energy imports. This implies that higher per capita income is negatively associated to the share of energy imported, but that economic size (population) increases that share. WTO membership appears to increase the share of imports by about 15 percentage points while a depreciation of the national currency relative to the dollar (higher nominal exchange rate) makes imports more expensive and lowers their share in total energy use. While these are reasonable results, Kyoto membership, instrumented or not, has no measurable effect on energy imports. The argument that climate policies may reduce countries' dependence on imported fossil fuels is not supported by the data.[37]

Summarizing, we find some evidence that Kyoto led to deindustrialization of committed countries, possibly by relocating industrial production to non-committed countries. That effect lies between 1 and 2 percentage points and is therefore not economically negligible. We do not find, however, that this is associated with lower GDP per capita or lower net exports. The point estimates are negative, but not statistically significant.

3.6. Robustness: long diff-in-diff estimation.

Bertrand et al. (2004) argue that standard errors of treatment effects in diff-in-diff estimation are inconsistent and the estimator's standard deviation is underestimated if the outcome and treatment variable are both serially correlated over time. This might lead to an overrejection of the null of no effect. In our case, this condition is most likely met. Once the Kyoto dummy switches to one it stays on and the treatment variable changes little over time. The same is true for most outcome variables. The authors suggest a long diff-in-diff estimator to cure the problem, i.e. apply a fixed-effects estimator to the pre- and post-treatment averages. There has been some

[37]This finding is robust to restricting the sample to net energy importers (the Kyoto coefficient turns negative but remains insignificant). It is also robust to applying 5th time lags to the potentially endogenous variables log GDP, log nominal exchange rate, and openness.

heterogeneity in the timing of Kyoto's ratification across countries, but most countries have ratified between 2001 and 2003. Therefore, we define the pre-treatment period to be 1997 to 2000 and the post-treatment period to be 2004 to 2007.[38] Subsequently, we apply the long diff-in-diff estimator as a further robustness check. This is analogous to the graphical analysis in chapters 3.2. to 3.5., but takes into account the possible endogeneity of Kyoto commitment.

Table 5 shows the results. Each column corresponds to the IV regression of one outcome variable on Kyoto status in the base sample and sample without transition countries, respectively. Instruments for a country's Kyoto status are again the ICC dummy and its spatial lag. Controls are included as in Tables 3-4. The EU dummy is not included since between the two periods it only changes for new accession countries and is therefore effectively a transition country dummy. Again, our instrumentation strategy works well. The over-identification test does not reject[39] and the test on excluded instruments rejects weak instruments. The IV estimates of Kyoto's effect from the long diff-in-diff model square well with the once found in the fixed-effects model on yearly data both in terms of the estimates' sign and significance. The only exceptions are log emissions per GDP where we no longer find a statistically significant coefficient, the manufacturing share where we only find a significant effect in the subsample of non-transition countries and energy use per capita and the net energy import share where the coefficients turn out to be significant now. Overall, the point estimates are somewhat larger in absolute terms for some of the long diff-in-diff estimates.

Summarizing the fixed-effects and long diff-in-diff results, there is some evidence that Kyoto countries reduced overall CO_2 emissions by roughly 10% relative to non-Kyoto countries. Kyoto commitment had a pronounced effect on diesel and gasoline prices, and led to restructuring of the energy and electricity production. Interestingly, there has been a substantial increase in the share of renewables and new energy sources (such as wind, solar or hydro power). In terms of macroeconomic outcomes, we find some evidence for a decline in the share of manufacturing in Kyoto countries of roughly 2 percentage points. However, our results suggest that Kyoto has not affected real GDP per capita.

[38] 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.

[39] Only for the diesel price, the over-identification test is weakly rejected at a p-value of 0.10 and 0.09, see columns (13) and (14).

4. Conclusion

The success or failure of the Kyoto Protocol in curbing GHG emissions is mostly judged against the text of the agreement. It is very likely that many countries will not reach their promised emission targets in the 2008-12 period. And additionally, for most countries that will meet their target, this emission ceiling was slack in the first place. Therefore, Kyoto is deemed pointless. Yet, we argue that this accounting exercise cannot inform about the causal effect of Kyoto commitment. Instead, to find the treatment effect of Kyoto one should assess outcomes such as emissions against their unobserved counterfactual. But Kyoto membership is likely not random. This complicates finding the counterfactual due to reverse causality. Therefore, we use an IV strategy. We propose to use a country's preferences for multilateralism, i.e. its commitment to aims of the international community, as instrument for Kyoto. Ratification of the Rome Statute that constitutes the International Criminal Court and its spatial lag serves us as a proxy. We argue that these instruments are valid because they are strongly correlated to Kyoto status and will very likely fulfill the exclusion restriction.

We then use our instruments in a panel of 150 countries over the years 1997-2007 to estimate Kyoto's average treatment effect. We find evidence in fixed-effects and long diff-in-diff models that Kyoto reduced the average Kyoto country's CO_2 emissions by about 10%. This result is surprising, given the generalized belief that the Kyoto Protocol is ineffective and given the negative findings of Aakvik and Tjøtta (2011) on the Helsinki and the Oslo Protocols. Yet, we believe that our finding is not implausible. Unlike other environmental pollutants, CO_2 emission is costly due to the price of fuels, energy and electricity. Kyoto commitment adds incentives to save on fuel and energy use. The observed difference in emission growth rates between Kyoto and non-Kyoto countries could be the result of climate policy efforts in Kyoto countries and informal enforcement mechanisms (naming and shaming). And we indeed find, that Kyoto commitment increases fuel prices and leads to favorable changes in the energy mix. But part of the observed difference could also be a result of carbon leakage due to relocation of industries. The sign of the spatial Kyoto lag hints at this possibility. Taken together, those effects can explain the seizable treatment effect on emissions. Moreover, we find that Kyoto led to a certain degree of deindustrialization but had no causal effect on GDP per capita growth.

A key novelty of our paper is the application of the ICC ratification dummy as instrument for Kyoto commitment. It might prove useful in other contexts as well. For example, it could be used to estimate Kyoto's effect on bilateral trade flows for energy-intensive sectors and thus help underpin the debate about carbon-related border tax adjustments.

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Table 1: Summary statistics

Variable	Obs	Mean	Std. Dev.	Source
Kyoto dummy (0,1)	1,547	0.12	0.32	www.unfccc.int
Spatial lag, Kyoto dummy	1,547	0.12	0.37	own construction
Kyoto stringency (0,1,2)	1,547	0.19	0.55	own construction
ICC membership dummy	1,547	0.33	0.47	www.icc-cpi.int
Spatial lag, ICC dummy	1,547	0.27	0.94	own construction
log CO_2 emissions (metric tons)	1,547	9.81	2.26	World Bank, WDI 2010
log CO_2 emissions per unit of GDP (kg per 2000 US$ GDP)	1,547	-8.14	0.83	own construction
log CO_2 emissions per capita (metric tons)	1,547	0.52	1.67	own construction
Pump price of diesel fuel (USD/l)	659	0.56	0.34	World Bank, WDI 2010
Pump price of gasoline (USD/l)	659	0.71	0.35	World Bank, WDI 2010
Share of renewables in total energy production	1,282	21.66	27.39	World Bank, WDI 2010
Share of coal in total electricity production	1,282	17.79	27.06	World Bank, WDI 2010
Share of fossil fuels in total electricity production	1,282	69.22	27.02	World Bank, WDI 2010
Share of alternative sources in total electricity production	1,282	2.37	5.00	own construction
log net energy imports (% of energy use)	872	-0.98	0.86	World Bank, WDI 2010
log electricity use per capita (in kWh)	1,271	7.30	1.52	World Bank, WDI 2010
log energy use per capita (in kg oil equivalent)	1,282	7.20	1.01	World Bank, WDI 2010
log real GDP per capita	1,547	8.65	1.15	PWT 6.3 (rgdpch)
Share of manufacturing in GDP	1,380	15.54	7.57	World Bank, WDI 2010
Net exports per unit of GDP	1,368	-1.15	7.34	World Bank, WDI 2010
log GDP	1,547	17.94	1.88	PWT 6.3 (cgdp)
log GDP squared	1,547	325.54	68.65	own construction
Openness, current price	1,547	0.85	0.47	PWT 6.3 (openc)
log remoteness	1,547	-0.61	0.95	own construction
log nominal exchange rate to the US dollar	1,547	3.19	2.81	World Bank, WDI 2010
Polity IV index (-10: perfectly aucratic regime, 10: full democracy)	1,547	3.60	6.37	www.systemicpeace.org
log population	1,547	9.29	1.50	World Bank, WDI 2010
log stock of multilateral environmental agreements	1,547	3.27	0.58	iea.uoregon.edu
Chief executive party orientation (right: 1, center: 2, left: 3)	1,547	1.25	1.27	Word Bank, DPI 2010 (execrlc)
WTO dummy	1,547	0.81	0.39	www.wto.org
EU dummy	1,547	0.12	0.32	europa.eu

Note: The table shows summary statistics and data source for variables over the period 1997-2007.

Table 2: Explaining Kyoto status: panel fixed-effects estimates, 1997-2007

Dependent variable: Kyoto commitment dummy or Kyoto stringency	(1)	(2)	(3)	(4)	(5)	(6)	**(7)**	(8)	(9)	(10)	(11)	(12)
Sample	base	base	base	base	base	base	**base**	plus OPEC	base	w/o poor	w/o small	base
Model / dep. var.	base	base	base	base	base	base	**base**	base	time lags	base	base	stringency
ICC dummy				0.13***	0.12***	0.12**	**0.09****	0.09***	0.08**	0.14**	0.11**	0.14**
				(0.03)	(0.03)	(0.04)	**(0.03)**	(0.03)	(0.03)	(0.05)	(0.04)	(0.05)
ICC dummy, spatial lag				0.01**	0.01**	0.01***	**0.005*****	0.005***	0.001	-0.12*	0.01***	0.01**
				(0.005)	(0.004)	(0.003)	**(0.001)**	(0.001)	(0.002)	(0.05)	(0.002)	(0.002)
log GDP per capita[a]	-3.54***	-1.59*	-2.61***	-1.85***	-1.62**	-2.24**	**-1.43****	-1.29**	-1.40*	-2.01	-1.38*	-1.51*
	(0.76)	(0.63)	(0.62)	(0.55)	(0.51)	(0.81)	**(0.45)**	(0.4)	(0.59)	(1.55)	(0.64)	(0.61)
log GDP per capita, squared[a]	0.23***	0.10*	0.16***	0.11**	0.10**	0.13*	**0.09****	0.08**	0.09*	0.11	0.08	0.09*
	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)	(0.05)	**(0.03)**	(0.03)	(0.04)	(0.09)	(0.04)	(0.04)
log CO2 emissions per capita[a]	-0.23***	-0.12*	-0.11*	-0.11*	-0.10*	-0.07	**-0.09****	-0.07*	-0.05*	0.01	-0.07	-0.12*
	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.08)	**(0.03)**	(0.03)	(0.02)	(0.07)	(0.04)	(0.05)
log geographical remoteness		-4.11***	-4.77***	-4.57***	-4.60***	-5.19***	**-2.65*****	-2.48***	-2.76***	-5.95***	-3.86***	-3.50***
		(0.74	(0.61)	(0.55)	(0.56)	(0.87)	**(0.58)**	(0.56)	(0.56)	(1.02)	(0.82)	(1.03)
log population		-0.5										
		(0.43)										
Chief executive party orientation			-0.01									
			(0.02)									
log stock of other multilateral environmental agreements (MEA)			-0.06									
			(0.1)									
WTO dummy					-0.16**	-0.17**	**-0.16****	-0.14*	-0.17**	-0.13	-0.09**	-0.2
					(0.05)	(0.06)	**(0.06)**	(0.06)	(0.06)	(0.08)	(0.03)	(0.11)
EU dummy					0.19***	0.15**	**-0.01**	-0.002	0.002	-0.08	-0.09	0.21
					(0.05)	(0.05)	**(0.09)**	(0.1)	(0.09)	(0.08)	(0.12)	(0.19)
Share of renewables in total energy production						0						
						(0.01)						
Share of manufacturing sector in GDP						-0.01						
						(0.01)						
Kyoto dummy, spatial lag							**0.38*****	0.38***	0.37***	0.34***	0.31***	0.76***
							(0.07)	(0.07)	(0.07)	(0.09)	(0.08)	(0.14)
Number of observations	1,886	1,886	1,664	1,886	1,886	1,162	**1,886**	2,018	1,867	967	1,108	1,886
Number of countries	172	172	153	172	172	113	**172**	184	172	88	101	172
within R^2	0.29	0.45	0.49	0.47	0.49	0.54	**0.58**	0.58	0.58	0.67	0.64	0.6
Robust F-stat	4.77	12.87	17.06	15.06	208.03	371.94	**10.26**	9.89	10.98	22	10.77	7.56
RMSE	0.19	0.17	0.17	0.16	0.16	0.18	**0.14**	0.14	0.14	0.18	0.15	0.24

Note: Heteroskedasticity-robust standard errors (clustered at country-level) in parentheses, * $p<0.1$, ** $p<0.05$, *** $p<0.01$. Time controls and constant included (not shown). Kyoto commitment: Dummy (0-1). Spatial lags are constructed as weighted averages over foreign variables, with population over squared distance used as weights. [a] variables included with their 5th time lag in column (9).

Table 3: The effect of Kyoto on CO_2 emission levels. Panel fixed-effects estimation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Method:	FE		IV-FE									
Dep.var.:	Log EM	Log EM	Log EM	Log EM	log (EM/Y)	Log EM	Log EM	log (EM/Y)	Log EM	Log EM	Log EM	Log EM
Sample:	base	base	base	base	base	base	base	base	w/o OPEC	w/o poor	w/o transition	
Kyoto variable:	Kyoto dummy		Kyoto dummy			Stringency			Kyoto dummy			
Kyoto status (dummy or (0,1,2))	-0.09**	-0.06**	-0.11**	-0.11**	-0.10*	-0.06**	-0.06**	-0.05*	-0.11*	-0.09*	-0.11	-0.19***
	(0.04)	(0.03)	(0.05)	(0.06)	(0.06)	(0.03)	(0.03)	(0.03)	(0.06)	(0.05)	(0.07)	(0.05)
Kyoto commitment, spatial lag	0.01	0.02***	0.03***	0.03***	0.03***	0.03***	0.03***	0.03***	0.03***	0.07**	0.03***	0.04***
	(0.01)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)	(0.04)	(0.003)	(0.004)
log GDP (log Y)	1.91***	2.19*	2.15*	0.83***		2.21*	0.83***		2.12	3.14***	2.18*	0.85***
	(0.54)	(1.28)	(1.27)	(0.26)		(1.28)	(0.26)		(1.30)	(1.20)	(1.30)	(0.27)
log GDP, squared	-0.03**	-0.04	-0.04			-0.04			-0.04	-0.06*	-0.04	
	(0.01)	(0.03)	(0.03)			(0.03)			(0.03)	(0.03)	(0.03)	
log population	0.96***	0.75***	0.59*	0.84***	0.83**	0.65**	0.89***	0.88***	0.63*	0.68**	0.52	
	(0.27)	(0.25)	(0.3)	(0.32)	(0.34)	(0.28)	(0.31)	(0.33)	(0.36)	(0.27)	(0.32)	
log stock of multilateral	-0.04	-0.23**	-0.22**	-0.22**	-0.23***	-0.22**	-0.20**	-0.22***	-0.21**	-0.46***	-0.22**	-0.20*
environmental agreements	(0.15)	(0.09)	(0.09)	(0.09)	(0.08)	(0.09)	(0.09)	(0.08)	(0.10)	(0.14)	(0.10)	(0.11)
Chief executive party	21.47**	17.33***	16.92***	13.23***	12.04**	16.78***	13.32***	12.10**	13.94***	3.97	18.92***	13.66**
orientation	(9.22)	(4.81)	(4.80)	(5.08)	(4.75)	(4.79)	(5.04)	(4.70)	(5.02)	(6.48)	(5.60)	(6.13)
Openness	-0.11	-0.12	-0.13			-0.12			-0.10	-0.28***	-0.14	
	(0.09)	(0.09)	(0.09)			(0.09)			(0.10)	(0.08)	(0.10)	
WTO dummy	-0.01	-0.01	-0.02			-0.02			-0.02	-0.05	0	
	(0.06)	(0.05)	(0.05)			(0.05)			(0.05)	(0.07)	(0.06)	
EU dummy	-0.07**	-0.05*	-0.03			-0.02			-0.04	0.01		
	(0.03)	(0.03)	(0.03)			(0.03)			(0.03)	(0.04)		
Polity IV (-10,10)	-0.004	-0.01	-0.01			-0.01			-0.01	-0.02**	-0.01	
	(0.005)	(0.004)	(0.004)			(0.004)			(0.005)	(0.007)	(0.004)	
Number of obs.	2,335	1,547	1,547	1,547	1,547	1,547	1,547	1,547	1,444	782	1,415	1,415
number of countries	148	148	148	148	148	148	148	148	138	72	136	136
First stage diagnostics												
Weak identification test			19.9	17.15	17.33	22.4	21.92	22.23	19.67	8.38	22.77	23.82
Hansen-Sargan J stat (p-val)			0.19	0.31	0.36	0.19	0.3	0.35	0.18	0.37	0.2	0.35
Second stage diagnostics												
Adjusted R2	0.4	0.47	0.47	0.45	0.09	0.47	0.45	0.09	0.47	0.6	0.48	0.43
F-stat	21.85	18.18	26.22	28.62	27.63	26.82	29.81	26.49	23.31	12.62	32.8	46.21
RMSE	0.26	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.14	0.12	0.14	0.15

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 spatial lag thereof. All regressions use a comprehensive set of year dummies (not reported).

Table 4: The effect of Kyoto on outcome variables. Panel fixed-effects estimation

Method:	FE	IV-FE	IV-FE	FE	IV-FE	IV-FE	FE	IV-FE	IV-FE	FE	IV-FE	IV-FE
Sample:	base	base	no trans.	base	base	no trans.	base	base	no trans.	base	base	no trans.
	(A) Shares in energy production						**(B) Shares in electricity production**					
Dep.var.:	**Renewables**			**Fossil fuel**			**Coal**			**Alternative energy**		
	(A1)	**(A2)**	**(A3)**	**(A4)**	**(A5)**	**(A6)**	**(B1)**	**(B2)**	**(B3)**	**(B4)**	**(B5)**	**(B6)**
Kyoto	1.45***	2.90***	3.33***	-0.75	-2.67***	-2.58**	0.10	-0.72	-0.59	1.14***	1.94***	2.01***
	(0.46)	(0.81)	(0.99)	(0.54)	(1.03)	(1.11)	(0.92)	(1.82)	(2.43)	(0.26)	(0.58)	(0.52)
Number of obs. (countries)		1,282 (120)			639 (136)			639 (136)			1,282 (120)	
1st stage Weak ID test (F-stat)		18.94	21.77		18.12	19.29		18.12	19.29		18.94	21.77
1st stage Over-ID test (p-val)		0.91	0.95		0.15	0.13		0.80	0.88		0.91	0.95
2nd stage adj. R2	0.25	0.23	0.23	0.70	0.69	0.64	0.59	0.57	0.52	0.25	0.23	0.23
	(C) Pump prices, USD/l						**(D) Log per capita use of**					
Dep.var.:	**Diesel fuel**			**Gasoline**			**Energy**			**Electricity**		
	(C1)	**(C2)**	**(C3)**	**(C4)**	**(C5)**	**(C6)**	**(D1)**	**(D2)**	**(D3)**	**(D4)**	**(D5)**	**(D6)**
Kyoto	0.07**	0.18***	0.17**	0.10***	0.21***	0.22***	-0.05***	-0.05	-0.05	-0.05*	-0.09**	-0.10*
	(0.03)	(0.05)	(0.07)	(0.03)	(0.05)	(0.07)	(0.02)	(0.03)	(0.04)	(0.02)	(0.04)	(0.05)
Number of obs. (countries)		639 (136)			639 (136)			1,282 (120)			1,282 (120)	
1st stage Weak ID test (F-stat)		18.12	19.29		18.12	19.29		18.94	21.77		18.94	21.77
1st stage Over-ID test (p-val)		0.15	0.13		0.80	0.88		0.41	0.35		0.74	0.83
2nd stage adj. R2	0.70	0.69	0.64	0.59	0.58	0.52	0.08	0.06	0.05	0.10	0.07	0.08
	(E) Macro variables						**(F) International trade**					
Dep.var.:	**log GDP per capita**			**Manufacturing share in GDP**			**Net exports over GDP**			**net energy imports over use**		
	(E1)	**(E2)**	**(E3)**	**(E4)**	**(E5)**	**(E6)**	**(F1)**	**(F2)**	**(F3)**	**(F4)**	**(F5)**	**(F6)**
Kyoto	-0.01	-0.05	-0.0515	-0.82*	-1.22*	-1.92**	-0.27	-1.14	-1.67	0.01	0.05	0.09
	(0.04)	(0.07)	(0.08)	(0.50)	(0.71)	(0.90)	(0.94)	(1.90)	(2.43)	(0.06)	(0.11)	(0.13)
Number of obs. (countries)		1,388 (137)			1,263 (132)			1,282 (120)			1,242 (119)	
1st stage Weak ID test (F-stat)		17.35	17.59		16.28	18.30		18.94	21.77		15.37	17.71
1st stage Over-ID test (p-val)		0.32	0.36		0.37	0.62		0.91	0.95		0.67	0.73
2nd stage adj. R2	0.43	0.43	0.35	0.07	0.07	0.05	0.25	0.23	0.23	0.02	0.02	0.03

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 (not reported): ln GDP; ln GDP, squared; ln population; ln MEAs; Chief executive party orientation; Openness; WTO and EU dummy; Polity; ln nominal exchange rate. Full results in appendix, Tables A1 to A4.

Table 5: Robustness check. Kyoto IV estimates. Long diff-in-diff estimator

Dep.var.:	Log emission		Log emission per GDP		Renewables share		Fossil share		Coal share		New clean share		Diesel price	
Sample:	base (1)	w/o trans (2)	base (3)	w/o trans (4)	base (5)	w/o trans (6)	base (7)	w/o trans (8)	base (9)	w/o trans (10)	base (11)	w/o trans (12)	base (13)	w/o trans (14)
Kyoto dummy	-0.14*	-0.13	-0.13*	-0.10	3.74**	3.73***	-3.91**	-3.02*	-1.89	-1.89	2.96***	3.12***	0.35***	0.26***
	(0.09)	(0.10)	(0.07)	(0.08)	(1.47)	(1.46)	(1.88)	(1.68)	(2.51)	(3.09)	(0.83)	(0.66)	(0.07)	(0.07)
No. of observations	146	131	171	156	120	105	120	105	120	105	120	105	139	124
1st stage overid test (p-value)	0.56	0.41	0.59	0.38	0.54	0.59	0.27	0.56	0.77	0.48	0.34	0.35	0.10	0.09
1st stage weak id test (F-stat)	16.68	22.30	18.60	29.78	15.12	22.09	15.12	22.09	15.12	22.09	15.12	22.09	17.76	23.56
2nd stage R^2	0.55	0.53	0.23	0.16	0.33	0.29	0.22	0.20	0.15	0.15	0.20	0.26	0.24	0.19
2nd stage F-stat	17.42	13.39	30.52	21.71	9.21	7.50	6.04	5.62	1.36	1.18	3.06	5.57	8.01	5.32

Dep.var.:	Gasoline price		Log energy use per capita		Log electricity use per capita		Manufacturing share		Log GDP		Net trade over GDP		Net energy import share	
Sample:	base (15)	w/o trans (16)	base (17)	w/o trans (18)	base (19)	w/o trans (20)	base (21)	w/o trans (22)	base (23)	w/o trans (24)	base (25)	w/o trans (26)	base (27)	w/o trans (28)
Kyoto dummy	0.37***	0.30***	-0.09***	-0.07*	-0.17***	-0.19***	-1.17	-2.12**	-0.04	-0.09	0.92	1.39	0.60**	0.54*
	(0.07)	(0.07)	(0.03)	(0.04)	(0.04)	(0.05)	(0.76)	(0.99)	(0.10)	(0.09)	(2.56)	(2.92)	(0.31)	(0.29)
No. of observations	139	124	120	105	119	104	157	143	147	132	129	114	120	105
1st stage overid test (p-value)	0.70	0.66	0.58	0.61	0.25	0.17	0.67	0.66	0.49	0.75	0.09	0.16	0.28	0.25
1st stage weak id test (F-stat)	17.76	23.56	25.24	30.82	24.88	30.67	20.4	29.47	16.17	21.93	10.69	12.14	15.12	22.09
2nd stage R^2	0.17	0.16	0.26	0.25	0.27	0.27	0.10	0.12	0.20	0.15	0.18	0.19	0.12	0.13
2nd stage F-stat	6.80	4.41	7.12	6.78	3.52	3.18	3.52	3.82	4.61	2.39	3.73	3.18	0.94	0.94

Note: Long diff-in-diff estimation on pre- and post-treatment period averages: 1997-2000 (pre); 2004-07 (post). Heteroskedasticity-robust standard errors in parentheses. * $p<0.1$,** $p<0.05$, *** $p<0.01$. Instruments for Kyoto variable: membership in International Criminal Court and spatial lag thereof. All regressions use an array of additional controls as in Tables 3 to 4. EU dummy not included. In columns (3), (4), (21) and (22) variables dropped if insignificant in both the first- and second-stage regressions.

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