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Do collective actions clear common air? The effect of international environmental protocols on sulphur emissions

Arild Aakvik, Sigve Tjøtta *

Department of Economics, University of Bergen, Box 7802, 5020 Bergen, Norway

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ABSTRACT

This paper considers the effects of voluntary international environmental protocols on emissions, in particular the effect of the 1985 Helsinki Protocol and the 1994 Oslo Protocol on the reduction of sulphur oxides. The analysis employs panel data from 30 European countries over the period between 1960 and 2002. We divide all countries into 'signatories' and 'controls', *i.e.*, those that have signed and ratified a specific protocol and those that have not. Using a difference-in-difference panel data regression model, including yearly dummies and country-specific quadratic growth trajectories, we find no significant effect of either the Helsinki or the Oslo agreement in reducing sulphur emissions.

1. Introduction

A well-known example of collective action in response to environmental problems across national borders is the Kyoto Protocol on reduction of greenhouse gas emissions. The crossing of national jurisdictional borders adds a new dimension to Samuelson's (1954) general theory for public goods: under current international law, the imposition of obligations on sovereign states is possible only with consent; hence, multinational organisations and international agreements often have weak or no explicit control or enforcement mechanisms. Compliance with agreements is also often difficult to control and verify, and they seldom include explicit sanction mechanisms. It is thus reasonable to address the question of whether these protocols work. If they do not, we should rethink how we address these types of collective action problems.

The political economy literature on voluntary international agreements hold opposing views on their efficiency, see Downs (2000), Congleton (2001), Barrett (2003), Sandler (2004), and Ringquist and Kostadinova (2005) for excellent reviews of the literature. The game theoretical approach anchored in self-interested countries often concludes that international environmental agreements "tend to codify Nash behaviour and, as such, do not present much of a co-operative gain" (Arce et al., 2001, p. 494).[1] In

* Corresponding author. Tel.: +47 55 58 92 20; fax: +47 55 58 92 10.
E-mail address: sigve.tjotta@econ.uib.no (S. Tjøtta).

[1] See Mähler (1990), Hoel (1992), Barrett (1994, 2001, 2003), Carraro and Siniscalco (1993, 1998), Murdoch and Sandler (1997), Arce et al. (2001), and Finus and Tjøtta (2003).

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contrast, Lange and Vogt (2003) explain the large number of observed international agreements by arguing that these are driven by preferences for equity along with self-interest. An alternative approach is that international agreements affect national action through changing preferences. International agreements change preference through legitimization, role definition and reflection (Downs, 2000). Similarly, Douglass North argues that changing beliefs may induce change in institutions and affect actions and policies (North, 2005).

Congleton (1995, 2001) distinguish between symbolic, procedural, and substantive environmental agreements. Symbolic agreements declare good will to address a specific environmental problem, procedural agreements develop institutions where environmental targets are developed, and substantive agreements specify targets and regulations. These different types of agreements may also reflect different phases of international treaties.

In this paper, we evaluate two specific international environmental agreements: the 1985 Helsinki Protocol on the Reduction of Sulphur Emissions or Their Transboundary Fluxes by at Least 30 Percent, and the 1994 Oslo Protocol on Further Reduction of Sulphur Emissions. Both protocols are voluntary in signing and compliance and are substantive as they have specific targets. But as Congleton (2001) points out a substantive agreement has only an effect if domestic legislation is implemented to meet its requirement. Hence, both agreements share many characteristics with other organizations and agreements managing regional and global commons.

As far as we are aware, an econometric approach has not yet been used to evaluate the 1994 Oslo Protocol, though the 1985 Helsinki Protocol has undergone some quantitative assessment. Levy (1993), Murdoch et al. (1997), and Helm and Sprinz (2000) find that the 1985 Helsinki Protocol reduced sulphur emissions further than expected without the protocol; however, Ringquist and Kostadinova (2005) criticise these studies for their failure to take into account the fact that ratification of the Protocol is not random but a result of a self-selection decision process. They found that the 1985 Helsinki Protocol had no significant effect on sulphur emissions.[2]

To control for the self-selection process and for observed and unobserved factors, we use difference-in-difference estimations.[3] These focus on the difference in emissions before and after signing a specific protocol and compare that difference with results in non-signatory countries. When using panel data and difference-in-difference models, unobserved factors that may be the cause of selection bias, are differenced away. We identify the effects by repeated annual observations of emissions from signatories and controls before and after the signing date; we also employ a flexible panel data model that allows for country-specific fixed effects, country-specific quadratic growth trends, and year effects. These unobserved factors may capture factors such as country specific technology, regulation, ideology and economic conditions, and changes in these factors.

We expand on the econometric framework in Ringquist and Kostadinova (2005) by introducing both the 1985 Helsinki Protocol and the 1994 Oslo Protocol for reductions in sulphur emissions. We use a longer time span for analysis. In addition, we include country-specific linear and quadratic time trends in our econometric model, as well as fixed effects and year effects. This is a flexible way to control for unobserved heterogeneity when estimating the effect of the Helsinki and Oslo Protocols on sulphur emissions. We also look at relative changes in emissions rather than absolute levels in the difference-in-difference model. This is important in models with potential selection bias, because we allow for both the level and the yearly changes in emissions to differ for the different treatment groups prior to the signing period.

Our results support the findings in Ringquist and Kostadinova (2005) that the 1985 Helsinki Protocol had no significant effect. We also find that the 1995 Oslo Protocol had no effect. The lack of effects for both protocols is robust with respect to different specifications and robustness checks.

The paper is structured as follows: Section 2 provides some background information on international agreements in general and on sulphur reduction agreements in particular. Section 3 presents the data and some descriptive statistics. Section 4 outlines the econometric approach, emphasising the program evaluation principles. Section 5 details the main results and robustness checks; finally, Section 6 provides concluding remarks.

2. Background

2.1. International environmental agreements

Environmental problems across national borders require some form of international cooperation. The large number of multilateral agreements on environmental problems is evidence of the recognition of this challenge: see Barrett (2003) and Matisoff (2010) for overviews. This includes agreements on marine fisheries and pollution, international rivers, lakes and groundwater, conservation of species, and protection of pet animals. Some of these agreements are regional, such as the *Agreement Concerning the International Commission for the Protection of the Rhine against Pollution*, which came into force in 1965 between five countries; others are global, such as the *United Nations Framework Convention on Climate Change*, adopted in 1992, to which 189 countries are signatories.

Conflicts of interest and coordination problems characterise most multinational environmental problems. For example, each country meets a conflict of interest in that it has an incentive to a free ride on emission reductions from other countries; consequently, countries that would benefit from cooperation wind up in a prisoners' dilemma. Even without conflicts of interest,

[2] In a similar approach Bratberg et al. (2005) finds that the 1988 Sofia Protocol on nitrogen oxides has a significant effect.

[3] Heckman and Robb (1985), Heckman and Hotz (1989), and Moffitt (1991) provide overviews of econometric evaluation methods using panel data. They show how a difference-in-difference approach can be used to control for selection bias due to non-random treatment and control groups.

managing commons across state borders may be difficult to overcome due to problems with coordination. For example, a given government may find it difficult to contribute to the managing of commons without credible contributions from other governments.

Conflicts of interest and coordination problems may be partly resolved as governments engage in repeated intergovernmental relations in managing environmental commons and other relations, as with the integration and enlargement of the European Union (EU), the enlargement of the North Atlantic Treaty Organization (NATO), and international trade agreements. These relations may overcome the incentive to free ride. Using numerous examples, Ostrom (1990) argues that local communities handle such incentives by voluntary cooperation when supplying public goods. In the same manner, governments may manage to (partly) deal with incentives to free ride and with coordination problems.

2.2. The 1985 Helsinki and 1994 Oslo Protocols on the reduction of sulphur emissions

For many European countries, most deposits of sulphur and nitrogen oxides are airborne from other countries. Winds carry these air pollutants for hundreds, even thousands of kilometres before depositing them on soil and water. In turn, these pollutants have damaging effects on fish stocks and forests, and are associated with health threats to people due to reduced air quality. Clearly, nitrogen and sulphur oxide emissions in one country create environmental and health consequences in others.

To deal with these air pollution issues, a group of European and North American countries agreed upon the *1979 Geneva Convention on Long-range Transboundary Air Pollution.* At its 25th anniversary in 2004, the number of parties in the agreement was 49, including the E.U. Eight follow-up protocols extended the 1979 Geneva Convention by specifying abatement targets for these parties; the first of these was the *1984 Geneva Protocol on Long-term Financing of the Cooperative Program for Monitoring and Evaluation of the Long-range Transmission of Air Pollutants in Europe*. This protocol has three main components: (i) the collection of emission data for sulphur and nitrogen oxide and other air pollutants; (ii) the measurement of air and precipitation quality, and (iii) the modelling of atmospheric dispersion. The protocol came into force on 28 January 1988, and by June 2004 encompassed 41 parties.

There are three follow-up protocols regulating sulphur emissions: (i) the *1985 Helsinki Protocol on the Reduction of Sulphur Emissions or their Transboundary Fluxes by at least 30 Percent*, (ii) the *1994 Oslo Protocol on Further Reduction of Sulphur Emissions*, and (iii) the *1999 Gothenburg Protocol to Abate Acidification, Eutrophication and Ground-level Ozone*. According to Article 2 of the 1985 Helsinki Protocol, each signatory should reduce sulphur dioxide emissions by at least 30% compared with their 1980 level 'as soon as possible and at the latest by 1993'.[4] This came into force on September 2, 1987 and has 22 parties. The 1994 Oslo Protocol is a direct follow-up to the 1985 Helsinki Protocol, but participation is not restricted to countries participating in the initial sulphur protocol: for example, Greece, Ireland, Poland, and the United Kingdom did not sign the 1985 Helsinki Protocol but did sign the 1994 Oslo Protocol. The 1994 Oslo Protocol specifies different emission ceilings, expressed as annual emissions ceilings for 2000, 2005, and 2010. It took effect on August 5, 1995 with 25 parties. The 1999 Gothenburg Protocol sets emissions ceilings on sulphur, nitrous oxides, and volatile organic compounds as other major air pollutants responsible for the formation of ground-level ozone and ammonia.

3. Data and descriptive statistics

We use data from 30 European countries over the period between 1960 and 2002. All the countries in question signed and ratified the 1979 Geneva Convention, and are therefore invited to negotiate and sign follow-up protocols. A country is a 'signatory' of a protocol if it has *both* signed and ratified it. Ratification was in 1993 for the 1985 Helsinki Protocol and in 2002 for the 1994 Oslo Protocol. As shown in Table 1, 18 of the 30 countries signed the 1985 Helsinki Protocol, 19 signed the 1994 Oslo Protocol, 13 signed both, and six signed neither. The 1985 Helsinki Protocol was signed on July 9, 1985, and we assume that from 1986 to 1993 was the period of potential effect. The 1994 Oslo Protocol was signed on June 14, 1994, and we assume that the effect is from 1995 until 2002, the last year included in our sample.

The data on sulphur emissions are largely from Stern (2006), which in turn uses different sources and estimation methods: from 1980 and onwards, emission data are from European Monitoring and Evaluation Program (EMEP).[5] According to this program, each country reports emissions from different sectors. Sector estimates use inputs such as fuel as explanatory variables. For the period from 1960 to 1969, estimates result from either a frontier model or an environmental Kuznets model. The latter includes gross domestic product (GDP) as an explanatory variable. In the intermediate period of 1970 to 1979, estimates are from two sources: for OECD countries estimated emissions are constructed similar to the EMEP data, while for non-OECD countries estimates are either results from a frontier or a Kuznets model. A word of caution about emission data; they are crude estimates rather than collected from meters.

The population and GDP data are from the PENN World Tables Version 6.2 (Heston et al., 2006). We also include a control dummy variable for the former communist countries of Eastern Europe, thereby reflecting the transition process. We use 1991 as the first transition year, and indicate post-transition with a dummy variable. See the Appendix A for a more detailed description of the data.

GDP per capita is higher for signatories than for the controls. In 1985, the signatory year for the Helsinki Protocol, signatories' mean GDP per capita was $18,419 USD, compared to $10,895 for the controls. In 1994, the signatory year for the Oslo Protocol,

[4] See the protocol's website, http://www.unece.org/env/lrtap/status/lrtap_s.htm.
[5] The Cooperative Programme for Monitoring and Evaluation of the Long-Range Transmission of Air Pollutants in Europe (the EMEP program) monitors emissions as regulated by the 1979 Geneva Convention. Website: http://www.emep.int/.

Table 1
Signatory countries in the protocols and controls, from 1960–2002.

Helsinki & Oslo	Helsinki only	Oslo only	Neither
Austria	Bulgaria	Czech Republic, 1991–2002	Iceland
Belgium	Russia, 1991–2002	Greece	Poland
Denmark	USSR, 1960–1990	Ireland	Portugal
Finland	West Germany, 1960–1989	Slovakia, 1991–2002	Romania
France	East Germany, 1970–1989	Spain	Turkey
Hungary		United Kingdom	Czechoslovakia, 1960–1990
Italia			
Germany, 1990–2002			
Luxemburg			
Netherlands			
Norway			
Sweden			
Switzerland			

signatories' means are $20,296 USD per capita compared to $9821 for the controls. The controls use sulphur more intensively in their economies; sulphur emissions per GDP for controls are higher than signatories. In the respective signing years, the 1985 Helsinki Protocol signatories' mean as 2.7 k per USD GDP, compared to 4.2 k for the controls (compare with the 1994 Oslo Protocol's mean, 1.6 k for the signatories and 3.5 k for the controls).

The trend in sulphur emissions decreased for both groups after around 1980 (Fig. 1). Sulphur emissions increased for all countries from 1960 until at least 1975; from the beginning of the 1980s, when negotiations for the 1985 Helsinki Protocol began, most countries had already started to reduce sulphur emissions. For both protocols, the annual growth for the mean signatory country was below that of the mean control country during the protocol periods.[6] Moreover, though the picture for the pre-protocol countries is somewhat mixed, it appears as though annual growth for the mean signatory country was below that of the mean control country. In sum, the raw data appear to indicate that both protocols worked. For a more precise statement, we now turn to the econometric analyses.

4. Empirical modelling

Our goal is to estimate the causal effect of the signing of the environmental protocols on the reduction of sulphur emissions. Many unobserved variables potentially affect both political propensity to sign a protocol and willingness to reduce sulphur emissions. For example, environmental awareness on the part of the constituency may affect both the propensity to sign an international environmental agreement to reduce sulphur emissions and the implementation of policies toward reducing domestic sulphur emissions.[7] If there is a positive correlation between environmental awareness in the constituency and the signing of an international agreement, it will create an upwardly biased estimate of the effect when using standard regression models. We solve this problem of endogeneity by using a difference-in-difference approach.[8]

Difference-in-difference estimation focuses on the difference in emissions before and after signing a specific protocol and compares it with this difference for non-signatory countries. To model emissions, we start by defining the following variables:

Y_{it} the natural logarithm of sulphur emissions in country i for year t,
X_{it} a vector of covariates including GDP and population,
T_p years where protocol $p=1$ and 2 may have an effect,
τ_p the first year when protocol p may have an effect, and

$$D_{it}^{p} = \begin{cases} 1 \text{ if country } i \text{ signed protocol } p \text{ in year } t \in T_p \\ 0 \text{ otherwise} \end{cases}$$

The level of emissions for country i for year $t=1, 2, \ldots T$ is:

$$Y_{it} = \alpha_t + c_i + g_i t + \frac{1}{2} h_i t^2 + \delta^1 D_{it}^1 [t-(\tau_1-1)] + \delta^2 D_{it}^2 [t-(\tau_2-1)] + X_{it}\beta + v_{it}, \quad (1)$$

where α_t captures the year effects, parameters c_i, g_i, and h_i are country-specific quadratic trends, and v_{it} is the error term. Country-fixed effects control for unobserved influences on sulphur emissions that vary across countries. However, the factors that influence sulphur emissions may vary within a country over time, confounding the estimates of the country effects. We thus include country-specific linear and quadratic time trends in our model. This is a flexible way to control for unobserved heterogeneity when

[6] This drop in emissions around 1991 for the controls of the 1994 Oslo Protocol reflects data censorship: Russia, with emission of 2.6 Gg S, comes into the data set in 1991, whereas USSR, with emission of 10.9 Gg S, leaves it.
[7] Other unobserved factors may include technology, regulation, ideology and economic conditions.
[8] References and examples using the difference-in-difference approach can be found in Angrist and Krueger (1999), Besley and Case (2004), Rosenbaum (2002) and Lee (2005). For details on identification in difference-in-difference models, see Lee and Kang (2006).

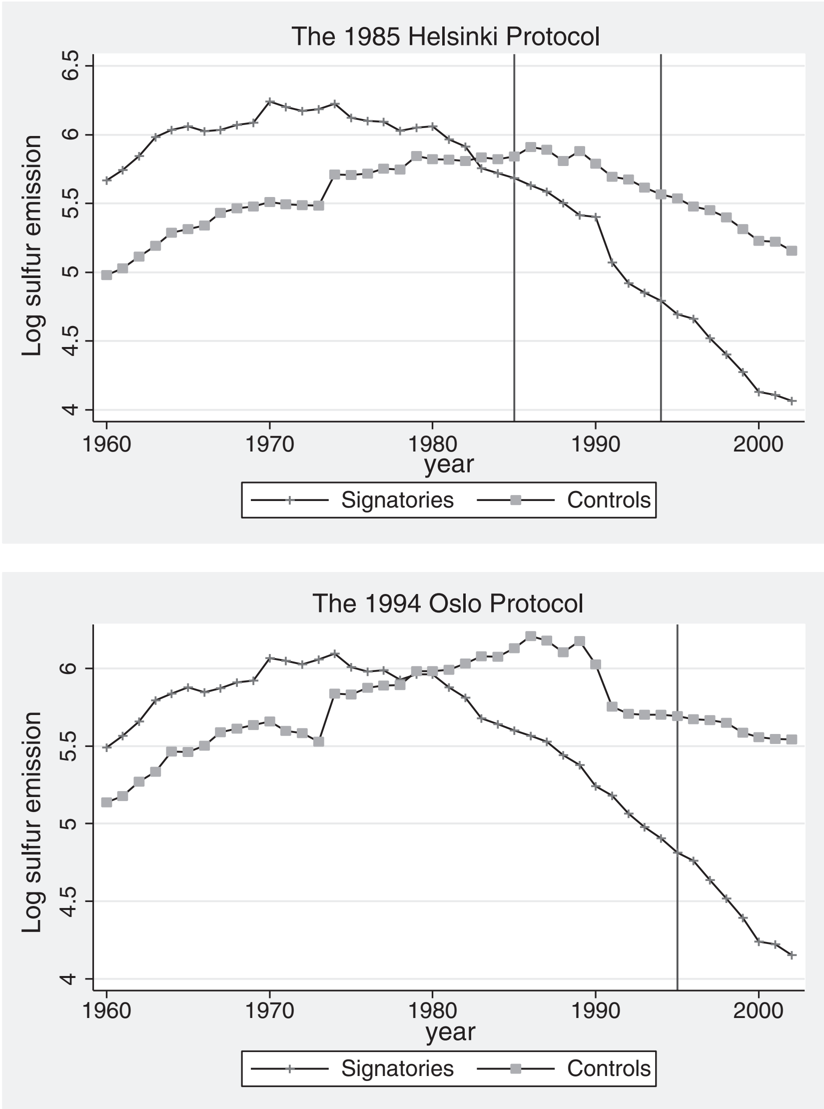

Fig. 1. Mean log sulphur emissions for signatories of the 1985 Helsinki and 1994 Oslo Protocols and their respective control groups. Vertical lines mark the period (start of period) of potential effect of protocols.

estimating the effect of the Helsinki and Oslo Protocols on sulphur emissions. We capture the annual effects of the two protocols with the parameters δ^1 and δ^2. By time differentiating Eq. (1), we obtain:

$$\Delta Y_{it} = \beta_t + g_i + h_i t + \delta^1 D_{it}^1 + \delta^2 D_{it}^2 + \Delta X_{it}\beta + \Delta v_{it}, \tag{2}$$

where $\beta_t = \alpha_t - \alpha_{t-1}$ and Δ is the time difference operator, for example $\Delta Y_{it} = Y_{it} - Y_{it-1}$.

The interpretation of the coefficients in the level (1) and differences (2) models is the same. However, autocorrelation in the error terms v_{it} may influence estimates of the standard errors of the coefficients in the two different models. In particular, the estimation of difference model (2) is preferred if the autocorrelation in the error terms v_{it} is high. For more details, see Lee and Kang (2006) and Wooldridge (2002). As we report in the next section, the autocorrelation is high; we therefore prefer difference model (2).

The treatment effect of protocol p at time $t \in T_p$, *i.e.*, δ^1 and δ^2, is interpreted as an annual effect. The identifying assumptions for δ^1 and δ^2 are as follows: (i) the change over time in sulphur emissions in the counterfactual situation is the same as the control group, *i.e.*, $E(\Delta_t Y_{it}^c | D_{it}^p = 1) = E(\Delta_t Y_{it} | D_{it}^p = 0)$; (ii) the pre-treatment relative change for the group of signatories is the same as it would have been without signing the protocol, *i.e.*, there is no adjustment in the pre-signing period. Hence, as an estimator for the annual effect for protocol p we utilise:

$$\delta^p = E\left(\Delta_t^2 Y_{it} | D_{it}^p = 1\right) - E\left(\Delta_t^2 Y_{it} | D_{it}^p = 0\right).$$

However, contrary to standard difference-in-difference models, we use this to analyse yearly changes in outcomes rather than emissions in absolute terms. In addition, we control for *country-specific growth effects*, along with country and year effects. These models are also better suited to causal inference than the standard difference-in-difference model, as we allow for a more dynamic specification of unobserved factors.

5. Results and robustness checks

5.1. Main results

Table 2 provides the results of the regression model in differentiated form in Eq. (2). Except for the participation and transition country dummies, the variables are logarithms and time-differentiated; we can therefore interpret their coefficients as elasticities. Our preferred specification of the model (Model III) includes controls for both year effects and country-specific quadratic growth effects. We also include the results from regressions with fixed and linear growth effects in Models I and II in Table 2. Note that in the differentiated model (2) we differentiate away the country-fixed effects α_i; it turns out that allowing for country-specific linear or quadratic growth effects in emissions is important because countries vary substantially in their emission trajectories. Adding linear and quadratic trend to the fixed effect model turns significant coefficients non-significant; this demonstrates that the usual fixed effects specification is not necessary robust.

Turning to the most important variables in the analysis, we see that with country-fixed effects (Model I in Table 2) the estimated annual effect of the 1985 Helsinki Protocol in the period from 1986 to 1994 is –0.062, corresponding to a 6.2% annual reduction in emissions. The estimated effect of the 1994 Oslo Protocol is a 9.3% annual reduction in emissions. The estimated robust standard errors are less than 0.02 for the Helsinki and Oslo estimates, and thus both are highly significant.

However, including the country-specific quadratic trend growth rates render the significant estimates insignificant. The protocols also yield a 1.5% yearly reduction for the Helsinki Protocol and a positive effect of 0.7% for the Oslo Protocol. The robust standard errors for these estimates are 0.023 and 0.035, respectively. Thus, the coefficient estimates are not significant at any conventional level using either a two-sided or a one-sided test.

Turning to the estimates of the covariates, Model III in Table 2 shows that a one-percent increase in gross domestic product increases annual emissions by 0.54%. The population variable is not significantly different from zero in our preferred model (Model III allowing for country-specific quadratic trends). In the former communist countries of Eastern Europe, the annual relative change in emissions is between −3% and −4% annually. However, this effect is not statistically different from zero in Model III. Our main results may then be sensitive to some aspects of the empirical formulation, and we perform several robustness checks.

5.2. Robustness checks

5.2.1. Level estimation, first-order auto regression

As remarked upon in the introduction, estimation of Eq. (1) by OLS is subject to possible serial correlation and may therefore involve biased estimated standard errors of the coefficients (see, for example, Wooldridge, 2002; Bertrand et al., 2004; Helland and Tabarrok, 2004). Clearly, as we model emissions using time differences (ΔY_{it}), potential serial correlation in the dependent variable is reduced compared with estimating a model using the level of emissions (Y_{it}) as the dependent variable.

To investigate serial correlation in the dependent variable, we estimate serial correlations from regressions on time differences of emissions ($\Delta_t Y_{it}$) after controlling for the covariates ($\Delta_t X_{it}$) from the model given by Eq. (2). We obtain the serial correlation

Table 2
Regression coefficients for relative changes in sulphur emissions, 1960–2002, difference-in-difference specification.

	Country-specific fixed effect model (I)	Country-specific linear trend model (II)	Country-specific quadratic trend model (III)
Helsinki 1986–1993	−0.062***	−0.031	−0.015
	(0.018)	(0.021)	(0.023)
Oslo 1995–2001	−0.093***	−0.040	0.007
	(0.016)	(0.025)	(0.035)
Gross domestic product	0.532***	0.446***	0.544***
	(0.174)	(0.188)	(0.138)
Population	1.012	−0.530	0.230
	(0.892)	(1.315)	(1.393)
Transition = 1 after 1990	−0.040**	−0.037	−0.006
	(0.016)	(0.023)	(0.046)
Year effects (α_t)	$F = 3.47$***	$F = 3.43$***	$F = 2.73$***
Country-specific growth effects:			
Linear term (g_i)	No	$F = 3.04$***	$F = 3.52$***
Quadratic term (h_i)	No	No	$F = 2.97$***
Constant	0.023	0.012	−0.040
	(0.024)	(0.027)	(0.024)
R^2	0.227	0.269	0.293
N	1008	1008	1008

Notes: Dependent variable is first-order difference in log emissions. Robust standard error is in parentheses. All independent variables except the dummy variables are first-order difference in logarithms. Significant level of two-tailed test at * 10%, ** 5%, and *** 1%.

coefficients using a regression of the residuals on the corresponding lagged residuals.[9] The estimated first-order serial correlation coefficients are small, at about −0.035, and not significantly different from zero.

The estimated serial correlation coefficients are considerably smaller than similar correlation coefficients obtained by regression on the corresponding level model usually employed in the literature, *i.e.*, regression of emission (Y_{it}) after controlling for covariates (X_{it}) and the year and country dummies. We estimate this model using Eq. (1). The estimated first-order coefficients are about 0.8 and strongly significant. This demonstrates that we can reduce problems with serial correlation related to the dependent variable using time differences in logarithmic emissions and covariates as variables instead of the levels of emissions and of covariates. According to theory, the standard error from the level model is much smaller than the standard error for the difference model.[10] Positive serial correlation in the error term will cause standard errors to be understated, exaggerated by serial correlation in the independent variables (see Helland and Tabarrok, 2004). The problem with serial correlation is much smaller for the difference model in Eq. (2) compared with the level model in Eq. (1).

An alternative for differentiating the model is to correct for the autocorrelation. Table 3 provides the results from estimating the level model with first-order correlation. As we can see, these results confirm the results of the difference model.

5.2.2. Time of signing, measurement errors, and Eastern European countries

By dropping data around the signing time, we avoid the problem of exactly when the countries commenced their emission reduction strategies. For the 1985 Helsinki Protocol, the first formal meeting was held between 7 and 10 June 1983, but even during the negotiation of the 1979 Geneva Convention (LRTAP9) sulphur reduction was on the agenda.[11] We therefore estimated Model III and skipped the data during the 1980–1987 period around the Helsinki Protocol and the 1992–1997 period around the signing of the 1994 Oslo Protocol. The results of these robustness checks do not change our main results.

The method of producing data on sulphur emissions varies between countries and over time. Estimates on sulphur emission in the periods from 1960 to 1969 and from 1970 to 1969 are based on estimates including GDP as an explanatory variable. Data sources and measurement methods for sulphur emissions vary in particular between the three periods of 1960–1969, 1970–1979, and 1980–2002. We therefore run robustness checks excluding the former two periods; the results do not change our conclusions. Estimation results are available upon request.

We also ran our preferred Model III in Table 2 with a sample excluding Eastern European countries; the result did not change. The estimated effects are −0.020 and 0.009 for the 1985 Helsinki Protocol and the 1994 Oslo Protocol, respectively. However, these are both insignificant, as the corresponding estimated standard errors are 0.026 and 0.052.

6. Concluding remarks

Our opening question was whether voluntary multinational environmental agreements succeed in attaining their aims. Based on our analysis of the 1985 Helsinki Protocol and the 1995 Oslo Protocol on sulphur emissions, our answer to this question is no, conditional on fundamental assumptions in our analysis.

We use panel data from 30 European countries over the period between 1960 and 2002. We divide these countries into 'signatories' and 'controls', *i.e.*, those that did and those that did not signify and ratify the specific protocol. Both groups indeed reduced emissions, at least in terms of sulphur emissions per capita and per dollar of GDP. Moreover, descriptive statistics indicate larger emission reductions among signatories. However, using a difference-in-difference estimator that focuses on differences in emissions before and after signing a protocol, and comparing this with the differences for controls, shows effects not significantly different from zero. These results are robust with respect to different model specifications, in particular robustness checks to cope with biased standard errors resulting from autocorrelation in the error term.

As we focus on emission reductions, we may omit any success accompanying other dimensions of the protocols in question. For example, the 1994 Oslo Protocol also focused on cost-efficient emission reduction and the damage caused by the pollutants, as the deposit of oxidised sulphur should not exceed specified critical loads. Our analysis may also have overlooked the technology diffusion effect of a specific protocol; if the sulphur protocols influenced signatory countries to develop new technologies to reduce their sulphur emissions, control countries may have adopted these same technologies and hence reduced their emissions. Our data on sulphur emissions and gross domestic product are surrogates of complex aspects of human activities, and may be poor surrogates. Finally, our study does not capture a broader perceptive of institutions and institutional changes. International agreements may alter belief, change institutional matrix and alter environmental actions beyond the specific sulphur protocols.

Our finding of no significant effect of international agreements could be a support for the view that international environmental agreements are a waste of time and that problems of long-range transboundary air pollution are impossible to solve. However, our results could also mean that we should place greater emphasis on the design of international environmental agreements. We believe that the success of international agreements depends on both participation and compliance incentives. A specific agreement can fail to incorporate all relevant countries; depending on the group of participating countries, it can also fail to implement efficient emissions policies. International treaties are probably necessary, but are not currently sufficient for success. Thus, there is room for improvement when it comes to their design and implementation, particularly regarding incentive structures and the use of the price mechanism.

[9] The regression results are available upon request.
[10] Results not reported here.
[11] According to Sliggers and Kakebeeke (2004, Chapter 2).

Table 3
Regression coefficients for relative changes in sulphur emissions, from 1960–2002, level specification correction for first-order autocorrelation.

	Country-specific fixed effect model (I)	Country-specific linear trend model (II)	Country-specific quadratic trend model (III)
Helsinki 1986–1993	−0.005	−0.006	−0.008
	(0.006)	(0.006)	(0.006)
Oslo 1995–2001	−0.072***	−0.027	0.025
	(0.0215)	(0.021)	(0.027)
Gross domestic product	0.410***	0.515***	0.660***
	(0.013)	(0.130)	(0.136)
Population	1.807	−1.277	−1.947*
	(1.121)	(1.156)	(1.153)
Transition = 1 after 1990	−0.011	−0.039	0.012
	(0.023)	(0.024)	(0.041)
Year effects (α_t)	Yes	Yes	Yes
Country-specific growth effects:			
Fixed effect	Yes	Yes	Yes
Linear term (g_i)	No	Yes	Yes
Quadratic term (h_i)	No	No	Yes
Constant	−20.25***	7.894***	11,834***
	(0.425)	(1.867)	(2.837)
First-order autocorrelation	0.96	0.83	0.74
N	1038	1038	1038

Notes: Dependent variable is log emissions. Robust standard error is in parentheses. All independent variables except dummy variables are in logarithms. Significant level of two-tailed test at * 10%, ** 5%, and *** 1%.

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Appendix A. Data description

A.1. Countries

All countries signed and ratified the *1979 Geneva Convention on Long-range Trans-boundary Air Pollution.* Since the 1979 Geneva Convention, the political map of Europe has changed: before the reunification of Germany in 1990, we use West Germany 1960–1989 and East Germany 1970–1989. For reunified Germany, we use data from the 1990–2002 period. On 1 January 1993, Czechoslovakia underwent a 'velvet divorce' into its two national components, the Czech Republic and Slovakia. The two new states inherited the former state's responsibilities relating to the 1978 Geneva Convention and its follow-up protocols. We use 1960–1990 data for Czechoslovakia and 1991–2002 for the Czech Republic and Slovakia. In December 1991, the USSR splintered into Russia and 14 other independent republics; we use data for the USSR from 1960 to 1990 and for Russia from 1991 to 2002.

A.2. Signatories of the protocols

We define a country as a signatory of the *1985 Helsinki Protocol on the Reduction of Sulphur Emissions or their Transboundary Fluxes by at least 30 percent* if the country signed and ratified the protocol in 1993. Both the Czech Republic and Slovakia ratified the protocol in 1993, even though Czechoslovakia did not sign. Here we classify Czechoslovakia and the Czech Republic and Slovakia as controls of the 1985 Helsinki Protocol. We define a country as a signatory of the *1994 Oslo Protocol on Further Reduction of Sulphur Emissions* if it signed and ratified the protocol in 2002.

Sulphur emissions data are from Stern (2006). Data for 1980–2001 are available from the EMEP website (www.emep.in). Additional data for 1970–1980 are from earlier OECD statistics from Denmark, Finland, France, Ireland, Italy, the Netherlands, Norway, Portugal, Spain, Sweden, Switzerland, and the United Kingdom. Stern also estimates data from other sources for 1960–1970 and for non-OECD countries in the period from 1960 to 1980.

For 2002, we supplement Stern with data from EMEP (www.emep.in). For Russia from 1990 to 2002, we use EMEP expert emissions predictions to approximate the missing years, 1991–1994.

Population data are from PENN World Table Version 6.2 (Heston et al., 2006, variable population). For East and West Germany, the USSR, and Czechoslovakia during this period, we use PENN World Table Version 5.6.

Gross domestic product is also from PENN World Table Version 6.2 Heston et al. (2006). We use real GDP per capita in 1996 dollars (variable RGDPCH[16].) For Bulgaria from 1980 to 1990, Czechoslovakia from 1960 to 1990, East Germany from 1970 to 1989, West Germany from 1950 to 1992, and the USSR from 1960 to 1990, we use GDP per capita from PENN World Table Version

5.6. To obtain consistent data for the GDP per capita figures, we regress GDP per capita from PENN World Table 6.2 on GDP per capita PENN World Table Version 5.6 after controlling for countries and years, and use the predicted GDP per capita in 1996 dollars.

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