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A panel data analysis of the long-run effect of environmental taxes on R&D expenditures at the macro-level

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Abstract

Whether and how environmental taxes affect R&D at the macro-level is an empirical question that has not been addressed in the literature. This paper fills this gap by examining the impact of environmental taxes on R&D expenditures using panel data for the period 1994-2021 from 49 countries. The main results of this study are as follows: (i) environmental taxes have, on average, a positive long-run effect on R&D expenditures; (ii) the direction of causality runs from environmental taxes to R&D and not from R&D to environmental taxes; and (iii) while the long-run effect of environmental taxes varies across countries, it is positive in almost all cases, suggesting that the average positive long-run effect of environmental taxes on R&D is not driven by a few countries. We also find some evidence of a positive effect of environmental taxes on both environmental and non-environmental R&D, based on a smaller sample of countries over a shorter time period.

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1. Introduction

Securing both economic growth and environmental sustainability stands as one of the most critical global challenges of today and the future. As evidenced by numerous studies (see, e.g., Blanco et al., 2016; Minniti and Venturini, 2017; Herzer, 2022), research and development (R&D) drives technological progress and is therefore a crucial pillar for long-term economic growth. In addition, R&D may play a pivotal role in advancing environmental sustainability, either directly through the development of cleaner technologies or indirectly by generating knowledge that can be utilized in the development of green technologies.

By imposing taxes on activities with negative environmental impacts, governments worldwide seek not only to encourage the adoption of environmentally friendly practices, but also to stimulate R&D in green technologies. The simple logic is that firms can avoid taxes by reducing their harmful environmental impacts either through installing existing pollution abatement technologies or developing new green technologies.

However, even if environmental taxes provide incentives to reduce harmful environmental impacts by developing new technologies, this does not necessarily imply that environmental taxes lead to an increase in overall R&D activity. Since environmental taxes impose costs on producers, such taxes may reduce the resources available for non-environmental R&D. Thus, non-environmental R&D activity may decline due to environmental taxes, which may compensate the increase in environmental R&D activity. If many firms choose to acquire existing environmental technologies instead of investing in uncertain environmental R&D, environmental taxes may also have a negative net effect on overall R&D in such a scenario.

Alternatively, environmental taxes also have the potential to sustain or even increase the resources allocated to non-environmental R&D if tax revenues are recycled to reduce corporate taxes or other non-environmentally related taxes that have negative effects on R&D activity. Thus, environmental taxes may have a large positive effect on overall R&D, including the R&D of firms not subject to environmental taxes, not only by providing incentives for the development of environmentally-friendly technologies but also by increasing the availability of resources for R&D (including non-environmental R&D) through the reduction of corporate taxes or other non-environmentally related taxes. Furthermore, environmental taxes may have an economically significant positive effect on overall R&D if their revenues are instead used to finance public R&D expenditures or R&D subsidies.

It is thus an empirical question whether and how environmental taxes affect overall R&D activity, including that of firms not taxed for environmental reasons, as well as that of the public sector. Surprisingly, however, there are no studies on this macro question, and related studies at the firm- or industry-level are very scarce. More specifically, we identified only two such related studies.

Liu et al. (2023) find, in Chinese firm-level data, that environmental taxes reduce the overall R&D expenditures of firms that are taxed. However, their study, by its nature, is unable to capture the effect of environmental taxes on the R&D expenditures of all firms, including those that are not taxed for environmental reasons but possibly benefit from environmental taxes through revenue recycling to reduce other, non-environmental taxes or through R&D subsidies. In addition, their study cannot capture the potential indirect effect of environmental taxes on public R&D expenditures.

Costa-Campi et al. (2017) observe in their industry-level study for Spain not only a significant positive impact of environmental taxes on environmental R&D expenditures in the manufacturing sector, but also a positive but insignificant effect of environmental taxes on non-environmental R&D expenditures. Thus, although Costa-Campi et al. (2017) do not explicitly examine the impact of environmental taxes on overall R&D expenditures of all firms, their findings suggest that this impact is positive, at least for Spain. However, the study by Costa-

Campi et al. (2017) is inherently unable to capture the potential effects of environmental taxes when the government uses the revenues from these taxes to finance governmental R&D.

In addition, the studies by Liu et al. (2023) and Costa-Campi et al. (2017) are case studies for individual countries, and their results may not necessarily be generalized to a broader cross-section of countries.

Given the lack of studies on the effect of environmental taxes on overall R&D, the aim of this paper is to fill this gap. Our study is novel in several respects. First, it examines the impact of environmental taxes on R&D expenditures at the macro-level. Second, it explores this macro impact across a cross-section of (49) countries, including high-, middle-, and low-income countries. Third, it employs recently developed heterogeneous panel data techniques. These techniques allow us to investigate both the average long-run effect of environmental taxes on R&D across countries and the potential cross-country heterogeneity in this effect. Fourth, it employs causality tests to evaluate the direction of causality in the long-run relationship between environmental taxes and R&D expenditures. Fifth, this study presents some preliminary results on the impact of environmental taxes on both environmental and non-environmental R&D at the macro level across countries, though these findings are based on a smaller sample of (16) countries.

2. Basic model and methodology

The basic model we use in our main analysis is a cross-sectionally augmented autoregressive distributed lag (CSARDL) model developed by Chudik and Pesaran (2015). In our application, it is represented in error correction form by the equation

$$\begin{aligned}\Delta\log R\&D_{it} = c_i + b_{1i}\log R\&D_{it\text{-}1} + b_{2i}\log TAXES_{it\text{-}1} + b_{3i}\Delta\log TAXES_{it} \\ +\phi_{1i}\overline{\log R\&D_{t\text{-}1}} + \phi_{2i}\overline{\log TAXES_{t\text{-}1}} \\ + \sum_{j=0}^{p-1}\phi_{3ij}\Delta\overline{\log R\&D_{t\text{-}j}} + \sum_{j=0}^{p-1}\phi_{4ij}\Delta\overline{\log TAXES_{t\text{-}j}} + \varepsilon_{it}\end{aligned} \tag{1}$$

where $\log R\&D_{it}$ is the natural logarithm of real R&D expenditures of country i in year t, while $\log TAXES_{it}$ denotes the natural logarithm of real revenues from environmental taxes for the same countries and years. The variables $\overline{\log R\&D_{t\text{-}1}} = N^{-1}\sum_i^N \log R\&D_{it\text{-}1}$ and $\overline{\log TAXES_{t\text{-}1}} = N^{-1}\sum_i^N \log TAXES_{it\text{-}1}$ represent the cross-sectional averages of $\log R\&D_{it\text{-}1}$ and $\log TAXES_{it\text{-}1}$, respectively.[1]

The CSARDL estimator belongs to the class of so-called common correlated effects (CCE) estimators, which use cross-sectional averages as proxies for unobserved time-varying common factors. Failure to control for such factors can induce cross-sectional dependence in the residuals, ε_{it}, and thereby lead to inconsistent estimates if the common factors are correlated with both the dependent variable and the explanatory variable(s). Since all coefficients in equation (1) are country specific, the effects of the unobserved common factors are allowed to vary across countries.

[1] If the number of lags of the cross-sectional averages in the original levels CSARDL model is denoted as p, then the number of lags of the first-differences of the cross-sectional averages in the reformulated CSARDL model in error correction form is $p - 1$. Chudik and Pesaran (2015) recommend setting the number of lags of the cross-sectional averages in the original CSARDL model equal to the integer part of $T^{1/3}$, which is equivalent to recommending setting $p - 1$ in the error correction form of the CSARDL model equal to the integer part of $T^{1/3}$. Given that the average number of observations per country in our sample is $\overline{T} = 25$, we estimate equation (1) with $p - 1 = 1$ lag.

While the CCE approach controls for more than one common factor and permits the individual responses to the common factors to differ across countries, the common practice of employing time dummies (or demeaned data) implicitly assumes a single common factor and an equal effect of the common factor across all countries. Due to these assumptions, time dummies (or demeaned data) are generally less effective in removing error cross-sectional dependence compared to the CCE approach.

However, the CCE procedure does not guarantee error cross-sectional independence. Therefore, to test whether our models are free from error cross-sectional dependence stemming from unobserved common factors, we use the cross-sectional dependence test proposed by Juodis and Reese (2021).[2]

The c_i are country-specific constants; the coefficient b_{3i} captures the short-run, one-period effect of environmental taxes on R&D; and the coefficient b_{1i} is the so-called error correction coefficient, which quantifies the speed with which $\log R\&D_{it}$ adjusts towards its long-run level following a change in $\log TAXES_{it}$. This coefficient should be negative and statistically significant if $\log TAXES_{it}$ has a long-run effect on $\log R\&D_{it}$. Finally, the ratio ($b_{2i}/|b_{1i}|$) represents the long-run coefficient on $\log TAXES_{it}$.

Since equation (1) is estimated using the mean group estimator developed by Pesaran and Smith (1995), it represents a CSARDL mean group (CSARDLMG) regression. Such a mean group regression involves estimating separate regressions for each country and then averaging the individual country coefficients. Thus, the *average long-run coefficient* on $\log TAXES_{it}$ represents the mean of the individual or country-specific long-run coefficients on $\log TAXES_{it}$, and the individual or country-specific long-run coefficients on $\log TAXES_{it}$ are the individual or country-specific ratios of the country-specific coefficients on $\log TAXES_{it\text{-}1}$ to the absolute values of the country-specific coefficients on $\log R\&D_{it\text{-}1}$. Alternatively, the, *long-run average coefficient* on $\log TAXES_{it}$ can be computed as the ratio of the average of the country-specific coefficients on $\log TAXES_{it\text{-}1}$ to the absolute value of the average of the country-specific coefficients on $\log R\&D_{it\text{-}1}$.

While the mean group estimator allows the slope coefficients to vary across countries, it is well known that efficiency gains can be achieved through the use of pooled estimators when the individual slope coefficients are homogeneous. Therefore, to assess whether the mean group estimator is the correct choice, or whether we should use a pooled version of the CSARDL estimator, we explicitly test whether the slope coefficients are heterogeneous using the test for slope homogeneity developed by Blomquist and Westerlund (2013).

In this context it is important to mention a potential issue when estimating panel data models with a long time-series dimension, as in the present application: If the underlying variables exhibit stochastic trends or are unit root non-stationary, then: (*i*) residual non-stationarity—and thus the absence of cointegration between the variables—can lead to spurious regressions (see, e.g., Kao, 1999), and (*ii*) inference in non-stationary panel models can be misleading even when the variables are cointegrated (see, e.g., Kao and Chiang, 2000).

However, while Chudik and Pesaran (2015) prove the consistency of the CSARDLMG estimator under the assumption of stationarity, Kapetanios et al. (2011) demonstrate theoretically and experimentally that the static mean group and pooled CCE estimators remain consistent and yield correctly sized tests even when the variables are $I(1)$, provided that they cointegrate. In addition, Pesaran and Shin (1999) show that if the underlying variables are $I(1)$ and cointegrated, the conventional time series ARDL estimator yields (super) consistent estimates of the long-run coefficients and that valid inferences on the long-run parameters can be drawn using standard asymptotic theory. In the case of non-stationary variables, it can therefore be assumed that inference based on the CSARDLMG estimator remains valid,

[2] We use the Juodis and Reese (2021) test instead of the standard Pesaran (2004) test due to the latter's lack of power in detecting error cross-sectional dependence when the estimated models involve cross-sectional averages. The Juodis and Reese (2021) test, a modified version of the Pesaran (2004) test, eliminates this limitation.

provided that the variables are cointegrated. Thus, before applying the CSARDLMG estimator, it is important to investigate the integration and cointegration properties of the variables.

To check the robustness of the results from the CSARDLMG estimator, we employ the CCE mean group (CCEMG) estimator of Pesaran (2006) and the cross-sectionally augmented distributed lag mean group (CSDLMG) estimator of Chudik et al. (2016). The CCEMG estimator is static and, in our case, relies on a mean group regression of $\log R\&D_{it}$ on $\log TAXES_{it}$ as well as the cross-sectional averages of both variables. The CSDLMG estimator is dynamic and, in our case, is based on a mean group regression of $\log R\&D_{it}$ on $\log TAXES_{it}$ and one lag of the first differences of $\log TAXES_{it}$, as well as cross-sectional averages of $\log R\&D_{it}$ and $\log TAXES_{it}$, and two lags of the cross-sectional averages of $\log TAXES_{it}$.[3] Both estimators estimate the average long-run coefficient on $\log TAXES_{it}$ directly. The disadvantage of both these estimators compared to the CSARDLMG estimator is that they require the regressors to be strictly exogenous, whereas the CSARDLMG estimator is valid in the case of weakly exogenous regressors (and thus in the case of short-run feedback effects between the variables). In addition, the error correction model given by equation (1) has the advantage that it allows us to test whether $\log R\&D_{it}$ is not weakly exogenous to $\log TAXES_{it}$ — by estimating the error correction coefficient b_{1i}, which should be both negative and statistically significant if $\log R\&D_{it}$ is endogenous.[4]

It is well known that under cointegration, endogeneity does not lead to inconsistent parameter estimates. It is also well known that the existence of cointegration means the presence of long-run causality in at least one direction (Granger, 1988). However, while a significant error correction coefficient in equation (1) can be interpreted as an indication that long-run causality runs from $\log TAXES_{it}$ to $\log R\&D_{it}$, it cannot be ruled out that long-run causality also runs from $\log R\&D_{it}$ to $\log TAXES_{it}$, for example, if R&D efforts contribute to output growth, and the resulting increase in output leads to higher pollution emissions that are taxed.

If there is long-run causality from $\log R\&D_{it}$ to $\log TAXES_{it}$, one may be skeptical about whether a positively estimated long-run coefficient on $\log TAXES_{it}$ indeed captures a positive effect of environmental taxes on R&D expenditures, or if it instead reflects a positive effect of R&D expenditures on revenues from environmental taxes. Therefore, we carefully examine the direction of causality between the two variables — after estimating the long-run coefficient on $\log TAXES_{it}$. Finally, we provide the estimated long-run coefficients on $\log TAXES_{it}$ for each country in our sample. This allows us to investigate whether our finding for the average long-run effect of environmental taxes on R&D is due to a limited number of countries with large positive or negative effects. Although this is not the focus of the study, we also present some preliminary results on the impact of environmental taxes on both environmental and non-environmental R&D in a separate section.

3. Data

To construct our measure of real R&D expenditures, we use gross expenditures on R&D as a percentage of GDP from the UNESCO database, available at http://data.uis.unesco.org, and from the OECD Main Science and Technology Indicators database, available at https://data-explorer.oecd.org/vis?df[ds]=DisseminateFinalDMZ&df[id]=DSD_MSTI%40DF_MSTI&df[ag]=OECD.STI.STP&df[vs]=1.3&dq=.A.G%2BT_RS...&lom=LASTNPERIODS&lo=5&to[TIME_PERIOD]=false, and multiply this percentage ratio by real GDP at PPP from the World Development Indicators (WDI), accessible at https://databank.worldbank.org/source/world-development-indicators.

[3] This specification is based on the recommendation of Chudik and Pesaran (2015) and Chudik et al. (2016), who suggest setting the number of lags of the cross-sectional averages equal to the integer part of $T^{1/3}$.

[4] A potential disadvantage of the CSARDLMG estimator is that it may suffer from the well-known small T time series bias in dynamic panel data models. To mitigate this bias, we apply the recursive mean adjustment method.

Similarly, we construct our measure of real revenues from environmental taxes by multiplying the WDI's GDP in constant PPP dollars by environmentally related tax revenues as a percentage of GDP from the OECD Environmental Statistics, available at https://stats.oecd.org/Index.aspx?DataSetCode=EPS#. Since the data on environmentally related tax revenues (as a percentage of GDP) are available between 1994 and 2021, our analysis covers this period.

Data on environmentally related tax revenues and real R&D expenditures (as a percentage of GDP) are not available for many countries for all years between 1994 and 2021. Since our analysis focuses on the long-run effect of environmental taxes on R&D, requiring sufficient time to develop, we include only those countries with complete time series data on $\log R\&D_{it}$ and $\log TAXES_{it}$ spanning at least 15 years within this period. This results in an unbalanced panel of 49 countries (listed in Table 5) with an average of 25 observations per country.[5]

4. Results

4.1. Estimates of the long-run relationship between revenues from environmental taxes and R&D expenditures for our entire panel of 49 countries

The results of panel unit root and panel cointegration tests, reported in Table A1 and Table A2 in the appendix, indicate that $\log R\&D_{it}$ and $\log TAXES_{it}$ are $I(1)$ and cointegrated. Thus, the CSARDLMG estimator is applicable here.

Column (1) of Table 1 reports both the average long-run coefficient on $\log TAXES_{it}$ and the long-run average coefficient on $\log TAXES_{it}$, along with the coefficient on $\log R\&D_{it\text{-}1}$, from the CSARDLMG regression given by equation (1). Column (1) of the table also reports the result of the test for slope homogeneity developed by Blomquist and Westerlund (2013), denoted by SL, as well as the result of the cross-sectional dependence test developed by Juodis and Reese (2022), denoted by CD, applied to the residuals from the regression. The slope homogeneity test suggests that the individual slope coefficients are heterogeneous, justifying the use of the mean group version of the CSARDL estimator, and the cross-sectional dependence test indicates no sign of error cross-sectional dependence due to omitted common factors.

Turning to the estimated coefficients we find evidence that environmental taxes exert a long-run causal effect on R&D expenditures, as indicated by the negative and statistically significant coefficient on $\log R\&D_{it\text{-}1}$. In addition, both the average long-run coefficient on $\log TAXES_{it}$ and the long-run average coefficient on $\log TAXES_{it}$ are positive and statistically significant.

In columns (2) and (3) of Table 1, we use the CCEMG estimator and the CSDLMG estimator. The estimates of the average long-run coefficient on $\log TAXES_{it}$ from these estimators are qualitatively and quantitatively very similar to the corresponding estimate in column (1). For completeness, it should however be noted that the Juodis and Reese (2022) test indicates the presence of cross-sectional dependence due to unobserved common factors in the residuals from the CCEMG regression in column (2).

[5] Three points should be mentioned here. First, for some countries, we filled small data gaps by log-linear interpolation. Second, we excluded Mexico due to the nature of its environmental tax revenue data, which, unlike the data for other countries, are net of subsidies and therefore sometimes negative. Third, our sample includes all countries for which data are reported except five countries with very few observations (Costa Rica, Pakistan, the Philippines, Trinidad and Tobago, and Vietnam).

Table 1. Estimates of the long-run relationship between environmental taxes and R&D

	(1) CSARDLMG	(2) CCEMG	(3) CSDLMG
log$R\&D_{it-1}$	-0.296***		
	(0.035)		
Average long-run coefficient on log$TAXES_{it}$	0.975***	0.930**	0.908***
	(0.073)	(0.418)	(0.166)
Long-run average coefficient on log$TAXES_{it}$	1.027***		
	(0.065)		
SL (p-value)	0.000	0.000	0.000
CD (p-value)	0.325	0.000	0.499
Number of observations	1079	1226	1128

Notes: CSARDLMG = cross-sectionally augmented autoregressive distributed lag mean group estimator (in error correction form) developed by Chudik and Pesaran (2015); CSDLMG = cross-sectionally augmented distributed lag mean group estimator of Chudik et al. (2016); CCEMG = common correlated effects mean group estimator of Pesaran (2006). The dependent variable in the CSARDLMG regression is Δlog$R\&D_{it}$. The dependent variable in the CSDLMG and CCEMG regressions is log$R\&D_{it}$. The average long-run coefficient on log$TAXES_{it}$ is the average of the individual or country-specific long-run coefficients on log$TAXES_{it}$. The individual or country-specific long-run coefficients on log$TAXES_{it}$ are the individual or country-specific ratios of the country-specific coefficients on log$TAXES_{it-1}$ to the absolute values of the country-specific coefficients on log$R\&D_{it-1}$ (computed using the delta method). The long-run average coefficient on log$TAXES_{it}$ represents the ratio of the average of the country-specific coefficients on log$TAXES_{it-1}$ to the absolute value of the average of the country-specific coefficients on log$R\&D_{it-1}$ (computed using the delta method). We applied the recursive mean adjustment method to mitigate the small sample time series bias in the CSARDLMG estimation. All regressions control for country fixed effects. The CSARDLMG, CCEMG, and CSDLMG estimators control for error cross-sectional dependence due to unobserved common factors via the use of (weighted) cross-sectional averages. While the CSARDLMG results are derived from a specification without lags of the first differences of log$TAXES_{it}$ and log$R\&D_{it}$, and include one lag of the cross-sectional averages of the first differences of log$TAXES_{it}$ and log$R\&D_{it}$, the CSDLMG results are based on a specification with one lag of the first differences of log$TAXES_{it}$ and two lags of the cross-sectional averages of log$TAXES_{it}$ (following the suggestion of Chudik and Pesaran (2015) and Chudik et al. (2016), who recommend setting the number of lags of the cross-sectional averages equal to the integer part of $T^{1/3}$). SL denotes the test for slope homogeneity developed by Blomquist and Westerlund (2013). CD denotes the cross-sectional dependence test of Juodis and Reese (2022), applied to the residuals from the regressions. Standard errors are in parentheses. The CSARDLMG, CCEMG, and CSDLMG standard errors are robust not only to heteroscedasticity and autocorrelation but also to general forms of spatial dependence. *** (**) indicates significance at the 1% (5%) level.

4.2. Causality tests

To investigate the direction of causality in the long-run relationship between log$TAXES_{it}$ and log$R\&D_{it}$, we employ the two-step procedure suggested by Canning and Pedroni (2008). In the first step, we use the individual country estimates of the long-run coefficients to construct an error correction term. In the second step, we include this term lagged one period in a panel vector error correction model. If the coefficient α_1 of the lagged error correction term in the Δlog$R\&D_{it}$ equation of the model is significantly different from zero and the coefficient α_2 of the lagged error correction term in the Δlog$TAXES_{it}$ equation is not significantly different from zero, then long-run causality runs from log$TAXES_{it}$ to log$R\&D_{it}$. If α_2 is not significantly different from zero and α_1 is significantly different from zero, then long-run causality runs from log$R\&D_{it}$ to log$TAXES_{it}$. If both coefficients are significantly different from zero, then long-run causality runs in both directions. Following Eberhardt and Teal (2013), we augment the error correction equations with cross-sectional averages of both the dependent and independent variables as additional regressors to control for error cross-sectional dependence, thus following the CCE approach. The results of the tests for long-run causality between log$TAXES_{it}$ and log$R\&D_{it}$ from the Canning and Pedroni (2008) procedure are presented in Panel A of Table 2. They suggest that long-run causality is unidirectional from environmental tax revenues to R&D expenditures.

As an additional test for causality between log$TAXES_{it}$ and log$R\&D_{it}$, we use the Granger causality test developed by Dumitrescu and Hurlin (2012), which involves applying the standard VAR-based Granger causality test to each country separately and then averaging the individual Wald statistics. The bootstrap approach of Dumitrescu and Hurlin (2012) is used to calculate p-values that account for error cross-sectional dependence. The results are presented in Panel B of Table 2. Similar to the results in Panel A, they suggest that the direction of causality runs from $\log TAXES_{it}$ to $\log R\&D_{it}$ but not from $\log R\&D_{it}$ to $\log TAXES_{it}$.

Table 2. Causality tests

	Wald tests for causality (p-values)
Panel A: Canning and Pedroni (2008) approach	
H_0: $\log TAXES_{it}$ does not long-run cause $\log R\&D_{it}$	0.000
H_0: $\log R\&D_{it}$ does not long-run cause $\log TAXES_{it}$	0.362
Panel B: Dumitrescu and Hurlin (2012) approach	
H_0: $\log TAXES_{it}$ does not cause $\log R\&D_{it}$	0.018
H_0: $\log R\&D_{it}$ does not cause $\log TAXES_{it}$	0.405

Notes: The results from the Canning and Pedroni (2008) approach were obtained from a vector error correction model estimated based on the mean group estimator and are p-values of tests for the significance of the error correction term, which was calculated using the individual long-run coefficients from the CSARDLMG regressions. One lag of the first differences was included in the vector error correction model. The results from the Dumitrescu and Hurlin (2012) approach are p-values of tests for the joint significance of lagged explanatory variables, calculated using an average Wald statistic. One lag of the explanatory variables was included, as suggested by the BIC. All tests are based on regressions that control for country fixed effects. Following Eberhardt and Teal (2013), we augmented the error correction equations with cross-sectional averages of both the dependent and independent variables as additional regressors to control for error cross-sectional dependence arising from unobserved common factors. To account for error cross-sectional dependence in the Dumitrescu and Hurlin (2012) tests, we used their bootstrap approach. Since Dumitrescu and Hurlin's (2012) test combined with the bootstrap technique in Stata requires balanced panel data, the dataset was limited to 41 countries with complete data over the period 2000-2019. This dataset does not include Canada, Chile, Ecuador, India, Korea, Malta, Singapore, and South Africa.

4.3. Country-specific estimates of the long-run relationship between revenues from environmental taxes and R&D expenditures

We find that, on average, environmental taxes have a positive long-run effect on R&D. However, our finding for our entire panel of 49 countries does not necessarily imply a positive effect of environmental taxes on R&D in the majority of countries. The country-specific estimates of the long-run coefficients on $\log TAXES_{it}$ from the individual CSARDL regressions are reported in Table 3.

While caution is warranted in interpreting these estimates due to the relatively limited number of observations for each country, it can be concluded that there are cross-country differences in the long-run effects of environmental taxes on R&D, consistent with the tests for slope homogeneity. The coefficients vary from -0.475 in Ecuador to 2.385 in Ireland. However, the long-run coefficient on $\log TAXES_{it}$ is negative in only three countries (Australia, Ecuador, and Slovenia), and in all of these countries, it is not significantly different from zero. Thus, although the long-run coefficients vary from country to country, almost all (46) are positive. The implication is that the average positive long-run effect of environmental taxes on R&D is not due to a limited number of countries.

Table 3. Individual country estimates of the long-run relationship between environmental taxes and R&D

Country	log$TAXES_{it}$	Standard errors	Country	log$TAXES_{it}$	Standard errors
Argentina	0.858	0.636	Italy	0.953**	0.446
Australia	-0.138	5.230	Japan	1.062***	0.226
Austria	0.956**	0.481	Korea	0.921	1.171
Belgium	1.059**	0.537	Latvia	1.029***	0.368
Brazil	1.228	1.212	Lithuania	0.833*	0.507
Bulgaria	0.877	1.158	Luxembourg	1.223	0.880
Canada	1.059**	0.489	Malaysia	1.293	3.388
Chile	1.064*	0.605	Malta	0.598	0.984
China	1.129*	0.646	Netherlands	1.036	1.381
Colombia	0.936*	0.553	New Zealand	1.056	0.669
Croatia	1.002***	0.361	Norway	1.000**	0.509
Cyprus	0.983*	0.565	Poland	1.092	0.828
Czech Republic	1.033**	0.451	Portugal	0.652***	0.151
Denmark	0.916***	0.243	Romania	0.915	0.640
Ecuador	-0.475	0.858	Singapore	1.111	1.904
Estonia	0.950*	0.529	Slovak Republic	0.516	1.166
Finland	0.958**	0.396	Slovenia	-0.306	0.651
France	0.916**	0.405	South Africa	1.152	0.667
Germany	1.045	1.662	Spain	1.101**	0.536
Greece	1.227	0.859	Sweden	1.082**	0.456
Hungary	1.544	1.628	Türkiye	1.293	0.891
Iceland	1.333***	0.495	United Kingdom	1.219***	0.416
India	0.386	0.334	United States	0.521	0.660
Ireland	2.385	5.879	Uruguay	1.902***	0.383
Israel	1.297***	0.431			

Notes: The individual or country-specific long-run coefficients on log$TAXES_{it}$ are the individual or country-specific ratios of the country-specific coefficients on log$TAXES_{it-1}$ to the absolute values of the country-specific coefficients on log$R\&D_{it-1}$ (computed using the delta method). The reported standard errors are heteroskedasticity- and autocorrelation-consistent standard errors.

5. Additional results

We find evidence that environmental taxes lead to an increase in overall R&D activity, both on average across the countries and in the majority of countries in our sample. The obvious critique of this finding is that it is unclear whether the positive effect of environmental taxes on overall R&D activity is solely due to their impact on environmental R&D, or if environmental taxes also positively affect non-environmental R&D, or if they lead to a reduction in non-environmental R&D, as discussed in the Introduction. Unfortunately, the limited availability of data on environmental and non-environmental R&D makes it difficult to answer this question with a high degree of confidence.

To provide a preliminary answer to this question, we use the limited data on real R&D expenditures available from the OECD, classified by socioeconomic objectives, to construct measures of both environmental and non-environmental R&D.[6] We then regress these two

[6] We define environmental R&D as the sum of real R&D expenditures at PPP for pollution prevention and real energy R&D expenditures. The former is classified by the OECD under its R&D classification by socioeconomic objectives as 'Environment,' while the latter is classified under 'Energy.' Since energy R&D covers efforts aimed at improving the production, storage, transportation, distribution, and rational use of all forms of energy, including improving energy efficiency, we include it in our measure of environmental R&D. Non-environmental R&D is defined as total R&D minus our measure of environmental R&D. The data used to construct our measures of environmental and non-environmental R&D are available at
https://data-explorer.oecd.org/vis?df[ds]=DisseminateFinalDMZ&df[id]=DSD_RDS_GERD%40DF_GERD_SEO&df[ag]=OECD.STI.STP&dq=.A.._T....._T.XDC.&pd=2015%2C&to[TIME_PERIOD]=false.

measures (in logs) on environmental taxes (also in logs), lagged by one year to mitigate potential endogeneity and to account for the time it may take for the taxes to affect environmental and non-environmental R&D. The regressions also include several control variables, as well as country and time fixed effects.

We control for the growth rate of real GDP at PPP $GDPGROWTH_{it}$, the gross tertiary enrolment rate $TERTIARY\_SCHOOLING_{it}$, the percentage ratio of trade to GDP $(TRADE/GDP)_{i}$, the percentage ratio of net FDI inflows to GDP $(FDI/GDP)_{it}$, and an index of regulatory quality $REGULATORY\_QUALITY_{it}$.[7] All these variables are included with one lag, like environmental taxes. The data on these variables are from the WDI. Combining the data on all variables, including environmental taxes, results in an unbalanced panel of 16 countries from 1994 to 2015.[8] It is perhaps needless to say that the unbalanced nature of this panel and its short time dimension prevent us from using the methods from the previous section.

As can be seen from Table 4, $\log TAXES_{it\text{-}1}$ is significantly and positively correlated with both environmental R&D and non-environmental R&D.[9] From this, it can be cautiously concluded that the positive effect of environmental taxes on overall R&D activity, found in the previous section, is likely due to both a positive effect on environmental R&D and a positive effect on non-environmental R&D. Of course, this conclusion should be viewed with caution given the small sample size and potential issues associated with these simple regressions.

Table 4. Estimates of the relationship between lagged environmental taxes and both environmental and non-environmental R&D

	(1) Dependent variable: log of environmental R&D	(2) Dependent variable: log of non-environmental R&D
$\log TAXES_{it\text{-}1}$	0.721***	0.228**
	(0.213)	(0.114)
$GDPGROWTH_{it\text{-}1}$	1.409*	0.699**
	(0.794)	(0.334)
$TERTIARY\_SCHOOLING_{it\text{-}1}$	-0.008	-0.001
	(-1.591)	(0.003)
$(TRADE/GDP)_{it\text{-}1}$	-0.009***	-0.002
	(0.003)	(0.002)
$(FDI/GDP)_{it\text{-}1}$	0.003	-0.001
	(0.003)	(0.002)
$REGULATORY\_QUALITY_{it\text{-}1}$	-0.128	0.061
	(0.189)	(0.098)
Number of countries	16	16
Number of observations	151	151

Notes: The regressions include country and time fixed effects. Numbers in parentheses are White heteroskedasticity-consistent standard errors. *** (**) [*] indicates significance at the 1% (5%) [10%] level.

6. Conclusion

This study was the first to examine the relationship between environmental taxes and overall R&D activity at the macro-level. Using unbalanced panel data for 49 countries between 1994 and 2021, and employing heterogeneous panel data techniques, we found that: (*i*) environmental taxes have, on average, a positive long-run effect on R&D expenditures; (*ii*) the direction of causality runs from environmental taxes to R&D and not vice versa; and (*iii*) while the long-run effect of environmental taxes varies across countries, it is positive in almost all

[7] Since net FDI inflows are negative in some years, as is the index of regulatory quality, we do not use the logarithms of our ratio variables as well as $REGULATORY\_QUALITY_{it}$.

[8] The countries in our sample are Argentina, Bulgaria, Chile, the Czech Republic, Estonia, Hungary, Iceland, Korea, Lithuania, Norway, Portugal, Romania, the Slovak Republic, Slovenia, South Africa, and Spain.

[9] For space reasons, we do not discuss the signs of the estimated coefficients of the control variables in detail here. However, we briefly note that the unexpected sign of some coefficients, such as the coefficient on $TERTIARY\_SCHOOLING_{it\text{-}1}$, could be due to collinearity problems.

cases, indicating that the average positive long-run effect of environmental taxes on R&D is not driven by a few countries. We also found some support for a positive effect of environmental taxes on both environmental R&D and non-environmental R&D, although this is based on a smaller sample of countries over a shorter time period.

The obvious limitation of this study is that it provides no explanation for the observed cross-country heterogeneity in the effects of environmental taxes on R&D. It thus does not address which factors may influence the impact of environmental taxes on overall R&D activity, as well as on environmental and non-environmental R&D. We leave this question for future research, at both the micro and macro levels.

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Appendix

Table A1. Panel unit root tests

	Pesaran (2007)	Karavias and Tzavalis (2014)
Levels		
$\log R\&D_{it}$	0.908	0.910
$\log TAXES_{it}$	0.450	1.000
First differences		
$\Delta\log R\&D_{it}$	0.000	0.000
$\Delta\log TAXES_{it}$	0.000	0.000

Notes: Reported values are *p*-values. The unit root tests for the levels include country-specific intercepts and country-specific linear time trends. The unit root tests for the first differences include country-specific intercepts. The panel unit root tests developed by Pesaran (2007) control for error cross-sectional dependence by incorporating (weighted) cross-sectional averages and are applied to the original, untransformed data. To control for error cross-sectional dependence due to unobserved common factors in the panel unit root test developed by Karavias and Tzavalis (2014), we use demeaned data. The Pesaran (2007) test does not allow for structural breaks, which may lead to a false acceptance of the null hypothesis of a unit root. Therefore, we use also the Karavias and Tzavalis (2014) test, which allows for possible structural breaks. We allow for one endogenously determined structural break in the intercepts (and trends) of the series. One lag of the first differences was used in the Pesaran (2007) tests. The Karavias and Tzavalis (2014) test, by construction, does not involve lags of the first differences.

Table A2. Panel cointegration tests

	Panel statistics	Group mean statistics
Pedroni (1999)		
Variance ratio statistic	3.186***	
PP *rho*-statistics	-8.363***	-7.826***
PP *t*-statistics	-9.957***	-14.440***
ADF *t*-statistics	-11.525***	-15.438***
Westerlund (2005)		
Variance ratio statistics	-1.5615*	-2.893***
Westerlund (2007)		
τ-statistics (*z*-values)	-518.668	-29.094
[Bootstrap *p*-values]	[0.000]***	[0.000]***
α-statistics (*z*-values)	-442.031	-9.624
[Bootstrap *p*-values]	[0.000]***	[0.000]***
Gengenbach et al. (2016)		
ECM *t*-statistic		-3.713***

Notes: The dependent variable in the Pedroni (1999) and Westerlund (2005) tests is $\log R\&D_{it}$. The dependent variable in the tests of Westerlund (2007) and Gengenbach et al. (2016) is $\Delta\log R\&D_{it}$. One lag of the first differences was used in the Pedroni (1999) (PP and ADF) tests. One lead and one lag of the first differences were included in the Westerlund (2007) tests. No lags of the first differences were included in the Gengenbach et al. (2016) tests. The Pedroni (1999) and Westerlund (2005) test statistics are distributed as standard normal. The Westerlund (2007) statistics are distributed as standard normal in the case of no error cross-sectional dependence. While Pedroni's variance ratio test has a one-sided rejection region consisting of large positive values, all other tests reject for large negative values. The critical value at the 1% significance level for the Gengenbach et al. (2016) *t*-test for $N = 50$ (and one regressor) is -2.672. The results of the Pedroni (1999) and Westerlund (2005) tests are based on demeaned data to account for potential error cross-sectional dependence due to unobserved common factors. To account for error cross-sectional dependence in the Westerlund (2007) tests, we used the bootstrap approach of Westerlund (2007). Bootstrap *p*-values (based on 500 replications) are in brackets. The Gengenbach et al. (2016) test accounts for error cross-sectional dependence via the use of cross-sectional averages. We included one lag of the first differences of the cross-sectional averages in the Gengenbach et al. (2016) test. *** [*] indicates rejection of the null hypothesis of no cointegration at the 1% [10%] level.