Measuring the impact of multiple air-pollution agreements on global CO_2 emissions

Aurélie Slechten

Corresponding author.

The author is lecturer, Economics Department, Lancaster University Management School, United Kingdom.

Address: Lancaster University Management School, Bailrigg, LA1 4YX, Lancaster, United Kingdom.

E-mail: a.slechten@lancaster.ac.uk. Telephone number: +44 1524 594681

Vincenzo Verardi

The author is research fellow, Belgian National Science Foundation (FNRS), and professor, Center for Research in Economic Development (CRED), University of Namur, Belgium.

Address: Rempart de la Vierge, 8, B-5000 Namur, Belgium

Abstract

This paper studies the effect on carbon dioxide emissions of the various agreements that follow the Long-Range Transboundary Air-Pollution Convention and are related to acid rain problems. The analysis is based on a panel dataset of 150 countries over the period 1970 - 2008 and deals with the problems linked to the analysis of multiple agreements (e.g. time and membership overlap). We show that ratifying an additional treaty has a significant and negative impact on the level of CO_2 emissions, even if it is not targeted toward CO_2. (JEL: Q53, Q54)

I. INTRODUCTION

International agreements to control transboundary externalities have received increasing attention from policy-makers and scholars, driven by the acknowledgement of global problems such as climate change or ozone layer depletion as well as more regional problems associated with acid rains. A common feature of these international treaties is that they are generally designed to control emissions of one single pollutant. For example, the Kyoto Protocol aims at reducing carbon dioxide (CO_2) emissions, the main cause of global warming, while more conventional air-pollutants (e.g. sulfur dioxide SO_2, nitrogen oxide NO_x or volatile organic compounds VOC) are the targets of international treaties that follow the 1979 Convention on Long-Range Transboundary Air-Pollution (the 1979 LRTAP Convention, hereafter).

In reality, a single source of emissions is typically composed of multiple pollutants that simultaneously cause global and/or more regional environmental damages. For example, Barker (1993, p. 9) calculated that in the United Kingdom, the burning of fossil fuels is responsible, apart from CO_2 for over 99% of SO_2 and NO_x, 91% of particulate matter and 38% of VOC emissions, which imply more regional or local environmental damages (e.g. acid rains, degradation of ambient air quality).[1]

As they are emitted by a single source, existing abatement technologies may have joint effects on this multiplicity of pollutants. These effects can go in both directions. In this paper, we consider the case of acid rains control and CO_2 emissions. Different options are available to reduce SO_2 emissions. Some of these options, like switching from burning coal to burning natural gas would imply SO_2 as well as CO_2 emissions reductions.[2] On the other hand, scrubbers installed in power plants to neutralize SO_2 emissions use energy and therefore lead to more CO_2 emissions.[3] In the same way, switching from high-sulfur to low-sulfur coals can lower SO_2 emissions. These low-sulfur coals have a lower heat value; so more coal must be burned to generate a given amount of output leading to higher CO_2 emissions (see Barker et al. 1997).

An international treaty foreseeing abatement of one of these air-pollutants, like SO_2, may also have a significant ancillary impact on CO_2, and, as a consequence, on the design of future international climate agreements. Indeed, these ancillary effects will alter the cost-benefit calculations underlying policy targets. The objective of this paper is twofold: (1) estimate the impact on CO_2 emissions of international treaties that follow the 1979 LRTAP Convention and that address conventional air-pollutants such as SO_2, NO_x or VOC, and (2) derive some implications in terms of climate change policies

Identifying the effect of an agreement raises two problems: (1) endogeneity of the treaty ratification variables resulting from omitted variables or reverse causality (i.e. countries' incentives to ratify agreements may depend on their emission levels) and (2) timing effects of the treaty (i.e. effects may start early or be bunched at a future date). As we analyze the effect of multiple treaties, the identification challenge becomes higher because they overlap in time and in terms of signatory countries. There may not be sufficient heterogeneity between them to identify their individual effects.

We deal with the problem of endogeneity by instrumenting the decision to ratify an air-pollution agreement using the status of the death penalty as a proxy for universalism or progressivism. We deal with timing effects and time and membership overlap issues together. Since agreements that follow the 1979 LRTAP Convention are relatively similar in terms of their timing and signatory countries, it is impossible to identify the effects of these agreements individually. To overcome these issues, we group LRTAP treaties into a single variable. The idea behind this assumption is that agreements related to the same air-pollution issue (i.e. here acid rains) are linked and should have a similar impact on CO_2 emissions.

To the best of our knowledge, this paper is the first attempt to estimate empirically the impact on CO_2 emissions of various treaties not specifically targeted toward these emissions. Interestingly, LRTAP treaties are associated with statistically significant CO_2 emissions reductions. Even if these reductions are non-negligible, they are not large enough to completely forego negotiations of an international climate agreement. However, the ancillary effect identified in this paper suggests that LRTAP treaties may have a role to play in the future climate policies. For example, these two pollution issues could be tackled together at the international level in order to take the ancillary benefits into account and potentially achieve extra CO_2 emissions reductions.

The approach used in this paper differs from the existing empirical literature on international environmental agreements (Murdoch and Sandler 1996; Bratbeg et al. 2005; Aakvik and Tjøtta 2011) by considering multiple non CO_2-specific agreements at the same time, instead of focusing on a single one. It points out the limitations of studying the effects of each treaty in isolation. In line with this idea, Egger and Wamser (2012) challenge the existing literature on preferential agreements, which focuses on one policy area, by providing evidence of an important overlap in the conclusion of different types of preferential economic integration agreements.

Some papers deal with potential interactions between air-pollution issues, but they are either purely theoretical models or numerical simulations, e.g. integrated cost-benefit analyses. Ambec and Coria (2013) demonstrate that the optimal policy instrument (e.g. taxes, tradable permits or a mix between the two) in the presence of two pollutants depends on whether there are economies or diseconomies of scope in the joint abatement cost function. Caplan and Silva (2005) consider multiple pollutants causing regional and global damages and characterized by abatement externalities (i.e. emissions abatement of one pollutant has an ancillary impact on the emissions of the other pollutant). They show that the use of a global permit market to control CO_2 emissions that would be linked with regional permit market to control regional pollutants may lead to Pareto superior outcome.

Some papers using the numerical simulations (Burtraw et al. 2001; Driscoll et al. 2015) analyze a question that is the mirror of ours: they look at the impact of GHG mitigation policies on conventional air pollutants and air quality and find a positive relationship. Bollen et al. (2009) study the link between climate change policies and policies designed to reduce local pollutants (e.g. particulate matters) and conclude that combining both policies achieves extra CO_2 emissions reductions that are higher than what follows from the sum of the application of either policy alone. However, in their model, abatement of local pollutants does not generate ancillary CO_2 emission reductions. Our paper thus provides an additional argument in favor of combining both policies.

The structure of the paper is the following: Section II describes the data and the identification strategy. Section III reports the results for different specifications. The results are then discussed in section IV. A sensitivity analysis is presented in section V. Section VI concludes.

II. DATA AND IDENTIFICATION STRATEGY

The aim of this paper is to study whether a country's participation in a non CO_2-specific air-pollution agreement has an impact on the level of CO_2 emissions of that country. In this section, we first describe our emissions and air-pollution treaties data. We then turn to the identification issues raised by our question.

Data

We use a panel dataset that covers 150 countries and 38 years (1970-2008). Data on CO_2 emissions (in kilotons) come from the World Development Indicator (WDI) Dataset (World Bank, 2012).[4] These data only include CO_2 emissions from energy-related sources (approximately 70% of total anthropogenic CO_2 emissions, see Stern 2006).[5]

A single source of CO_2 emissions is generally also responsible for other air-pollutants emissions. The typical examples are the so-called conventional air pollutants, e.g. SO_2, NO_x or VOC (see Barker 1993). To select the international agreements targeting air-pollutants released with CO_2 emissions in most industrial processes, we refer to the International Environmental Agreements (IEA) Database Project (Version 2012.1). It provides for each country a list of the environmental agreements in which the country is involved, with the signature, ratification and entry into force dates, and when relevant the withdrawal date.[6] In the IEA Database, the agreements of interest for this analysis belong to the *Long-Range Transboundary Air-Pollution* lineage, which consists of one initial convention, 8 protocols and 15 amendments and that are targeted to conventional air-pollutants, responsible for acid rains or degradations in ambient air quality.

This lineage started with the 1979 Convention on Long-Range Transboundary Air Pollution, which followed increasing concerns by policy-makers about the harmful effect of transboundary pollution caused by SO_2 or NO_x emissions that can travel some hundreds of kilometers before deposition. This initial Convention served as a basis for eight follow-up protocols and a series of amendments. In our analysis, we cannot include all these treaties because they are not all comparable. We only include those that satisfy the three following criteria: (1) the objective of the treaty is the reduction of emissions of some air-pollutant, (2) the treaty includes explicit quantified emission reduction targets, and (3) it should involve the country (i.e. it should not rely on the tacit acceptance procedure).[7]

The 15 amendments rely on the tacit acceptance procedure and are thus deleted (these are mainly technical modifications of the original treaty). The initial 1979 LRTAP Convention is also dropped because it does not include explicit targets. It only provides for the establishment of institutions entitled to negotiate the subsequent protocols. For the same reason, the 1984 monitoring and evaluation protocol EMEP, which only requires that signatories report their emissions to the treaty secretariat, is also dropped. We are left with seven treaties related to air-pollution that include emissions reductions targets for ratifying countries. Details on these agreements can be found in Table 1.

[INSERT TABLE 1 HERE]

We will assume that an agreement's year of ratification in national parliaments is the point in time from which this agreement has an impact on emissions. Ratification is preferred to signature because ratification involves political parties, the media, and the general public, while the signature of an agreement has no immediate political relevance. This choice is in line with other empirical analyses of international environmental agreements (e.g. Bratberg et al. 2005; Aichele and Felbermayr 2012): there exists some anecdotal evidence that countries have engaged in policy initiatives after the ratification of an agreement and before its entry into force.

Figure 1 shows the number of tons of CO_2 per capita emitted by each country as a function of the number of ratified LRTAP agreements by this country for the years 1985, 1995 and 2005. Among the 150 countries of the sample, there is a lot of heterogeneity in terms of ratification behavior. Some countries, like China, did not ratify any agreement during the sample period. The United States and the United Kingdom have ratified respectively 3 and 6 treaties in 2005. In general, European countries are the ones that have ratified the largest number of agreements.

[INSERT FIGURE 1 HERE]

The gap in the number of ratifications between the United States and the United Kingdom has increased sharply since 1995. Figure 1 also shows that only the UK has reduced its emissions between 1985 and 2005 (i.e. the period during which LRTAP treaties have been ratified). This reduction is of about 9% over the period. Looking at the data, a similar pattern arises for other European countries. In the US, emissions first increase between 1985 and 1995 and then decrease by only 4% between 1995 and 2005. China's emissions have increased sharply during this period, while the number of agreements ratified by this country remained at zero. From Figure 1, one might believe that it is because the UK and other European countries have ratified many treaties that they were able to reduce their emissions while China and the US still accounted for approximately 40% of total world emissions in 2008.

Identification strategy

The first insights from Figure 1 do not account for the fact that the changes in the emission behavior can be due to spuriousness: other variables can explain the emission behavior of the ratifiers. Additionally to confounding effects, we need to deal with four problems when identifying the effects of multiple agreements on CO_2 emissions: (1) time and membership overlap (is there sufficient variation in terms of treaties' timing and signatory countries), (2) timing effects (the effect of an agreement does not necessarily occur immediately after its ratification) (3) persistence of CO_2 emissions (due to the substantial inertia of some of CO_2, it is plausible to assume that this year's CO_2 emissions are dependent on the CO_2 emissions of previous years), and (4) endogeneity of a treaty ratification. We detail below how we overcome these issues.

Controlling for confounding effects

Spuriousness can be checked for by making use of control variables. The following model examines how CO_2 emissions react to the ratification of air-pollution agreements controlling for other variables:

$$\log\,(CO_2)_{it} = \alpha_i + \delta_t + \beta X_{it-1}^{k} + \mathbf{Z_{it}}\boldsymbol{\gamma} + \varepsilon_{it} \qquad [1]$$

In equation (1), *i* denotes the country and *t* the year. Variables are defined as follows: $\log\,(CO_2)_{it}$ is the log of total CO_2 emissions of country *i* in year *t* (in kilotons).[8] α_i is the country fixed effect, δ_t is the time fixed effect. These fixed effects control for unobservable country-heterogeneity and common time-varying effects that could affect emissions (see Hsiao 1986). Controlling for unobserved heterogeneity is needed to capture factors such as country specific technology, regulation or ideology or world business cycles. The variable of interest X_{it-1}^{k} is a dummy variable, where k is the reference number of the agreement in Table 1, defined as:[9]

$$X_{it-1}^{k} = \begin{cases} 1 & \text{if country } i \text{ has ratified the agreement } k \text{ by time } t-1 \\ 0 & \text{otherwise} \end{cases}$$

As treaties are not systematically ratified on the first of January of year *t*, we consider that a treaty ratified in *t-1* will have an impact on CO_2 emissions from year *t*. β is the coefficient of interest. It represents the yearly average effect of the ratification of an agreement *k* by country *i* on this country's emissions compared to business-as-usual emissions after controlling for a set of covariates. This coefficient may be positive or negative depending on the options used to curb conventional air-pollutants (e.g. scrubbers or fuel-switching).

$\mathbf{Z}_{\mathrm{it}}$ is the matrix containing the control variables. Summary statistics for these control variables are presented in Table A1 in Appendix A. Data are available from the WDI Database (World Bank 2012) and the Polity IV Database. The first economic factor that we include as a control variable is total Gross Domestic Product (GDP). The GDP data are reported in constant 2000 US dollars. We expect a significant positive relationship between GDP and emissions. The intuition is simple: a higher economic activity induces, ceteris paribus, a higher level of pollution due to increased resource use and waste generation (Panayotou 1997; Stern 2002).[10] We also include the GDP growth rate to account for the short-term variations in the economic activity (business cycles). Indeed, following van Vuuren and Riahi (2008), economic growth is expected to have both a positive effect on CO_2 emissions (due to the increase in energy demand) and a negative effect (due to the improvement in energy efficiency).

Following the international trade literature (see for example, Copeland and Taylor 2004), trade openness is assumed to affect the level of CO_2 emissions in two different ways: (i) increased trade may result in more CO_2 emissions due to an enhanced economic activity, (ii) increased trade may result in reduced CO_2 emission because countries face greater competitive pressure and become more efficient in resource use (Cole 2004). We define trade openness as the sum of exports and imports of goods and services divided by GDP.

Next, we control for the total population given that population size may contribute to CO_2 emissions through increased energy demand from the power, industry or transport sectors (Li and Reuveny 2006; Shi 2002). Since the composition of the economic activity may also influence the level of CO_2 emissions (see Stern 2002), we include the shares of agricultural and industrial productions in GDP. Indeed, industrial and agricultural sectors are more resource-intensive than the tertiary sector. Following Neumayer (2002), who finds a positive relationship between the number of multilateral environmental agreements ratified by a country and the level of democracy in this country, we include a *Democracy* indicator available from the Polity IV Database. This indicator measures countries' institutionalized democracy. It is an additive eleven-point scale (0-10), zero being the worst situation for democracy.[11]

Our last control is $logMEA_{it}$, which counts the number of multilateral environmental agreements other than the LRTAP treaties that country *i* has ratified up to year *t*. Since the early 1990s, there has been a growing political concern about CO_2 emissions and their impact on climate change (Barrett 2003). Some countries have been more proactive in dealing with this issue (i.e. they have been *greener*) and it is reasonable to assume that countries that have ratified LRTAP treaties are also greener than those that did not. Therefore, LRTAP treaties may in fact capture growing national CO_2 emissions policies adopted in greener countries. In order to control for that, we include $logMEA_{it}$ as a proxy for a country's environmental awareness (see Aichele and Felbermayr 2012).

Time and membership overlap

To correctly identify the effects of the seven LRTAP treaties included in the analysis, there must be sufficient heterogeneity in terms of the timing of the agreements and in terms of the ratifying countries. To check for this, we refer to Tables 1 and 2.

[INSERT TABLE 2 HERE]

First, as shown in Table 1, the number of ratifiers at the end of our sample period is roughly similar for all LRTAP agreements (i.e. it ranges from 19 to 29). Moreover, the identity of the ratifiers is also much the same across them. This can be seen from Table 2, which reports the correlations between the dummies X_i^k for the year 2008 (the last year of our sample, and thus the year for which the membership overlap is the highest). These correlations are very high (e.g. above 0.7 for most pairs of treaties), indicating a low heterogeneity in terms of membership between LRTAP protocols. Second, as shown in Table 1, treaties have been ratified since the end of the 1980s until 2005, but the time span between two agreements is relatively short (generally less than 5 years).

Due to this time and membership overlap, the treaty dummies X_{it-1}^k will be highly correlated through time and across countries and identifying accurately the effect of each individual agreement is problematic. We thus aggregate the agreements in a single variable. Our argument behind this strategy can be found in their patterns of development. Countries first agree on an umbrella convention, i.e. the 1979 LRTAP Convention under the auspices of which all subsequent protocols and amendments are negotiated. These protocols are thus related. We create a new variable, $LRTAP_{it-1}$, which is the sum of dummies X_{it-1}^{k} ($k=1... 7$) for country i in year $t-1$, and we replace X_{it-1}^{k} by $LRTAP_{it-1}$ in equation (1).

With this definition, we will not be able to capture the impact of each individual treaty but we will look at the average effect of the accumulation of treaties. The estimated coefficient will be the average effect of an additional air-pollutant treaty on a country's level of CO_2 emissions. From Figure 1, it can be seen that the variable *LRTAP* varies over time and between countries.

Timing effects

To analyze the timing issue, we refer to Table 3, which reports the dates at which emission targets foreseen in agreements should be met and the year by which a treaty has been ratified by 50% of member countries. It is possible that the effect of an agreement does not occur immediately after its ratification, i.e. targets should not be met right after the ratification and implementing domestic air-pollution control policies may take time. To try to remedy this problem, we change the point in time from which a treaty has an impact on emissions by using alternative definitions of the *LRTAP* variable: (1) a treaty has an impact after it enters into force and (2) a treaty has an impact t years after its ratification (where $t =1... 4$).

[INSERT TABLE 3 HERE]

The last timing issue concerns the Kyoto Protocol. Aichele and Felbermayr (2012) show that domestic CO_2 emissions in committed countries have been reduced by about 7% after the ratification of the Kyoto Protocol, while the share of imported over domestic emissions in those committed countries has increased by about 14%. As our sample period covers the period during which the Kyoto Protocol has been adopted (1997) and ratified (mainly 2002), this may have an impact on our results. We discuss this issue in the sensitivity analysis.

Persistence of CO_2 emissions

Equation (1) is in some sense *static*. Due to the substantial inertia of the dependent variable, it is plausible to assume that this year's CO_2 emissions are dependent on the CO_2 emissions of previous years. This is why we introduce a lagged dependent variable in our model:

$$\log{(CO_2)_{it}} = \alpha_i + \delta_t + \rho \log(CO_2)_{it-1} + \beta LRTAP_{it-1} + \mathbf{Z_{it}\gamma} + \varepsilon_{it} \qquad [2]$$

ρ is the coefficient of the lagged dependent variable. The coefficients of the explanatory variables, β and $\boldsymbol{\gamma}$, have different interpretations compared to the previous basic *static* specification. They are the estimated responses of CO_2 emissions to changes in the explanatory variables, after controlling for the response for the previous years.

Some econometric problems arise from estimating equation (2): CO_2 may be non-stationary and the lagged dependent variable is correlated with the error term. The coefficients of the regressors may thus be seriously biased when estimating equation (2) with OLS. Note however that this bias decreases when the number of periods becomes large. Taking the first difference transformation and using the Anderson-Hsiao (AH) estimator allow to avoid these problems:

$$\Delta\log{(CO_2)_{it}} = \delta_t - \delta_{t-1} + \rho\Delta \log(CO_2)_{it-1} + \beta\Delta LRTAP_{it-1} + \mathbf{\Delta Z_{it}\gamma} + \Delta\varepsilon_{it} \qquad [3]$$

Where $\Delta \log (CO_2)_{it}$ is instrumented using lags 2 to 4 of $\log (CO_2)_{it}$.

Arellano and Bond (1991) argue that the AH estimator, while consistent, fails to exploit all the information available in the sample. For this reason, we also estimate equation (3) using the Arellano-Bond estimator.

Dealing with endogeneity

CO_2 emissions may depend on many other factors that are not included in our control variables. As some of these omitted variables may drive both CO_2 emissions and the decision to ratify a LRTAP treaty, our results may be biased. By using a two-way fixed effects model, we control for unobservable country-heterogeneity and common time varying effects, but some omitted variables may vary across countries and over time (e.g. a country's level of concern about environmental issues or the amount of foreign direct investments in that country). For example, foreign direct investments will have an impact on a country's level of CO_2 emissions. At the same time, the *Pollution haven hypothesis* predicts that, as FDI provide economic benefits to the host country, this country may be reluctant to ratify additional air-pollution agreements, which would impose stricter environmental regulations and would drive these FDI out of the country.[12]

Furthermore, even if LRTAP treaties are not targeted towards CO_2 emissions, there may also exist a reverse causality problem between these two variables. Air pollutants covered by LRTAP treaties are very often emitted together with CO_2 emissions, which implies that large CO_2 emitters can also emit large amounts of conventional air-pollutants. These large emitters would incur the greatest cost of reducing emissions. As air-pollutants can travel some hundreds kilometers before deposition, these countries do not necessarily enjoy the entire environmental benefits of their actions and may be reluctant to ratify LRTAP treaties.

To deal with these problems, we use an Instrumental Variable approach. Our instrument is an index measuring the status of the death penalty. It is constructed as follows:[13] we measure the status of the death penalty on a five-point scale (0-4), from constitutional authorization of the death penalty (0) to abolition of the death penalty for any offense in both peace and war periods (4) (see Table 4 for details on scores).

We argue that this is a valid instrument for the four following reasons that will be detailed below: (1) it is a relevant instrument to measure the propensity of a country to ratify air-pollution agreements, (2) the status of the death penalty does not affect the level of CO_2 emissions, (3) the level of CO_2 emissions does not influence the countries' decisions about the death penalty, and (4) the index varies sufficiently over time and across countries.

First, the pace at which a country ratifies international environmental agreements may be explained by its *universalism*, i.e. the meta-ethical conviction that some system of ethics applies universally (e.g. for every individual, independently of their culture, religion, nationality, sexuality...). Indeed, a country that is strongly universalist will be keener to ratify international agreements related to public goods because these treaties are ways to apply this system of ethics universally. Our idea is to use universalism as an instrument for treaties' ratification that is not directly related to CO_2 emissions. We believe that the pace at which the death penalty is abolished, but also the legalization of homosexual marriage or euthanasia, can be seen as symbols, and therefore as proxies, for progressive or universalist societies.

Second, this instrument does not affect the level of CO_2 emissions directly and it is obviously not caused by the level of CO_2 emissions. However, there may be a concern that the abolition of the death penalty might be driven by economic development, which in turn correlates with CO_2 emissions. We believe this should not be a major concern. On the one hand, we control for economic development in our analysis through our control variable GDP. On the other hand, there is some anecdotal evidence that this is not always the case: the United States and Japan, which are already very developed countries (they are amongst the countries with the highest GDP per capita levels in our database) both still constitutionally authorize the death penalty, while the Ivory Coast or Honduras, which are at an early stage of development have *de facto* abolished the death penalty since the 1960s.[14]

On a more rigorous level, Neumayer (2008) estimates that the most important determinants of abolition are political and that economic development does not matter for domestic death penalty abolition (see also Greenberg and West 2008). Note that we will test for the strength of our instrument in the Results Section. These tests will confirm us in our choice of the death penalty as an instrument.

Finally, to be a good instrument in the context of panel data, there must be sufficient heterogeneity among countries regarding the abolition of the death penalty and the index must also vary over time.[15] As shown in Table 5, in nearly 70 % of countries, the status of the death penalty has changed at least once between 1970 and 2008. The status of the death penalty also varies across countries (see Table 4). Moreover, the average death penalty index seems to vary significantly over time, as shown by Figure 2.[16]

[INSERT TABLE 4 HERE]

[INSERT TABLE 5 HERE]

[INSERT FIGURE 2 HERE]

III. RESULTS

Individual agreements

As an illustration, we first estimate equation (1) for each individual agreement *k* (*k=1... 7*) in Table 1.[17] We only present the results for the variables of interest X_{it-1}^{k} in Figure 3.[18] It appears that all the LRTAP treaties have a significant negative impact on CO_2 emissions. Furthermore, their effects are relatively similar. However, as mentioned above, due to the substantial overlap in terms of membership and timing, the impact of each individual treaty will not be estimated accurately. This is why in the next section we turn to models in which agreements are grouped into one variable that counts the number of agreements ratified by each country, *LRTAP*.

[INSERT FIGURE 3 HERE]

Accumulation of treaties

Table 6 presents the results for the *LRTAP* variable of the various specifications (equations (1)-(3)) detailed above. Equations (1) and (2) are estimated using a standard panel two-way fixed effects estimator. To control for heteroskedasticity and within country serial correlation, standard errors are estimated using the Huber-White sandwich estimator, clustered at the country level. Results are shown in the first two columns. The last three columns refer to equation (3). In these last three columns, standard errors are also clustered at the country level.

[INSERT TABLE 6 HERE]

In column 1 of Table 6 (static specification), the ratification by one country of each additional LRTAP agreement is associated with a reduction by approximately 4% of its CO_2 emissions. When we turn to a dynamic model, results in column 2 suggest a strong inertia in CO_2 emissions since the estimated coefficient of the lagged dependent variable is $\hat{\rho} = 0.793$. The effect of LRTAP agreements is still negative and statistically significant. Note that this is a *short*-term effect, i.e. the effect after controlling for the response of the previous years.

As noted in the previous section, some econometric problems arise from estimating equation (2): CO_2 emissions may be non-stationary and the lagged dependent variable is correlated with the error term. We run some panel unit root tests. Results are shown in Table 7. For all the tests, we reject the null hypothesis of the existence of unit roots in all panels. Our initial dynamic fixed-effect model would thus be fine as the bias of the autoregressive term would be negligible given the relative long time span of the data. However, when we run country-specific panel unit root tests, we find that about 21% of panels contain a unit root.[19]

[INSERT TABLE 7 HERE]

For this reason, we turn to the model in first difference, i.e. equation (3), estimated using the AH estimator (columns 3 and 4) and the AB estimator (column 5). In column 3, we only instrument the lagged dependent variable in first difference ($\Delta\log\,(CO_2)_{it}$) using lags 2 to 4 in level. In column 4, we deal with the problem of endogeneity by assuming that treaties' ratification may be endogenous and by instrumenting the differenced *LRTAP* variable ($\Delta LRTAP_{it-1}$) with the death penalty index in level. In columns 3 to 5 of Table 6, the coefficient of *LRTAP* remains negative and significantly different from zero. The value obtained with the AH estimator when treaties' ratification is also instrumented (column 4), seems too high: each additional treaty ratified by one country reduces the CO_2 emissions in that country by approximately 9%. As mentioned earlier, the AH estimator fails to exploit all the information available in the sample and the coefficient of interest may be very imprecisely estimated in column 4. The AB estimator in column 5 provides a more efficient estimator than AH and we will consider it as our final result.

The effect of *LRTAP* is negative and significant at the level of 5%: on average, ratification of an additional treaty has a short-term impact of 2.4% on CO_2 emissions, i.e. after controlling for the response of previous years. Obviously, the estimated coefficients in the dynamic and static models are not directly comparable. However, in the dynamic specification, the cumulative effect of an agreement on CO_2 emissions can be computed as $\beta/(1-\rho)$, where $\beta = -0.024$ is the short-term coefficient and $\rho = 0.706$ is the coefficient of the lagged dependent variable. With our estimates, this cumulative effect is thus equal to approximately 8.2% for LRTAP treaties, suggesting that the effect estimated with the static specification (4%) was probably underestimated.

To the best of our knowledge, there do not exist tests of the strength of instruments in AB models. We rely on the results of the first-stage AH estimator as is generally done in the literature. To test for the validity of our instruments, we first look at the first-stage equations of the model in column 4, which are given by:

$$\Delta y_{it-1}^{j} = \bar{\delta}_t - \bar{\delta}_{t-1} + \psi_1^j DP_{it-1} + \psi_2^j \log(CO_2)_{it-2} + \psi_3^j \log(CO_2)_{it-3} + \psi_4^j \log(CO_2)_{it-4} + \mathbf{\Delta Z_{it}\theta^j} + \Delta u_{it} \quad [4]$$

For $j = 1,2$.

Where $y_{it-1}^1 = LRTAP_{it-1}$, $y_{it-1}^2 = \log(CO_2)_{it-1}$ and DP_{it-1} is the death penalty index.[20]

[INSERT TABLE 8 HERE]

Column 1 in Table 8 shows the results for $\Delta\log(CO_2)_{it-1}$ and column 2 for $\Delta LRTAP_{it-1}$. Death penalty seems to be a good determinant of the ratification of LRTAP agreements.[21] The strength of the instruments (the lagged dependent variable in level and the status of the death penalty) is further checked with tests presented in Table 9. Instruments are quite strong. Indeed, we are sure at 95% that the maximal bias associated with the coefficient of interest is less than 10% of the OLS bias (weak identification test).[22] From the under-identification test, we can conclude that the first-stage equation is identified, i.e. the excluded instruments (*Death Penalty* and lags 2 and 4 of $\log(CO_2)$) are relevant (correlated with the endogenous regressor).[23]

[INSERT TABLE 9 HERE]

We also present the Arellano-Bond tests for AR(1) and AR(2) (See Table 10), for which the null hypothesis is that there is no autocorrelation in the error term. AR(1) is expected in first differences, because the differenced error terms in *t* and *t-1* both contain the ε_{it} term. To check if our instruments in levels are good instruments for the first-difference, we need to look at AR(2). Autocorrelation indicates that lags of the dependent variable (and any other variables used as instruments) are in fact endogenous, thus bad instruments. As shown in Table 10, we cannot reject that our instruments in level are valid instrument.[24]

[INSERT TABLE 10 HERE]

Finally, for the other results, most of the control variables have the expected sign. A higher GDP level is associated with higher CO_2 emissions. The coefficients of trade openness and population are positive but not significant. Both the shares of agricultural and industrial productions imply an increase of CO_2 emissions, but they do not have a significant impact. Democracy has a positive effect on CO_2 emissions (but only significant at the 10% level). The GDP growth rate coefficient has a negative sign in the static specification of column 1, but a positive sign in the dynamic specifications (indicating increases in energy consumption that seem to offset energy efficiency improvements during periods of economic growth).[25]

IV. INTERPRETATION OF THE RESULTS

The record of recent climate negotiations (e.g. Cancun, Copenhagen...) demonstrates that an ambitious climate change agreement is very difficult to achieve. By contrast, pollutants covered by LRTAP agreements (SO_2, NO_x or VOC) are local pollutants with larger and more visible short-term health benefits (Burtraw et al. 2001) than the long-term benefits obtainable through climate change measures (e.g. due to more substantial discounting). Treaties on these types of pollutants will thus be easier to reach and lead to a higher commitment by politicians. However, this does not imply that policy-makers should concentrate only on these local pollutants and completely forego achieving self-enforcing international agreements on CO_2 emissions.

Our results show that, even if they are not directly targeted towards CO_2 emissions, each additional LRTAP treaty is associated with an annual reduction of CO_2 emissions of approximately 2.4% and this effect accumulates overtime (i.e. 8.2% in the long-term). How can we interpret this estimate in terms of the future CO_2 emission reductions that some countries want to implement?

In advance of the Conference of the Parties in Paris (COP 21), some countries have submitted Intended Nationally Determined Contributions (INDC) containing, implicitly or explicitly, emission reduction commitments. For example, the United States have pledged to cut net greenhouse-gas emissions by 26% to 28% (relative to 2005 levels) by 2025, while the European Union has pledged to cut GHG emissions by at least 40% (relative to 1990 levels) by 2030 (IEA, 2015). Relative to 2008 levels (last year of our sample), these objectives correspond to a reduction in emissions of 24% by 2025 for the US and 36.5% by 2030 for the EU.

In order to evaluate the impact of the LRTAP treaties, we can compute what would have been the emissions in the absence of these agreements (assuming a yearly reduction of 2.4% which accumulates over time), for each country and each year of our sample. Emissions in the US would have been 21% higher in 2008, while emissions from the 27 countries member of the EU in 2008 would have been 41% higher. If the US and EU countries wanted to reach the same levels of emissions as those proposed in the current INDC,[26] considerably more effort would have been required: a reduction of 40.6% relative to 2008 level for the US and 63% for the EU. Given our estimates, the impact of LRTAP treaties on current CO_2 emissions is thus non-negligible.

Another way to interpret our results is in terms of the *remaining carbon budget*. According to a recent report released by the International Energy Agency (IEA 2015), with the INDC submitted for the COP21, the world's estimated remaining carbon budget consistent with a 50% chance of keeping the rise in temperature below 2 degrees Celsius will be consumed by around 2040. If we assume that a new LRTAP agreement is ratified by all countries in 2015, then in 2040, the world will have saved at least 7.4% of its carbon budget.[27]

In view of these examples, the impact of LRTAP treaties on CO_2 emissions is non-negligible but clearly not large enough to rely only on these policies to solve the climate change issue. An international self-enforcing agreement with CO_2 emission reduction targets will still be necessary. Nevertheless, the abatement complementarity between CO_2 emissions and conventional air-pollutants identified in this paper may have some implications regarding the design of the future climate change policies. Namely, it provides an empirical argument to jointly negotiate on regional and global pollution issues at the next COP. This could be particularly relevant for developing countries with sizeable CO_2 emissions, the participation of which is essential to reaching an efficient climate agreement. If these countries also suffer domestically from bad air quality, the incentive to control conventional air-pollutants will be strong. For example, adopting acid rain control measures could lead to substantial ancillary benefits in terms of CO_2 emissions, which may help these countries reach CO_2 targets more easily.

An example of how to implement this combination of policies has been proposed by Caplan and Silva (2005). They theoretically show how a global permit market can be linked with regional permit markets to control local pollutants and how this may lead to a Pareto superior outcome that is self-enforcing. In their model, there are abatement complementarities between the local and global pollutants and, with an international joint permit market; the emissions caps on both pollutants in each region will take into account these complementarities.

V. SENSITIVITY ANALYSIS

In this section, we test the robustness of our benchmark results, i.e. that the ratification of each additional air-pollution treaty is associated with a significant reduction of CO_2 emissions. All the results in this section are obtained by estimating equation (3) using the AB estimator. Details of these robustness checks can be found in Appendix B. They are summarized below.[28]

Timing issues

As our main result may be sensitive to the choice of the point in time from which a treaty has an impact on emissions, we have re-estimated the model using alternative definitions of the variable of interest. Results are not reported in full but available upon request (see Tables B1 and B2 in Appendix B). We first consider that the effects of a treaty do not occur immediately, but only k years after its ratification (for $k=2,3,4$): the new variable of interest is $LRTAP_{it-k}$. Second, we use entry into force rather than ratification as the point in time from which a treaty has an impact on CO_2 emissions. The results are similar compared to those in column 5 of Table 6: the short-term impact of LRTAP agreements is still negative and significant.[29]

Including other agreements

We also test whether our result is really driven by treaty ratifications or whether only the participation to the LRTAP Convention matters (see Table B3 in Appendix B):[30] we substitute the variable $LRTAP_{it}$ by a dummy variable $Convention_{it}$, which is equal to 1 if country i has ratified the Convention by time *t*. The impact of the Convention is negative but not significant.

Initially, we excluded from our analysis the 1984 EMEP treaty because it only required that countries report their emissions to the treaty secretariat. However, one might suggest that transparency (achieved through emissions reporting) is a key factor to achieve explicit reduction targets. We therefore modify the variable $LRTAP_{it}$ by including ratification of the 1984 EMEP treaty. The impact remains significant and is a little bit higher than our main result (-0.026 instead of -0.024). This could suggest that the simple obligation of reporting has contributed to emission reductions achieved by the LRTAP treaties.

Effect of the Kyoto Protocol

The Kyoto Protocol has been ratified during the period analyzed in this paper (most ratifications occurred in 2002).[31] As a consequence, some CO_2 emission reductions at the end of our sample period may potentially be due to the Kyoto Protocol. To control for this, we estimate our model for two different sample periods: (1) before the adoption of the Kyoto Protocol (1970-1997) and (2) before its ratification (1970-2002). Results (not reported in full) are similar (and even stronger) compared to those in column 5 of Table 6 (see Table B4 in Appendix B).

Other set of controls

Environmental agreements might affect the level of imports/exports (our measure of trade openness) and, as it is included as control variables, our results may be biased. Moreover, the coefficients of some of our control variables (e.g. proportion of agriculture in GDP, growth rate or *logMEA*) are not significant. As shown in Table B5 (Appendix B), omitting these control variables does not change the main results (the size and the significativity of the results are even higher). Other control variables (e.g. the amount of foreign direct investments or the proportion of electricity production from natural gas sources, which is less sulfur and carbon intensive than coal for example) were also introduced in the AB specification, but this did not change the main results of the model (see columns 3 and 4 of Table B5). Other variables would have been interesting to study, such as the legal origin (see Stern 2012). However, these variables do not vary over time and are likely to be captured by the fixed effects or to disappear when we turn to the AH or AB estimations.

Sub-samples of countries

We test whether our benchmark results are not driven by a particular sub-sample of countries. The thrust of our argument continues to hold (see Table B6 in Appendix B). Air-pollution agreements have a negative impact on CO_2 emissions, whatever the sub-sample considered: (1) without the biggest SO_2 emitters (United States, United Kingdom, Poland, Spain and Germany), (2) without the countries that were the most affected by acid rains before the ratification of the LRTAP agreements (Scandinavian countries),[32] and (3) without economies in transition.[33]

Net CO_2 emissions

Our data on CO_2 emissions do not take into account emissions/removals from land use, land use change and forestry (LULUCF). The data used in this paper are thus *gross* CO_2 emissions. However, there are examples of countries, such as Russia, that have reduced their gross CO_2 emissions and at the same time have destroyed substantial parts of their forest area, thereby increasing their net CO_2 emissions. In this case, emission reductions are over-estimated since the destruction of forests, which are carbon sinks, increases the stock of CO_2 in the atmosphere. In order to get an idea of the effect of air-pollution agreements on net CO_2 emissions, we remove from our sample countries with the highest deforestation rates (information comes from http://www.grida.no). In other countries, the gross CO_2 emissions (our data) should be very similar to net emissions and the coefficient of the variable of interest for those countries should thus not be affected by the fact that we do not take into account removals from LULUCF. The results in Table B6 (Appendix B) show that the effect is similar but a little bit smaller (-0.02 rather than -0.024) when we remove countries with the highest deforestation rates.

VI. CONCLUSION

The objective of this paper is to test for the effectivity of air-pollution agreements on the level of CO_2 emissions. There is strong evidence that CO_2 (a global pollutant) is often released with more conventional air-pollutants. Pollution abatements imposed by international treaties targeted to these conventional pollutants may thus jointly reduce the flows of both types of pollutants. Our analysis focuses on the effects of the treaties that follow the 1979 LRTAP Convention.

We deal with different issues pertaining to the identification of the effect of these multiple agreements: (1) endogeneity, (2) timing effects and (3) time and membership overlap between treaties. The main result is that LRTAP agreements, even if they are not CO_2-specific, have a statistically significant negative impact on CO_2 emissions. This puts forward the limitation of studying the effects of an environmental agreement in isolation. This also suggests that these two pollution issues could be tackled together at the international level in order to take these ancillary benefits into account in future policy targets.

This paper is a first attempt to study the ancillary effects of multiple air-pollution treaties empirically. We identify potential ancillary benefits of LRTAP treaties for climate change issues. However, climate change is a very complex problem and this study can be extended in several ways. Among others, LRTAP treaties are also expected to have an impact on SO_2 emissions, which are turned into sulphate aerosols. They have only a short lifetime in the atmosphere, but have a substantial cooling effect and can thus postpone the impact of climate change (see Tol 2004). SO_2 reductions due to LRTAP treaties may thus partially offset carbon emission reductions by those treaties. This example shows that in order to design an optimal international climate policy, it is crucial to understand and estimate all the interactions between air-pollution and climate treaties and their respective outcomes.

APPENDIXES

Appendix A

[INSERT TABLE A1 HERE]

Appendix B

[INSERT TABLE B1 HERE]

[INSERT TABLE B2 HERE]

[INSERT TABLE B3 HERE]

[INSERT TABLE B4 HERE]

[INSERT TABLE B5 HERE]

[INSERT TABLE B6 HERE]

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Tables

TABLE 1

International Environmental Agreements related to air-pollution (Mitchell, 2002-2012)

Ref.	Agreement Title and signature date	Ratification starts in:	Members[a]	
			Starting year	2008
1	Protocol On The Reduction Of Sulphur Emissions Or Their Transboundary Fluxes By At Least 30 Per Cent (Helsinki, 1985)	1985	1	21
2	Protocol Concerning The Control Of Emissions of Nitrogen Oxides Or Their Transboundary Fluxes (Sofia, 1988)	1988	8	29
3	Protocol Concerning The Control Of Emissions Of Volatile Organic Compounds Or Their Transboundary Fluxes (Geneva, 1991)	1993	3	19
4	Protocol On Further Reduction Of Sulphur Emissions (Oslo, 1994)	1995	3	23
5	Protocol On Heavy Metals To The Convention On Long-Range Transboundary air-pollution (Aarhus, 1998)	1998	1	24
6	Protocol On Persistent Organic Pollutants To The Convention On Long-Range Transboundary air-pollution (Aarhus, 1998)	1998	1	23
7	Protocol To Abate Acidification, Eutrophication And Ground-Level Ozone To The Convention On LRTAP (Gothenburg, 1999)	2002	3	22

[a] Members are countries that ratify one particular treaty, either in the starting year (column 3) or in 2008.

TABLE 2

Correlation matrix for the year 2008

	1	2	3	4	5	6	7
1	1						
2	0.8242	1					
3	0.7706	0.7779	1				
4	0.6815	0.8693	0.8393	1			
5	0.6624	0.7533	0.7633	0.7732	1		
6	0.6815	0.7287	0.7836	0.7946	0.9246	1	
7	0.5387	0.7037	0.7486	0.7650	0.8471	0.7650	1

Note: numbers 1-7 are the reference numbers in Table 1.

TABLE 3

Targets of LRTAP treaties

Treaty	When must targets be achieved?	Median Ratification Year[a]
1985 Helsinki	Reductions should be met before 1993	1986
1988 Sofia	Reductions should be met by 31 Dec 1994	1990
1991 Geneva	Cap should be met by 1999	1995
1994 Oslo	Cap on 2000 emissions	1998
1998 Aarhus	Reductions should be implemented no later than 2011 (2005 for new installations)	2002-2003
1999 Gothenburg	Cap on 2010 emissions	2004

Source: www.unece.org/env/lrtap.

[a] Median Ratification year = year by which 50 % of the 2008 member countries have ratified the treaty.

TABLE 4

Number of countries for each value of the Death Penalty Index

Index	Definition	1970	1990	2008
0	Death penalty still used	111	72	40
1	Death penalty abolished *de facto* for ordinary crimes	0	0	0
2	Death penalty abolished *de facto* for all crimes (Ordinary and war crimes)	20	33	31
3	Death penalty abolished for ordinary crimes	10	13	9
4	Death penalty abolished for all crimes	9	32	70

Note: de facto means that a country still has the death penalty in its Constitution but has not called on it for at least ten years and/or that there is a moratorium on the death penalty.

TABLE 5

Number of changes in the death penalty index by country between 1970 and 2008

Number of changes in the index	Number of countries
0	48
1	49
2	40
3	13

TABLE 6

Estimating the effect of an agreement's ratification on CO_2 emissions

(Dependent Variable: *log(CO_2)*)	Basic FE	Dynamic FE	AH	AH (endog.)	AB
log(CO_2) (t-1)		0.793***	0.766***	0.693***	0.706***
		(0.016)	(0.127)	(0.141)	(0.041)
LRTAP (t-1)	-0.043***	-0.011***	-0.011**	-0.090**	-0.024**
	(0.013)	(0.003)	(0.005)	(0.046)	(0.009)
log(MEA) (t)	-0.049	-0.017	0.047	0.032	0.012
	(0.058)	(0.013)	(0.040)	(0.040)	(0.035)
log(GDP) (t)	0.954***	0.195***	0.136	0.184*	0.258***
	(0.104)	(0.029)	(0.096)	(0.105)	(0.042)
log(Population) (t)	0.530***	0.033	0.098	-0.070	0.070
	(0.191)	(0.045)	(0.181)	(0.208)	(0.126)
log(Openness) (t)	0.026	0.023*	-0.004	-0.008	0.038
	(0.055)	(0.014)	(0.030)	(0.030)	(0.029)
GDP Growth Rate (t)	-0.668***	0.315***	0.378***	0.318**	0.114
	(0.165)	(0.082)	(0.128)	(0.137)	(0.102)
log(Prop. Agriculture) (t)	0.113	0.038**	-0.037	-0.039	0.002
	(0.085)	(0.019)	(0.025)	(0.025)	(0.038)
log(Prop. Industry) (t)	0.317***	0.086***	0.028	0.034	0.066
	(0.090)	(0.022)	(0.068)	(0.063)	(0.050)
Democracy (t)	0.007	0.002*	0.002	0.002	0.006*
	(0.005)	(0.001)	(0.003)	(0.003)	(0.003)
Observations	4,275	4,253	4,059	4,059	4,109
Number of countries	150	150	149	149	150
Within R-squared	0.664	0.817	0.886	0.886	0.898

Note: Robust standard errors in parentheses. R-squared = squared correlation between the observed and predicted level of the dependent variable. *** p<0.01, ** p<0.05, * p<0.1.

TABLE 7

Fisher-type unit-root tests for *log(CO_2)* based on augmented Dickey-Fuller tests

Tests	Statistic	p-value
Inverse Chi-squared(300) P	483.884	0.000
Inverse normal Z	-3.207	0.001
Inverse logit (749) L*	-4.227	0.000
Modified inv. chi-squared Pm	7.507	0.000

Note: Number of panels: 150; Average number of periods: 36.02; H0: all panels contain unit roots; Ha: at least one panel is stationary.

TABLE 8

First-stage results

	Endogenous regressors	
Instruments	$\Delta log(CO_2)_{t-1}$	$\Delta LRTAP_{t-1}$
Death Penalty (t-1)	0.000	0.017***
	(0.002)	(0.003)
log(CO_2) (t-2)	-0.085**	-0.002
	(0.037)	(0.007)
log(CO_2) (t-3)	0.056	-0.004
	(0.034)	(0.010)
log(CO_2) (t-4)	0.024	0.010
	(0.035)	(0.007)
Other Covariates	YES	YES
F(4, 148)	8.43 (0.00)	8.78 (0.00)
AP Chi-Sq. (3) (underid.)	33.54(0.00)	35.74(0.00)
AP F(3,148) (weak id.)	10.97	11.69

Note: Stock-Yogo weak ID test critical values (at 5%) for single endogenous regressor: 9.08 (10% maximal IV relative bias). Robust standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Estimates for the other covariates are not included in the Table, but they are available upon request.

TABLE 9

IV Statistics (AH estimation with LRTAP endogenous)

Under-identification test	Kleibergen-Paap rk LM statistic	25.334
	Chi-sq. (3) p-value	0.000
Weak identification test	Kleibergen-Paap rk Wald F statistic	9.983
Critical value at 5%[a]	*10% maximal IV relative bias*	7.56

[a] Critical values for the Cragg-Donald Wald F statistic

TABLE 10

Autocorrelation tests for AB estimation

Test	Stat.	p-value
AB test for AR(1) in first differences	$z = -4.51$	0.000
AB test for AR(2) in first differences	$z = -0.40$	0.691

TABLE A1

Descriptive statistics for the control variables

	Obs.	Mean	Std. Dev.	Min	Max
log(GDP)	6800	22.925	2.380	16.148	30.088
log(Population)	8444	14.941	2.336	8.636	21.015
log(Openness)	6283	4.211	0.653	-1.707	6.100
GDP Growth Rate	6710	0.034	0.062	-0.714	0.724
log(Prop. Agriculture)	5788	2.429	1.149	-3.314	4.543
log(Prop. Industry)	5822	3.310	0.444	0.632	4.561
Democracy	5648	4.268	4.176	0	10
log(MEA)	7800	2.13	1.56	0	5.22

TABLE B1

Sensitivity of the results: Effect of ratification in year $t - k$ on emissions in year t

(Dependent Variable: *log(CO₂)*)	*k = 1*[a]	*k = 2*	*k = 3*	*k = 4*
$\log(CO_2)$ (t-1)	0.706***	0.706***	0.706***	0.707***
	(0.041)	(0.041)	(0.041)	(0.041)
LRTAP (t-k)	-0.024**	-0.021**	-0.018**	-0.020**
	(0.009)	(0.009)	(0.009)	(0.010)

Note: Robust standard errors in parentheses. Estimates of other covariates are not reported but are available upon request. *** p<0.01, ** p<0.05, * p<0.1.

[a] *k = 1* is our main result from column 5 in Table 6.

TABLE B2

Sensitivity of the results: Entry into force

(Dependent Variable: *log(CO₂)*)	*k = 0*	*k = 1*	*k = 2*
$\log(CO_2)$ (t-1)	0.705***	0.705***	0.706***
	(0.041)	(0.041)	(0.041)
Entry (t-k)	-0.023**	-0.020**	-0.021**
	(0.009)	(0.009)	(0.009)

Note: Robust standard errors in parentheses. Estimates of other covariates are not reported but are available upon request. *** p<0.01, ** p<0.05, * p<0.1.

TABLE B3

Sensitivity of the results: LRTAP Convention and EMEP

(Dependent Variable: *log(CO_2)*)	Convention	EMEP
log(CO_2) (t-1)	0.704***	0.706***
	(0.041)	(0.041)
Convention (t-1)	-0.057	
	(0.038)	
New LRTAP(t-1)		-0.026***
		(0.010)

Note: Robust standard errors in parentheses. Estimates of other covariates are not reported but are available upon request. *** p<0.01, ** p<0.05, * p<0.1.

TABLE B4

Sensitivity of the results: Kyoto Protocol.

(Dependent Variable: *log(CO_2)*)	1970 - 1997	1970-2002
log(CO_2) (t-1)	0.642***	0.682***
	(0.065)	(0.050)
LRTAP (t-1)	-0.085**	-0.051**
	(0.040)	(0.021)
Observations	2,488	3,161
Number of countries	143	143
Within R-squared	0.809	0.853

Note: Robust standard errors in parentheses. Estimates of other covariates are not reported but are available upon request. *** p<0.01, ** p<0.05, * p<0.1.

TABLE B5:

Sensitivity of the results: other controls

(Dependent Variable: *log(CO_2)*)	(1)	(2)	(3)	(4)
log(CO_2) (t-1)	0.702***	0.710***	0.705***	0.684***
	(0.042)	(0.039)	(0.056)	(0.045)
LRTAP (t-1)	-0.026***	-0.030***	-0.024***	-0.037***
	(0.009)	(0.009)	(0.008)	(0.046)
log(MEA) (t)	0.018			
	(0.034)			
log(GDP) (t)	0.286***	0.328***	0.273***	0.359***
	(0.055)	(0.061)	(0.061)	(0.073)
log(Population) (t)	0.029	-0.050	0.026	-0.118
	(0.104)	(0.087)	(0.101)	(0.081)
log(Openness) (t)	0.043			
	(0.029)			
log(Prop. Industry) (t)	0.071*	0.114***	0.144***	0.116***
	(0.043)	(0.032)	(0.041)	(0.035)
Democracy	0.005*	0.006*	0.003	0.005
	(0.003)	(0.003)	(0.004)	(0.004)
Prop. Gas (t)			-0.001	
			(0.001)	
log(FDI) (t)				-0.000
				(0.001)
Observations	4,168	4,195	3,262	3,782
Number of countries	150	150	121	150
Within R-squared	0.897	0.897	0.899	0.879

Note: Robust standard errors in parentheses. R-squared = squared correlation between the observed and predicted level of the dependent variable. *** p<0.01, ** p<0.05, * p<0.1.

TABLE B6

Sensitivity of the results: sub-samples of countries.

(Dependent Variable: *log(CO_2)*)	(1)	(2)	(3)	(4)
log(CO_2) (t-1)	0.708***	0.706***	0.700***	0.702***
	(0.039)	(0.041)	(0.042)	(0.044)
LRTAP (t-1)	-0.022**	-0.033**	-0.026***	-0.020**
	(0.009)	(0.013)	(0.010)	(0.009)
log(MEA) (t)	0.011	0.011	0.012	0.006
	(0.035)	(0.035)	(0.041)	(0.036)
log(GDP) (t)	0.262***	0.256***	0.275***	0.258***
	(0.042)	(0.043)	(0.049)	(0.046)
log(Population) (t)	0.051	0.031	0.047	0.074
	(0.126)	(0.128)	(0.128)	(0.130)
log(Openness) (t)	0.039	0.042	0.037	0.023
	(0.029)	(0.030)	(0.032)	(0.028)
GDP Growth Rate (t)	0.108	0.114	0.117	0.119
	(0.104)	(0.102)	(0.109)	(0.107)
log(Prop. Agriculture) (t)	0.009	0.003	0.001	0.002
	(0.038)	(0.039)	(0.040)	(0.040)
log(Prop. Industry) (t)	0.072	0.062	0.067	0.065
	(0.050)	(0.050)	(0.051)	(0.052)
Democracy	0.006**	0.006*	0.006*	0.007**
	(0.003)	(0.003)	(0.003)	(0.004)
Observations	3,966	3,917	3,822	3,455
Number of countries	145	145	129	129
Within R-squared	0.898	0.898	0.900	0.889

Note: Robust standard errors in parentheses. R-squared = squared correlation between the observed and predicted level of the dependent variable. *** p<0.01, ** p<0.05, * p<0.1. (1) = without biggest emitters; (2) = without most affected countries; (3) = no Economies in Transition; (4) = no deforestation.

Figures

Figure 1: CO_2 emissions (in tons per capita) and number of LRTAP treaties ratified in 1985, 1995 and 2005

Figure 2: Evolution of the world average death penalty index (1970-2010)

Figure 3: Effect of each individual agreement: Individual agreements' coefficients and 95% confidence intervals (i.e. from estimating equation (1) for each agreement)

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[1] This pattern is also true in other countries (see OECD 1991, p. 36).

[2] According to the Energy Information Administration (EIA 1999), the amounts of CO_2 produced for each billion Btu of heat energy extracted are: 208,000 pounds for coal, 164,000 pounds for petroleum products, and 117,000 pounds for natural gas.

[3] Rubin and Nguyen (2015) estimate that a scrubber total energy requirement varies between 2.5% and 6.1% of a gross power plant output (using coal as the main input).

[4] http://data.worldbank.org/data-catalog/world-development-indicators.

[5] Note that those data do not take into account CO_2 emissions/removals from land use, land use change and forestry, LULUCF (IEA 2010). We will try to control for this in the sensitivity analysis in Section V.

[6] A treaty is defined as "an intergovernmental document intended as legally binding with a primary stated purpose of preventing or managing human impacts on natural resources". A description of the database is given in Mitchell (2003).

[7] This procedure is used to adopt urgently needed amendments to international environmental agreements. The body that adopts this amendment at the same time fixes a specific time within which the parties will have to opportunity to notify either their acceptance or rejection or to remain silent. In case of silence the amendment is considered as accepted by the party.

[8] Due to our log specification, the coefficients would have remained unchanged by taking CO_2 emissions per capita instead of total CO_2 emissions as the dependent variable. The only exception would have been the coefficient of the control variable *Population*.

[9] By using a within analysis rather than a between analysis, we may underestimate the effect of treaties on CO_2 emissions. We also run a pooled regression (using some additional control variables) and find stronger results. However, since time invariant omitted variables that may affect the level of CO_2 emissions can be numerous, we prefer to concentrate on within variations in the rest of the paper.

[10] The Environmental Kuznets Curve (EKC) hypothesizes an inverse-U shaped relationship between a country's per capita income and its level of environmental quality (Galeotti et al. 2006; Friedl and Getzner 2003). We test the EKC hypothesis by assuming a quadratic functional form for GDP in our specification but the main results remain unchanged.

[11] The Polity IV dataset contains coded annual information on regime and authority characteristics for independent states with more than 500,000 people in 2014. It covers the period 1800-2014. See Marshall et al. (2011) for a more detailed description of the dataset.

[12] Note that we have already addressed the issue of environmental awareness by including a proxy of that variable, i.e. *logMEA*. However, *logMEA* is not a perfect measure of environmental awareness and there may exist other proxies. We also try to include additional control variables (e.g. foreign direct investments) in the sensitivity analysis.

[13] Amnesty International provides up-to-date information as to the status of the death penalty for 197 countries.

[14] We also check that the correlation coefficient between GDP and our index of death penalty is very low (e.g. 0.17).

[15] Due to the lack of variations through time and across countries, we were not able to use legalization of homosexual marriage or euthanasia as instruments.

[16] We also reject the null hypothesis of no variation through time within a country as the F-statistic is *F(38,5662) = 88.69* (with a p-value of 0.00).

[17] All the results in this section are obtained using Stata 13.

[18] Results for the control variables are very similar to those of models analyzed in the next section.

[19] Results are not reported here but are available upon request.

[20] We also recode the death penalty variable and generate a set of 4 dummy variables that we use as instruments. Results do not change.

[21] Note that this result cannot be explained by an eventual common positive trend (i.e. the fact that both *LRTAP* and *DP* increase monotonically) as we use the status of the death penalty in level to instrument *LRTAP* in first-difference.

[22] Even if the Cragg-Donald Wald F Statistics is much higher than the Kleibergen-Paap rank Wald F statistic, the use of the Kleibergen-Paap statistic is more appropriate. It generalizes the Cragg-Donald statistic to the case of non-i.i.d. errors, allowing for heteroskedasticity, autocorrelation and/or cluster robust statistics.

[23] However, lags 3 and 4 of log (CO_2) are not statistically significant in the first-stage equation.

[24] A potential weakness of the AB estimator (and thus also AH estimator) is that the lagged levels may be rather poor instruments for first differenced variables. This is especially the case if the dependent variable is close to a unit root, which seems not to be the case here since $\hat{\rho} = 0.706$ (see column 5). In the presence of poor instruments in level, one could use the augmented version – system GMM. The system GMM estimator (Blundell and Bond) uses the level equation (e.g. equation (2) in our case). The variables in levels in the second equation are instrumented with their own first differences. However, using this method in a panel with fixed effects requires a new assumption: the first-differenced variables used as

instruments for the variables in levels should not be correlated with the unobserved country effects α_i in equation (2). In our case, this would require, for example, that the first-differenced death penalty index or GDP are not correlated with the fixed-effects capturing unobserved heterogeneity among countries, which is too strong as an assumption. Moreover, as the first-stage regression and the Stock and Yogo's test show, our instruments are not weak.

[25] Given that the dynamic model seems to be the appropriate specification for the process underlying CO_2 emissions, the coefficient estimated in the dynamic model seems more reliable.

[26] These objective levels were computed using our data on CO_2 emissions: a target level of emissions in 2025 that is 26% lower than the level in 2005 for the US and a target level of emissions in 2030 40% lower than the level in 1990 for the EU.

[27] This effect was obtained by assuming constant BAU emissions between 2015 and 2040. If BAU emissions were increasing over that period, this effect would have been even bigger.

[28] Additionally, we have reduced our sample by limiting the number of years in two different ways: (i) we have only considered every five years to break any possible auto-correlation in the error term and (ii) we have only considered recent years (i.e. after 1980 and after 1985). Results are not presented here but they remain unchanged.

[29] The reason is that due to the setting of LRTAP treaties, ratification and entry into force coincide (almost to the year) for many countries in our sample.

[30] This is the 1979 LRTAP Convention under the auspices of which all subsequent protocols are negotiated.

[31] In our dataset, 28 countries ratified the Kyoto Protocol in 2002. Three countries ratified it before 2002: Cyprus (1999), Czech Republic and Romania (2001).

[32] The damaging effects of acidification in Europe were discovered in the 1960s through their effects on Scandinavian fish stocks. These countries were the first to suffer damages from

transbounday acid rains and they were the first to adopt control on SO_2 emissions (primarily to encourage others to follow rather than to benefit directly).

[33] The reduction of emissions observed in EiTs countries in the 1990s is mainly due to the economic collapse in those former Soviet States. It can then be argued that the success of air-pollution agreements in reducing CO_2 emissions is an artifact of those transition countries' industrial restructuring.