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Environmental Kuznets Curve: Tipping Points, Uncertainty and Weak Identification

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Abstract We consider an empirical estimation of the environmental Kuznets curve (EKC) for carbon dioxide and sulphur, with a focus on confidence set estimation of the tipping point. Various econometric—parametric and nonparametric—methods are considered, reflecting the implications of persistence, endogeneity, the necessity of breaking down our panel regionally, trends and temporal instability, and the small number of countries within each panel. In particular, we propose a parametric inference method that corrects for potential weak-identification of the tipping point. Weak identification may occur if the true EKC is linear while a quadratic income term is nevertheless imposed into the estimated equation. Relevant literature to date confirms that non-linearity of the EKC is indeed not granted, which provides the motivation for our work. We also propose a non-parametric counterpart to the parametric confidence set, for sensitivity analysis. Viewed collectively, our results confirm an inverted U-shaped EKC in the OECD countries but generally not elsewhere, although a local-pollutant analysis suggests favorable exceptions beyond the OECD. Our measures of uncertainty confirm that it is difficult to identify economically plausible tipping points. Policy-relevant estimates of the

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tipping point can nevertheless be recovered from a local-pollutant long-run or non-parametric perspective.

Keywords Environmental Kuznets curve · Fieller method · Delta method · CO_2 and SO_2 emissions · Confidence set · Tipping point · Climate policy

1 Introduction

The environmental Kuznets curve (EKC) describes an inverted " U" relationship between per capita income and pollution levels. Viewed as a stylized feature, the EKC caught the attention of the profession following empirical work by—among others—Grossman and Krueger (1995).[1] Since then, research on the curve has evolved in response to two major challenges, both of which reflect common conceptual problems associated with reduced-form relationships. The first is a lack of compelling theoretical foundations. The second is a plethora of serious and lasting econometric imperfections given available data.[2]

Traditionally, the EKC is estimated using panel data regressions known to be plagued by trending, endogeneity, heterogeneity, and pooling problems. For these reasons, reported estimates are fragile for important parameters, including the coefficient on the quadratic income term.[3] This affects other objects of interest such as policy implications or inference about the *tipping point*, which refers to the level of income where per capita emissions reach their maximum.

Although substantial, this literature has not yet produced a serious consensus view. Even so, developments in econometrics have made applied works on the EKC more credible than it was in the early to mid-nineties. Progress has resulted from attention to functional forms and controls, and to assumptions on trends. Yet despite progress, little attention has been paid to estimation uncertainty about the tipping point. In this paper, we focus on this problem.

We consider an empirical estimation of the EKC for carbon dioxide and sulphur, with a focus on the tipping point. Our panel—of 114 countries for CO_2 and 82 for SO_2—spanning the period 1960–2007 is disaggregated into several groupings. OECD countries comprise one group while all others are grouped into six geographic regions. Disaggregation is necessary to reduce biases resulting from inappropriately pooling the data when countries are dissimilar. Our estimators take into account the high degree of persistence in the data and the presence of endogeneity. Disaggregating our panel into regions necessarily places models into a "small sample" (in particular small n, where n refers to the number of countries) framework. We thus favour panel data methods that have been proved to work relatively well in the small n context.

Historically, the tipping point has not been a primary object of interest in most of these studies. A voluminous part of this literature has rather focused on assessing the existence of the

[1] Early studies that found evidence of the EKC include Shafik (1994), Selden and Song (1994), Holtz-Eakin and Selden (1995) and Cole et al. (1997). These studies were generally optimistic about the potential for economic growth to solve environmental problems for several pollutants.

[2] For surveys, see *e.g.* Carson (2010), Wagner (2008), Vollebergh et al. (2009), Brock and Taylor (2005), Cavlovic et al. (2000), Dinda (2004), Stern (2001; 2003; 2004; 2010), Yandle et al. (2004), Dasgupta et al. (2002), Levinson (2002), and the references therein. Other works are also discussed below.

[3] The range of published estimates is wide and covers values close to zero for the quadratic component, and controversial income elasticities.

EKC, which broadly entails the following: at early stages of development, pollution initially rises with per capita income but then falls as per capita income exceeds some threshold level. Available studies have applied a variety of econometric models and methods, each taking into account a different feature of the data that was previously overlooked. For example (we refer the reader to the above cited surveys for a more exhaustive summary), Stern et al. (1996) argue that heteroskedasticity is present in grouped data. List and Gallet (1999) do not find support for the poolability of the data for US states. Harbaugh et al. (2002) find that results (on air pollutants) are sensitive to functional forms, additional covariates, sampling periods and geographic location. To tackle the problem of poolability, Lee et al. (2010) disaggregate their sample of 97 countries into four regions and estimate an EKC for water pollution. They find no EKC in the full sample of countries, but do find EKC's for developed regions. Non-parametric specifications and/or specifications focusing on pollution growth have also been considered; see List et al. (2003), Azomahou et al. (2006), Ordás-Criado et al. (2011), Kalaitzidakis et al. (2011) and the references therein.[4]

A second strand in the recent literature has questioned the feasibility of estimating the EKC by analyzing the time series properties of income per capita and emissions per capita. By investigating whether both variables have a unit root, scholars are questioning the extent to which the time series properties of the data render previous estimates of the EKC spurious. The question of whether income and emissions cointegrate is—in fact—at center stage. Perman and Stern (2003) use panel unit root tests and find that sulphur emissions, global GDP and its square expressed in natural logs are stochastically trending, casting doubt on the general applicability of the EKC hypothesis. In particular, they argue that typical specifications for the EKC are too simple for cointegration to hold. Richmond and Kaufmann (2006) estimate EKCs for CO_2 in a sample of 36 countries over the period 1973–1997. They find CO_2 emissions, fuel mix, and GDP per capita are all nonstationary. Romero-Avila (2008) use a panel stationarity test which allows for multiple breaks and cross-sectional dependence, and find that world per capita income is nonstationary and per capita CO_2 emissions are regime-wise trend stationary. Another example is Jalil and Mahmud (2009) who use a cointegration based analysis to estimate an EKC for China. They find evidence for a long run relationship between per capita CO_2 emissions and per capita income and a Granger causality test indicates that the direction of causation runs from economic growth to emissions. Stern (2010) proposes the between-estimator to address the cross-sectional dependence and time-effect problems documented by Wagner (2008) and Vollebergh et al. (2009). Stern also points out that time-dummies will not capture time-varying technological changes, and the latter may lead to contemporaneous correlation between regressors and country effects and/or residual errors.

Non-stationary time-series tools can provide concise and informative summaries of relations among environmental and growth data. But we should not expect that such analyses will resolve controversies. In this regard, our view conforms with Stern (2010) on one fundamental dimension: empirical work on the EKC confronts inevitable hurdles arising from persistence. For this reason, we do not rely on pre-testing in our analysis of tipping points. Instead, we consider the most recent panel techniques that have been proved reliable in dynamic contexts with persistent data. Our interest is to understand whether the tipping point can be estimated (given available econometric know-how) with enough precision regardless of the time series properties of the data.

[4] A tipping point consistent with our definition may be hard to formulate from a general non-parametric perspective.

Many researchers (refer to the above cited surveys) report point estimates of the tipping point without worrying about standard errors, and in the few cases where intervals are reported, computation details are often lacking. For instance Holtz-Eakin and Selden (1995) estimate the tipping point at $35,428, while Cole et al. (1997) estimate a tipping point of $62,700 for a quadratic function in logs and $25,100 for a quadratic function in levels. Cole et al. (1997) also estimate standard errors for the tipping point and find them to be large. Figueroa and Pastén (2009), who utilize a random coefficients model to analyze sulphur dioxide emissions, find an EKC present in 17 of 28 high income countries and estimate country specific tipping points which range between $6,201 and $12,863. Stern (2010), citing supporting evidence from Vollebergh et al. (2009), Wagner (2008) and Stern and Common (2001), argues that reported *lower* estimates of tipping points and elasticities are typically biased. Specifically, Stern examines the relationship between sulphur dioxide and carbon dioxide emissions and income using a variety of panel estimation techniques including OLS, first differences, fixed effects, and random effects. However, Stern argues that the between estimator is likely to be the most reasonable estimator of the long run relationship between income and emissions, because it is consistent for both stationary and non-stationary data in the presence of misspecified dynamics and heterogeneous regression coefficients. Stern finds no EKC using the between estimator for both pollutants, but instead a positive linear relationship. Stern also estimates the tipping point for each quadratic model as well as its standard error. With respect to carbon, Stern finds that the between estimator yields either a tipping point insignificantly different from zero (due to the coefficient on GDP squared being positive) using data from Vollebergh et al. (2009) and $653,110 using the data from Wagner (2008), with a standard error of $2,084,513.[5]

In short, while reported confidence intervals for EKC model parameters are often narrow, reported estimates of the tipping points are *all over the map* and suggest substantive disagreements. For the purpose of this paper, more important than the specific estimates is our concern with uncertainty. Providing empirically grounded policy advice requires measurable precision. Accounting for uncertainty carefully could change our conclusions about the strength of evidence on the EKC and might also lead us to question whether such a simple reduced form is answering the most interesting questions about income and emission data. Put differently, far more attention needs to be paid for identification of the tipping point.

The tipping point can be easily defined within a standard EKC regression. To set focus (our framework is formally defined below), let EM_{it}, be the logarithm of per capita emissions in country i and year t, and GDP_{it} be the logarithm of the country's per capita income. Consider the regression of EM_{it} on: (i) GPD_{it} (with coefficient β_1), (ii) GDP_{it}^2 (with coefficient β_2), and (iii) various controls, for $t = 1, \ldots, T$ and $i = 1, \ldots, n$. Then the tipping point corresponds to $\delta = \exp(-\beta_1/2\beta_2)$. Given consistent regression estimates, consistent point estimates for the tipping point follow straightforwardly. It is however rather difficult to derive reliable confidence bounds for a ratio of parameters.

The Delta method [defined formally in Sect. 3.2] is commonly prescribed for this purpose. In view of its Wald-type form, the method is justified asymptotically for a wide class of models suitable for estimation by consistent asymptotically normal procedures. However, even when the numerator and denominator are identifiable, a ratio involves a possibly discontinuous parameter transformation. More precisely in our case, as $\beta_2 \to 0$, the ratio $-\beta_1/2\beta_2$ becomes

[5] In Stern (2010, Table 4), for the case using Wagner's carbon data, the fixed effects estimated turning point is $41,678 with a standard error of $4,043 without time effects and $15,837 with a standard error of $1,060 with time effects. This contrasts sharply with the between estimator where the turning point is $653,110 with a standard error of $2,084,513.

weakly identified. This should not be taken lightly since a zero value for β_2 has not been convincingly refuted in the EKC literature.

When a parameter is weakly identified, reliance on usual standard errors can be misleading in the following sense. Usual confidence intervals of the form {estimate $\pm$ asymptotic α-level (say 5 %) cut-off point $\times$ asymptotic standard error} will **not** cover the true parameter value with probability $1-\alpha$ (say 95 %).[6] Coverage probabilities can in fact be way below the hypothesized (say 95 %) confidence level. So even if standard errors estimated using usual methods are narrow, they still provide a spurious assessment of the true uncertainty. The same holds true for standard bootstrap methods in the case of ratios.[7] Alternative methods based on generalizing Fieller's (1940, 1954) approach [also formally presented in Sect. 3.2 and Appendix 7] that will not suffer from this problem have recently gained popularity.[8] The main difference between the Delta and Fieller method is that the former will achieve significance level control [that is, will cover the unknown true value with the hypothesized probability (say 95 %)] only if the ratio is strongly identified [that is if β_2 is far enough from the zero boundary], whereas the latter does not require identification [that is, it is level-correct whether β_2 is zero, local-to-zero or non-zero].[9] In other words, the Fieller method is robust to modeling mistakes resulting from imposing non-linearity of the EKC.

Presuming a false degree of precision is consequential. For example, if the true EKC is linear (see *e.g.* Stern (2010), Kalaitzidakis et al. (2011) and the references therein for supportive arguments) and the researcher nevertheless imposes a quadratic income term into the estimated equation, then the standard confidence interval for the tipping point will appear quite tight yet will most certainly not cover the true value. Associated decisions are thus misguided (arbitrarily false). For the ratio to be identified, the denominator has to be far enough from zero.[10] It is however worth noting that such a check is hard-wired into the Fieller method: if β_2 is truly zero then the Fieller confidence set will be unbounded and will alert the researcher to this fact. The natural step when non-linearity of the curve is not granted (leading to possible weak identification of the tipping point) is to incorporate this uncertainty into set-estimation, which is what the Fieller method delivers in contrast to the Delta method. The Fieller approach thus comes with an assurance that it will inform us of poor-identification of the tipping point, which has an important potential to generate more reliable policy prescriptions based on the EKC.

We validate the above analysis with non-parametric specification checks, using the spline-based method from Ma and Racine (2013), Ma et al. (2012) and Racine and Nie (2011). In particular, for cases where an inverted-U shape is confirmed, we estimate a tipping point relaxing symmetry. Recall that an EKC is not necessarily symmetric, yet parametric quadratic equations typically impose symmetry. We thus check whether the latter assumption is overly restrictive and whether it affects tipping point estimates importantly. We also check for time instabilities non-parametrically, by breaking-down our sample over four time periods defined to parallel global economic cycles and technical and regulatory developments. Severe

[6] See Dufour (1997). Related results can also be found in the so called *weak instruments* literature which is now considerable; see the surveys by Dufour (2003), Stock et al. (2002), and the viewpoint article by Stock (2010). Weak instruments and inference on ratios raise comparable local identification problems.

[7] Bolduc et al. (2010) find that the *delta* and bootstrap method are spurious even in the simplest design they consider. Coverage rates collapsing to zero [which means that the probability of the estimated interval to include the unknown true value of the ratio is zero] are also documented for empirically relevant scenarios.

[8] See Zerbe et al. (1982), Dufour (1997), Bernard et al. (2007) and Bolduc et al. (2010).

[9] Applications of Fieller's method in econometrics are scarce; see Beaulieu et al. (2013), Bernard et al. (2007), Bolduc et al. (2010).

[10] For a parallel with the weak-instruments problem, refer to Stock (2010, pp. 86–87).

temporal inconsistencies are worth investigating for usual motives, and because our parametric treatment of time effects as fully flexible (across-regions) may not suffice to capture technology. For this reason, we also analyze the information-content of controls, since as proposed, controls may reflect technological progress. Formally, we produce and analyse an estimate of emissions as a function of each control, fixing remaining covariates including GDP to their median. These curves may help recover useful information for sensitivity analysis.

Our results reveal very serious uncertainty, even when focusing on cases where the coefficient on GDP_{it}^2 is significant and negative. On balance, we find that an EKC exists in the OECD countries but generally not elsewhere, although a local-pollutant analysis suggests more favorable results beyond the OECD. Despite its existence in the OECD, our measures of uncertainty suggest that it is difficult to identify an economically plausible tipping point. Policy relevant estimates of the tipping point can nevertheless be recovered from a local-pollutant long-run or nonparametric perspective. Non-parametric results incite further work on the statistical foundations of our proposed non-parametric tipping point set estimate which is—to the best of our knowledge—new to the (econometric and environmental) literature.

The paper is organized as follows. Our framework and estimation methods are presented in Sect. 2. In Sect. 3, we summarize our parametric confidence set estimation methods for the tipping point. Our empirical analysis is reported in Sect. 4. Section 5 presents concluding arguments. An appendix summarizes our data set and discusses technical details.

2 Framework

Two model classes are considered: (i) a parametric baseline case, and (ii) a non-parametric spline-based alternative. To be clear, we are not proposing these two cases as necessarily mutually exclusive. Rather, we see our non-parametric model as providing a useful specification check. Our specific treatment of the parametric case also aims to robustify inference against an unfounded quadratic assumption.

2.1 Baseline Parametric Model

The baseline case is the following panel regression

$$EM_{it} = \eta_t + \beta_{0i} + \beta_1 GDP_{it} + \beta_2 GDP_{it}^2 + \beta_3 IND_{it} + \beta_4 CIE_{it} + \beta_5 EFF_{it} + u_{it} \quad (2.1)$$

where EM_{it} is per capita emissions in country i and year t for $t = 1, \ldots, T$ and $i = 1, \ldots, n$; GDP_{it} is the country's per capita income, and IND_{it}, CIE_{it}, and EFF_{it} are control variables defined below. All variables are in logs. We consider annual per capita CO_2 emissions for 114 countries 1960–2007, as well as SO_2 emissions for 85 countries, 1960–2005. β_{0i} includes a country effect; further assumptions on the residual errors and time effects are discussed in Sect. 2.2. The baseline model assumes cross sectionally constant time effects reflected via the separate time period intercept η_t.

The first control, IND_{it}, is the share of GDP in a given year derived from industry. It has been observed that the per capita energy use of countries usually peaks at the same time as the industrial share of GDP.[11] This occurs at different times for different countries and reflects the particular experience of each country with respect to industrialization and eventual shifts to a service economy. The second control, CIE_{it}, is the number of kilograms of CO_2 emitted

[11] See, for example, Rühl and Giljum (2011).

per kilogram of oil equivalent energy. An important determinant of the carbon intensity of energy is the fossil fuel mix used in a country. Coal has twice the CO_2 emissions relative to natural gas per unit of energy and oil products are half way in between. CO_2 intensity of energy depends also on technology and on the efficiency of the combustion process. Lastly, the third control, EFF_{it}, is the percentage of energy a country uses that is derived from fossil fuels. This control takes into account a country's natural resource endowments. While fossil fuels are traded to various extents on world markets and thus are accessible to all countries, some energy sources are available only at the local level. This is the case of hydro and nuclear power, two energy sources that have very low emissions. The coefficients of all three controls are expected to be positive.

In addition to a panel encompassing the full sample of countries, regional panels are segmented into the OECD, Non-OECD Asia (hereafter referred to as Asia), the Middle East & North Africa, Sub-Saharan Africa, South America, and Central American & the Caribbean. A full list of countries included in each region appear in Appendix 6.

Equation (2.1) implies an inverted-U form with respect to GDP. The level of income at which the curve reaches a maximum can be solved for and is known as the tipping point, which corresponds to

$$\delta = \exp(-\beta_1/2\beta_2) \tag{2.2}$$

with $\beta_1 > 0$ and $\beta_2 < 0$. Sign restrictions imply that a maximum for the emissions is reached at a positive level of GDP. These restrictions are however not numerically imposed at the estimation stage. Our main objective is to derive a confidence region for δ. To present our estimators of the latter in their simplest form, we rewrite (2.1) as

$$EM_{it} = \eta_t + \beta_{0i} + g_1[-GDP_{it}] + g_2[GDP_{it}^2/2] + \beta_3 IND_{it} + \beta_4 CIE_{it} + \beta_5 EFF_{it} + u_{it} \tag{2.3}$$

so that the tipping point becomes

$$\delta = \exp(g_1/g_2). \tag{2.4}$$

The sole purpose of (2.3–2.4) is to simplify formulas and discussion. To avoid confusion with reference to available literature, all reported results in Sect. 4 pertain to the coefficients of (2.1).

2.2 Further Parametric Assumptions

Completing (2.1) requires assumptions on regressors and error terms, as well as on time trends. In the absence of an economic-theory basis, various—reasonable although typically ad hoc—options are considered for this purpose in available works, and on balance, none emerges as a best choice. We thus suggest the following as minimal set of assumptions for estimation purposes.

1. Possible endogeneity of the regressors in (2.1) in a static context, that is, ignoring persistence in the residual error terms. So we estimate the equation with the error component 2SLS estimator proposed by Baltagi and Li (1992). In static panels, available results on the finite sample [n small relative to T] properties of this estimator support its consideration in our context. Reported results instrument GDP_{it}, its square and CO_2 intensity of energy using first lags of these variables.[12]

[12] Results when all regressors were instrumented are qualitatively similar so we do not report them for space considerations.

2. Possible persistence in the residual u_{it}, of the common first order autoregressive process with coefficient ρ_0, leading to reparametrizing (2.1) into:

$$\begin{aligned} EM_{it} = \eta_t + \rho_{0i} + \rho_0 EM_{i,t-1} + \beta_1 GDP_{it} + \beta_2 GDP_{it}^2 + \beta_3 IND_{it} \\ + \beta_4 CIE_{it} + \beta_5 EFF_{it} + \rho_1 GDP_{i,t-1} \\ + \rho_2 GDP_{i,t-1}^2 + \rho_3 IND_{i,t-1} + \rho_4 CIE_{i,t-1} + \rho_5 EFF_{i,t-1} + e_{it} \end{aligned} \tag{2.5}$$

where the residual error term e_{it} is temporally uncorrelated. Though such restrictions are not always imposed, for further reference, it is useful to recall that (2.1) coincides with (2.5) when

$$\rho_{0i} = \beta_{0i}(1-\rho_0), \quad \rho_j = -\rho_0\beta_j, \quad j = 1, \ldots, 5. \tag{2.6}$$

We thus consider the bias corrected LSDV estimator of Kiviet (1995) and bootstrap standard errors (as in Bruno 2005). This method does not impose (2.6) and presumes a dynamically stable model, that is a non-unitary ρ_0 in our case. Bias correction of this estimator requires an initial consistent estimate; we use the Anderson and Hsiao (1982) estimator for this purpose which is better suited for our small n than its GMM counterparts. In contrast with the Baltagi and Li (1992) estimator, Kiviet's bias-corrected LSDV presumes that the regressors may be correlated with the individual-specific effect but are strictly exogenous with respect to e_{it}. So whereas the former works with endogenous regressors in a static context, the latter allows dynamics [as described] yet requires strict exogeneity of GDP and controls.[13]

3. Possible non-stationary data. Pesaran et al. (1999) provide an econometric framework whose validity has been demonstrated for fixed n, which allows (2.5), *provided* η_t *is supressed* , to be viewed as a stable long-run relation with associated error correction form

$$\begin{aligned} \Delta EM_{it} = -\rho_1 \Delta GDP_{it} - \rho_2 \Delta GDP_{it}^2 - \rho_3 \Delta IND_{it} - \rho_4 \Delta CIE_{it} - \rho_5 \Delta EFF_{it} \\ + \phi(EM_{i,t-1} - \beta_{0i} - \beta_1 GDP_{it} - \beta_2 GDP_{it}^2 - \beta_3 IND_i \\ - \beta_4 CIE_{it} - \beta_5 EFF_{it}) + e_{it} \end{aligned} \tag{2.7}$$

where $\phi = \rho_0 - 1$ is negative. Although related, the framework of Pesaran et al. (1999) differs from traditional cointegration definitions that require I(1) regressors. In other words, the existence of a long-run relation between the dependant variable and the considered regressors does not rest on whether the regressors are I(1). A separate time period intercept is in principle incompatible with this model's stochastic fundamentals. Consistency requires independence of the regressors and residual errors, yet long-run coefficients can be estimated consistently when regressors are not strictly exogenous by augmenting the lags in the equation. Reported estimates rely on the first lag. We also consider relaxing and imposing $\beta_3 = \beta_4 = \beta_5 = 0$, with short run coefficients $\rho_j \neq 0$, to examine the short versus long-run contribution of controls, a point we shall revisit in Sect. 4.

The parametric form (2.1) assumes that emissions in every country within each considered region react similarly to shifts in explanatory variables even if intercepts are allowed to differ. Time period dummies are not country-specific. Such restrictions on modeling country level and time trend heterogeneities may lack fit even within regions and despite reliance on further

[13] Instrumental variables (IV) methods [*e.g.* Anderson and Hsiao (1982), Arellano and Bond (1991), and Blundell and Bond (1998)] may seem an attractive solution to treat endogeneity as well as persistence. Unfortunately, with small n which corresponds to the problem at hand, these IV methods, when applicable [see *e.g.* Bun and Kiviet (2006) for conditions on n relative to T], can be severely biased and highly imprecise; see *e.g.* Kiviet (1995), Judson and Owen (1999), Bruno (2005) and Bun and Carree (2005).

controls. Admittedly, it is desirable to allow the importance of unobserved factors, such as technology or geopolitical shocks, to change over time. However, β_1 and β_2 are not identified, as the partial effects of interest, with fully flexible unobserved heterogeneity. See Vollebergh et al. (2009) for a discussion and specific suggestions for the EKC, and e.g. Ahn et al. (2001) for more general panel data perspectives. It is worth noting that accounting for time-varying (deterministic) individual effects at least given current econometric know-how may come via further assumptions on conditioning information and dynamics, or via subjective priors. Whether such assumptions are more or less restrictive than time period dummies is beyond the scope of the present paper, at least with regards to the baseline set-up. The stochastic-trend form we consider in (2.7) provides an alternative perspective on modeling time effects.

2.3 Non-parametric Checks

We supplement the above analysis with non-parametric robustness checks, using the spline-based method from Ma and Racine (2013), Ma et al. (2012) and Racine and Nie (2011). The conditional mean is assumed to follow a non-linear and unknown function approximated via best-fit B-splines allowing for heteroskedasticity of unknown form, again assumed to depend on GDP and controls, as follows:

$$EM_{it} = f(GDP_{it}, Controls) + \sigma(GDP_{it}, Controls)w_{it}, \quad f(.) \text{ and } \sigma(.) \text{ unknown} \tag{2.8}$$

where w_{it} are *i.i.d.*. Estimation assuming exogenous and possibly endogenous covariates is conducted. In the latter case, we use the same instruments as in the above defined Panel 2SLS method.

The estimation output [further details are in the Appendix] provides a graphical representation of: (i) the fitted function, partialled-out with respect to each covariate, and (ii) companion point-wise confidence bands. Such estimations do not account for the panel structure of the data nor for its time series properties, and impose stationarity. So aside from the shape restriction, the non-parametric assumptions are not necessarily weaker than some of our parametric assumptions. For this reason, estimated curves as not used to formally test the fit of (2.1). Instead, we view them as summary representations of our data. Relaxing the shape restriction can nevertheless be informative, in the absence of consensus in this literature on the fit of the quadratic model.

In particular, we look for asymmetries in the estimated function in addition to turning points, since—although not required for an EKC—our quadratic parametric equations impose symmetry. We also check for time instabilities. Recall that trends [via time effects in the above 2SLS and LSDV estimation, or the stochastic long-run formulation in (2.7)] are accounted for parametrically. We thus estimate (2.8) over four time periods : (1) 1960–1972, (2) 1973–1985, (3) 1986–1999, and (4) 2000–2007 for CO_2 and 2000–2005 for SO_2. Subsamples are defined to reflect oil-price shocks, global economic cycles and technological changes, though formal break and stability tests are not intended. Then again, severe inconsistencies across time may be telling.

Non-parametric estimation of (2.8) produces partial regression surfaces, as reported in Figs. 1, 2, 3, 4, 5, 6, 7, 8, 9, and 10, along with confidence bands. For clarity, a "partial" surface corresponds to the estimated $f(.)$ plotted as function of one predictor, while the

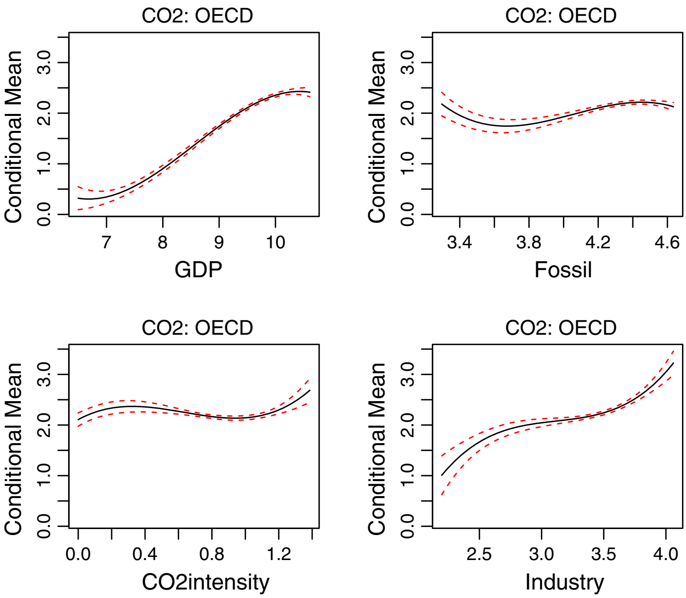

Fig. 1 CO_2: OECD all time periods—conditional regression splines estimates

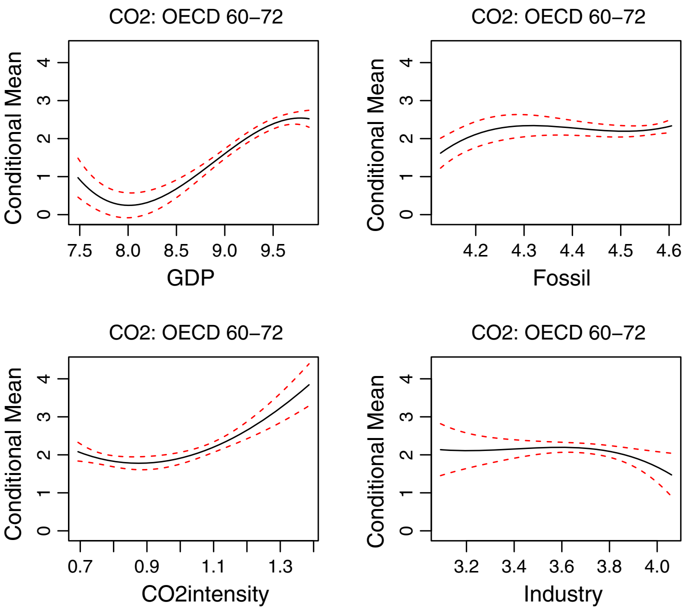

Fig. 2 CO_2: OECD—1960–1972 subsample—conditional regression splines estimates

remaining predictors, specifically, the covariates in (2.8) that do not appear on the axes in the reported figures, are held constant at their medians. In addition to analyzing the curves' form, we use these partial surfaces for the following purposes.

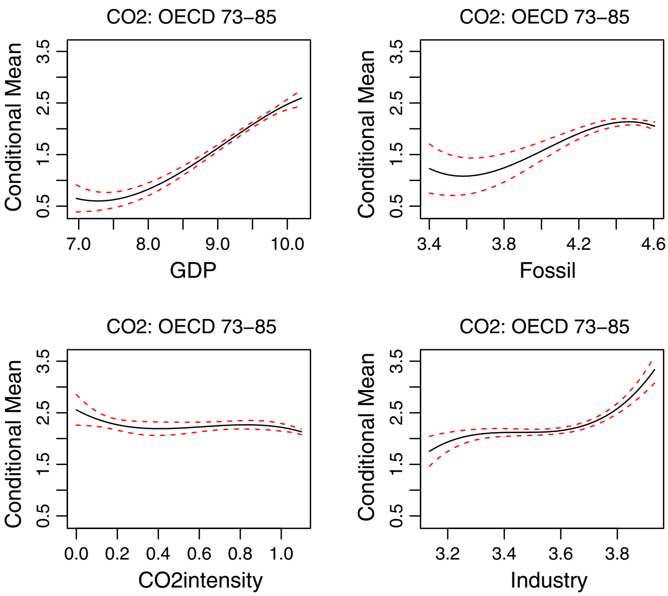

Fig. 3 CO_2: OECD—1973–1985 subsample—conditional regression splines estimates

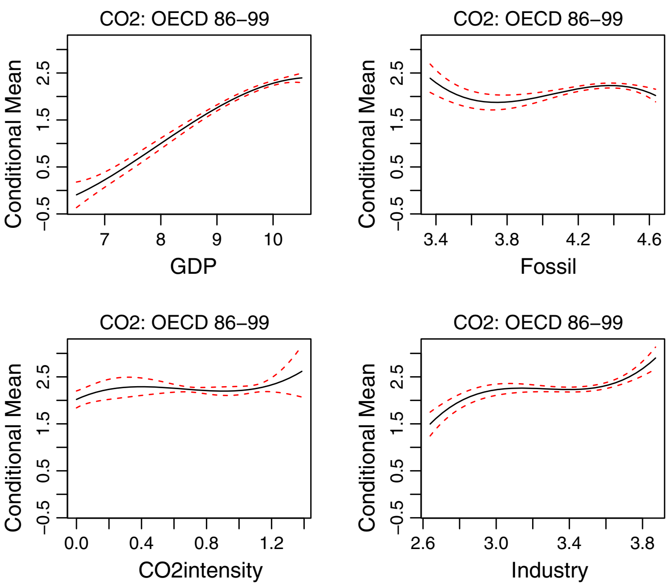

Fig. 4 CO_2: OECD—1986–1999 subsample—conditional regression splines estimates

1. Consider the partial Emission/GPD curve, which we denote

$$\hat{f}_{GDP} = \hat{f}(GDP|IND, CIE, EFF). \tag{2.9}$$

Points at which the derivative of this surface approach zero may thus be located. We use these points to derive diagnostic checks on the above parametric estimates of the tipping points. Further details are provided in Sect. 3.3.

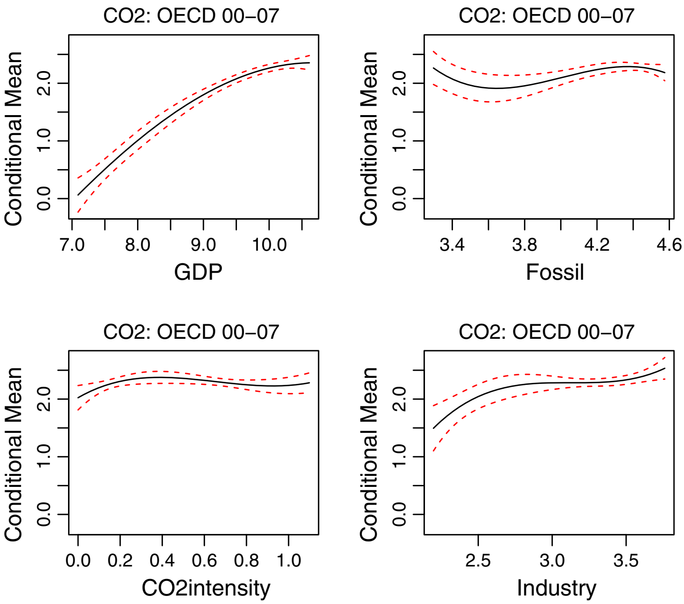

Fig. 5 CO_2: OECD—2000–2007 subsample—conditional regression splines estimates

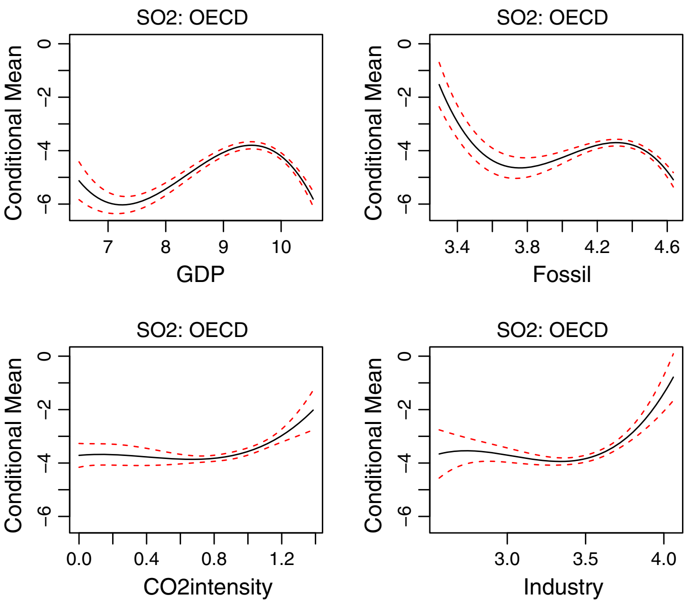

Fig. 6 SO_2: OECD all time periods—conditional regression splines estimates

2. Consider, in turn, the partial Emission/Control curves, denoted

$$\hat{f}_{IND} = \hat{f}(IND|GDP, CIE, EFF), \tag{2.10}$$

$$\hat{f}_{CIE} = \hat{f}(CIE|GDP, IND, EFF), \tag{2.11}$$

$$\hat{f}_{EFF} = \hat{f}(EFF|GDP, CIE, IND). \tag{2.12}$$

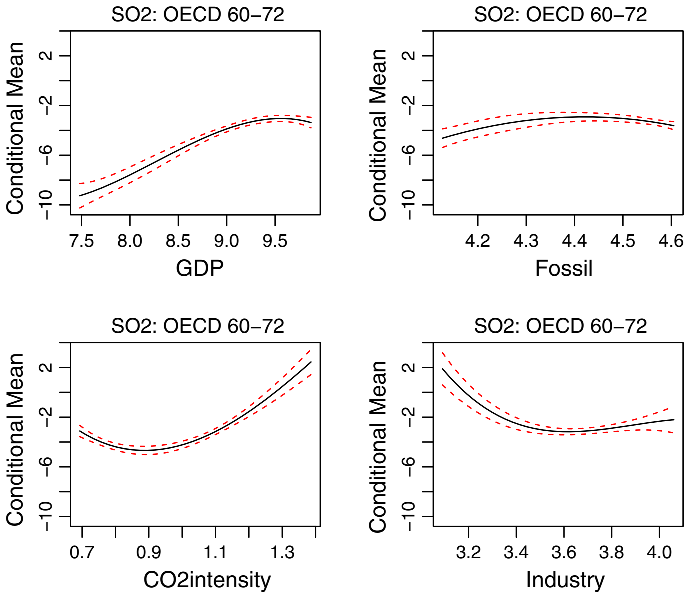

Fig. 7 SO_2: OECD—1960–1972 subsample—conditional regression splines estimates

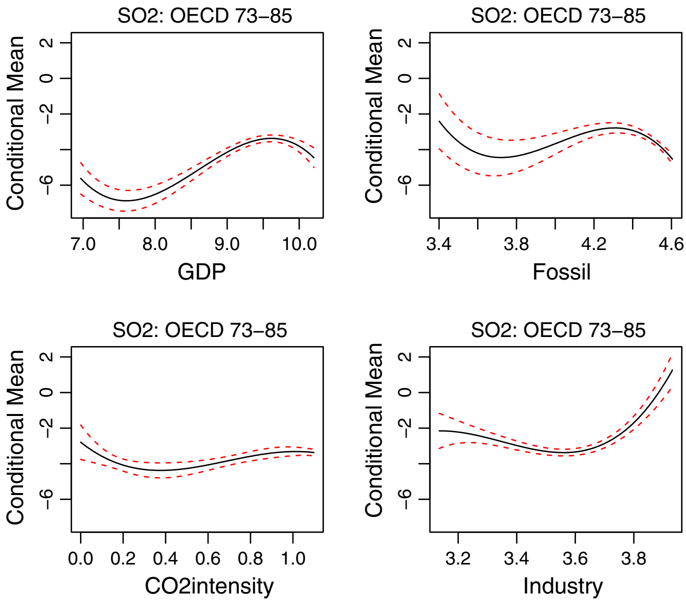

Fig. 8 SO_2: OECD—1973–1985 subsample—conditional regression splines estimates

One way to recover important information on technology that might be forgone via a regional parametric approach is to analyze (2.10–2.12). While fully-flexible (across-regions) time effects in the context of (2.1) may no longer suffice to proxy general know-how, controls may embed technical advances. In this respect, since a partial regression

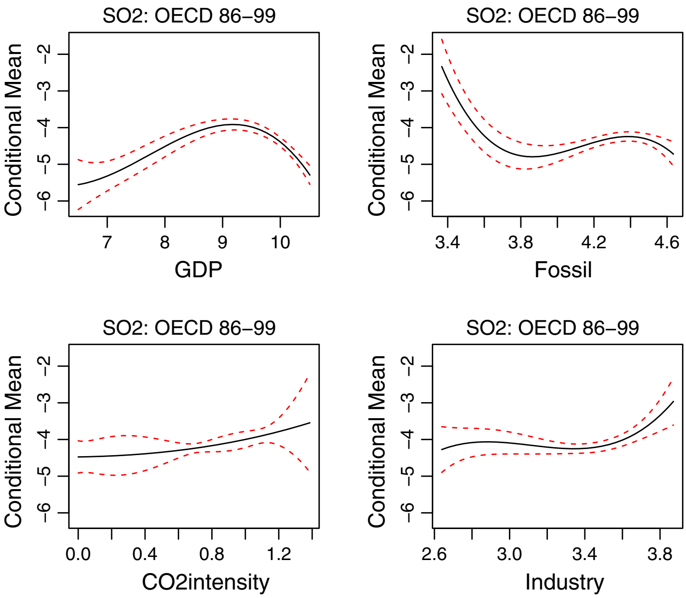

Fig. 9 SO_2: OECD—1986–1999 subsample—conditional regression splines estimates

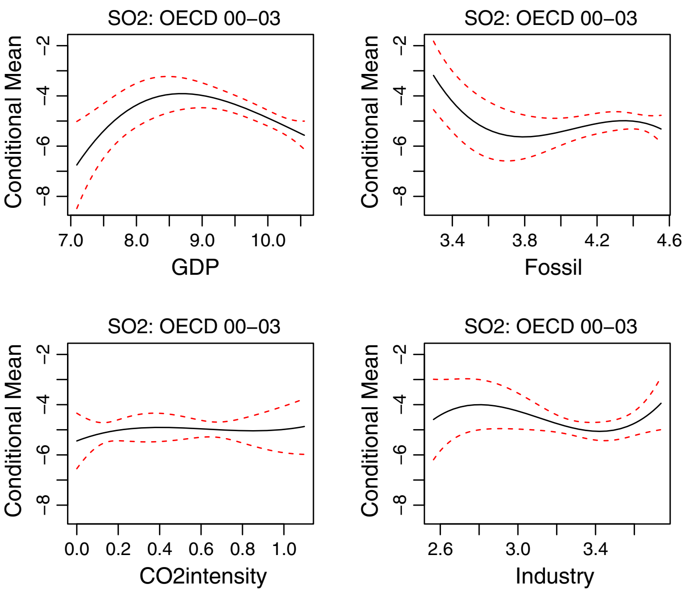

Fig. 10 SO_2: OECD—2000–2003 subsample—conditional regression splines estimates

surface [for example $\hat{f}_{IND} = \hat{f}(IND|.)$] may convey much more information than a scalar coefficient [for example β_3 as in (2.1)], (2.10–2.12) may have much more to tell about the technological and policy underpinnings of controls that their parametric counterparts when the traditional interpretation of time effects must be relaxed.

3 Estimation Uncertainty for Tipping Points

This section first discusses our set estimates for the tipping point. We first discuss identification and inference of δ defined by (2.3–2.4). We next propose our non-parametric diagnostics.

3.1 Identification of the Parametric Tipping Point

Typically, β_1 and β_2 [in (2.1)] and conformably, g_1 and g_2 [in (2.3)] are well identified. So for these parameters, consistent and asymptotically normal estimates [denoted $\hat{g}_1$ and $\hat{g}_2$] with a tractable variance/covariance matrix [denoted $\hat{\Sigma}_g$] are readily available. These include all methods we consider in this paper, as presented in Sect. 2.2. Plugging $\hat{g}_1$ and $\hat{g}_2$ into (2.4) yields a consistent estimate for δ [denoted $\hat{\delta}$]. Furthermore, given $\hat{\Sigma}_g$, a standard error [denoted $\hat{\Sigma}_\delta^{1/2}$] can easily be obtained for inference on δ, as shown *e.g.* in (3.1), via the Delta method. The usual t-statistic

$$\mathsf{t}_D\,(\delta_0) = (\hat{\delta} - \delta_0)/\hat{\Sigma}_\delta^{1/2}$$

associated with $\delta = \delta_0$ where δ_0 is any known value, which yields usual significance tests as well as the standard confidence interval for inference on δ, is thus easy to derive. Nevertheless, the asymptotic null distribution of $\mathsf{t}_D\,(\delta_0)$ is *not* Gaussian, because the definition of δ entails an identification *discontinuity*: when $\beta_2 = 0$, the ratio $-\beta_1/2\beta_2$ is not defined or, equivalently, the equation $2\beta_2 \log(\delta) = -\beta_1$ does not have a unique solution, which holds true even though β_1 and β_2 are well identified. Conformably, when $g_2 = 0$, the ratio g_1/g_2 is not defined or, equivalently, the equation $g_2 \log(\delta) = g_1$ does not have a unique solution, which holds true even though g_1 and g_2 are well identified. The distribution of $\mathsf{t}_D\,(\delta_0)$ is in fact non-standard and may be nuisance parameter dependent. Consequently, if we denote by $z_{\alpha/2}$ the two-tailed α-level standard normal critical point:

1. the significance level of a t test based on referring $|\mathsf{t}_D\,(\delta_0)\,|$ to $z_{\alpha/2}$ may be much larger than α, and
2. the confidence level of the usual interval estimate $\{\hat{\delta} \pm z_{\alpha/2}\hat{\Sigma}_\delta^{1/2}\}$ may be considerably lower than $1 - \alpha$.

For proofs, insights and further references, see Dufour (1997), Bolduc et al. (2010), Beaulieu et al. (2013), and the above cited surveys on the parallel weak-instruments literature. Concretely, when a parameter is not identifiable, data will barely carry any information on this parameter. Since any value in its parameter space is more or less equally acceptable, this should be reflected in any appropriate confidence set. In other words, weak-identification should, in principle, lead to diffuse confidence sets that can alert the researcher to the problem.

Unfortunately, if usual methods are applied when estimating weakly-identified parameters, for example, via an expression with bounded limits such as $\{\hat{\delta} \pm z_{\alpha/2}\hat{\Sigma}_\delta^{1/2}\}$, the expected diffuse intervals often do not materialize. Because of theoretical failures, usual methods produce very tight confidence intervals that are focused on "wrong" values. The econometric literature refers to this problem as one of poor coverage. For practitioners, this problem is doubly-misleading. First, estimated intervals would severely understate estimation uncertainty. Secondly, intervals will fail to cover the true parameter value, but in view of their tightness, this will go unnoticed.

These problems are averted if one applies a confidence set estimation method such as the Fieller method as proposed in this paper, which, in contrast to the Delta method, can produce unbounded outcomes. In contrast to a usual interval with bounded limits of the form $[c_l, c_u]$, with finite c_l and c_u, by unbounded outcomes, we mean, explicitly, confidence sets of the form: $]-\infty, c_l] \cup [c_u, +\infty[$ or even $]-\infty, +\infty[$.

3.2 Parametric Estimation Uncertainty

Assuming the considered estimator, $\hat{\delta} = \exp(\hat{g}_1/\hat{g}_2)$ where $\hat{g}_1$ and $\hat{g}_2$ are obtained using any of the above defined (in Sect. 2.2) parametric methods, is consistent and asymptotically normal, the so-called Delta method produces the following confidence interval $1-\alpha$ level confidence interval for δ:

$$DCS\,(\delta;\alpha) = \left[\hat{\delta} \pm z_{\alpha/2}\hat{\Sigma}_{\delta}^{1/2}\right],\ \hat{\Sigma}_{\delta} = \hat{G}'\hat{\Sigma}_{12}\hat{G},\ \hat{G} = \begin{bmatrix} \frac{\exp(\hat{g}_1/\hat{g}_2)}{\hat{g}_2} \\ \frac{-\hat{g}_1\exp(\hat{g}_1/\hat{g}_2)}{\hat{g}_2^2} \end{bmatrix} \tag{3.1}$$

where $z_{\alpha/2}$ refers to the two-tailed α-level standard normal cut-off point, and

$$\hat{\Sigma}_{12} = \begin{bmatrix} \hat{v}_1 & \hat{v}_{12} \\ \hat{v}_{12} & \hat{v}_2 \end{bmatrix}$$

refers to the subset of the variance/covariance matrix of the estimates of (2.3) that corresponds to $\hat{g}_1$ and $\hat{g}_2$. The basic steps leading to (3.1) can be summarized as follows.

First, the tipping point is viewed as a function [denoted $h(g)$] of $g = (g_1, g_2)'$ where $h(g)$ returns $\exp(g_1/g_2)$, so

$$\frac{\partial h(g)}{\partial g_1} = \frac{\exp(g_1/g_2)}{g_2},\quad \frac{\partial h(g)}{\partial g_2} = \frac{-g_1\exp(g_1/g_2)}{g_2^2}.$$

Second, the asymptotic variance of $h(\hat{g})$ with $\hat{g} = (\hat{g}_1, \hat{g}_2)'$ is derived as

$$\left[\text{Asymptotic Variance of } h(\hat{g})\right] = G(g)'\left[\text{Asymptotic Variance of } \hat{g}\right]G(g) \tag{3.2}$$

$$G(g) = \frac{\partial h(g)}{\partial g} = \begin{bmatrix} \frac{\partial h(g_1,g_2)}{\partial g_1} \\ \frac{\partial h(g_1,g_2)}{\partial g_2} \end{bmatrix}. \tag{3.3}$$

Next $\hat{G}$ is computed by plugging $\hat{g}_1$ and $\hat{g}_2$ into $G(g)$ in (3.3). Finally, $\hat{\Sigma}_{12}$ is substituted for the asymptotic variance of $\hat{g}$ in (3.2).

To set the stage for introducing the Fieller method, recall that $\left[\hat{\delta} \pm z_{\alpha/2}\hat{\Sigma}_{\delta}^{1/2}\right]$ collects the values of δ_0 for which the t-statistic

$$\mathsf{t}_D\,(\delta_0) = \left(\hat{\delta} - \delta_0\right)/\hat{\Sigma}_{\delta}^{1/2}$$

associated with

$$\mathcal{H}_D(\delta_0) : \delta - \delta_0 = 0 \Leftrightarrow \exp(g_1/g_2) = \delta_0$$

is not significant at the α level. Said differently, if we solve the inequality

$$\left|\hat{\delta} - \delta_0\right|/\hat{\Sigma}_{\delta}^{1/2} < z_{\alpha/2} \tag{3.4}$$

for δ_0, we get $\left[\hat{\delta} \pm z_{\alpha/2}\hat{\Sigma}_{\delta}^{1/2}\right]$. In statistics, solving for δ_0 in (3.4) is known as "inverting the $\mathsf{t}_D\,(\delta_0)$ test", where inverting a test with respect to a parameter means collecting all values (here δ_0) not rejected by this test at the α level. This definition relies on the usual duality between a t test and a standard confidence interval. Also, $\left[\hat{\delta} \pm z_{\alpha/2}\hat{\Sigma}_{\delta}^{1/2}\right]$ is known as "a Wald-type confidence interval".

In contrast, the Fieller method inverts an alternative t-statistic

$$\mathsf{t}_F\,(d_0) = (\hat{g}_1 - d_0\hat{g}_2)/[(\hat{v}_1 + d_0^2\hat{v}_2 - 2d_0\hat{v}_{12})^{1/2}]$$

associated with

$$\mathcal{H}_F(d_0) : g_1 - d_0 g_2 = 0.$$

Inverting $\mathsf{t}_F\,(d_0)$ requires solving for the set of d_0 values that are not rejected at level α using $\mathsf{t}_F\,(d_0)$ and a standard normal two-tailed cut-off $\mathsf{z}_{\alpha/2}$. In other words, we need to collect the d_0 values such that $|\mathsf{t}_F\,(d_0)| \leq z_{\alpha/2}$ or alternatively such that

$$(\hat{g}_1 - d_0\hat{g}_2)^2 \leq \mathsf{z}_{\alpha/2}^2(\hat{v}_1 + d_0^2\hat{v}_2 - 2d_0\hat{v}_{12}) \tag{3.5}$$

which is a second degree inequality in d_0. The resulting solution denoted $FCS\,(d;\alpha)$ [see (7.3) in Appendix 7] is either a bounded interval, an unbounded interval, or the entire real line $]-\infty, +\infty[$, where the unbounded solutions occurs when the denominator is close to zero.

$\mathcal{H}_F(d_0)$ can be linked to $\mathcal{H}_D(\delta_0)$ for $d_0 = \log(\delta_0)$. Because $FCS\,(d;\alpha)$ is obtained as described, taking the exponential of its limits provides the desired confidence set for δ. Mathematically, this corresponds to "projecting" the region implied by $FCS\,(d;\alpha)$. Said differently, it is easy to see that replacing d_0 by $\log(\delta_0)$ in (3.5) does not distort the inequality nor its statistical foundations. Solving for δ_0 in the resulting inequality would numerically coincide with applying the exponential to the limits of $FCS\,(d;\alpha)$.

Of course, the $\mathsf{t}_D\,(\delta_0)$ test and $DCS\,(\delta;\alpha)$ interval will have α and $(1-\alpha)$ as effective levels, if $\mathsf{t}_D\,(\delta_0) \overset{asy}{\sim} N(0,1)$. The $\mathsf{t}_F\,(d_0)$ test and $FCS\,(d;\alpha)$ interval will also achieve level control if $\mathsf{t}_F\,(d_0) \overset{asy}{\sim} N(0,1)$. As argued in Sect. 3.1, because $\mathcal{H}_D(\delta_0)$ requires $g_2 \neq 0$, the Gaussian approximation fails for $\mathsf{t}_D\,(\delta_0)$. In contrast, and because $\mathcal{H}_F(d_0)$ does not require restricting the parameter space of g_2, nor of g_1 for that matter

$$\hat{\delta} \text{ is asymptotically normal} \Rightarrow \mathsf{t}_F\,(d_0) \overset{asy}{\sim} N(0,1).$$

It follows that the $FCS\,(d;\alpha)$ will achieve level control whether g_2 is zero or not.

3.3 Non-parametric Diagnostics

Consider the partial Emission/GPD $\hat{f}_G = \hat{f}(GDP|Controls)$ surface [refer to (2.9) for definition and notation]. If this surface confirms an inverted-U shaped EKC, then its maximum allows us to define a non-parametric counterpart to δ. To obtain tipping point estimates comparable to those in Tables 4 ,5, 6, and 7, and because reported curves are in a log-scale conforming with our estimating equations, we first refit curves in levels. Then, in the presence of a inverted-U form, we locate the point were the derivative of the estimated function is the closest to zero. We back-out the GDP value corresponding to the latter as our tipping point estimate [denoted $\tilde{\delta}$], as well as a conformable confidence interval.

Although underlying bands rest on asymptotic theory [see Ma and Racine (2013) for regularity conditions], the finite sample performance of the interval we back-out have, to the best of our knowledge, not been analyzed as yet. To be clear, available bands have not been substantiated for formal inference on zero-derivative sets, even asymptotically. For this reason, we do not interpret results from a strict inferential perspective. Severe inconsistencies between these and our parametric results are nevertheless worth checking for.

4 Empirical Results

Data used in this paper are available from the World Bank's World Development Indicators (WDI) online database, and Stern (2005). CO_2 annual data, for 114 countries over the 1960–2007 period, is collected from the Carbon Dioxide Information Analysis Center, Environmental Sciences Division at the Oak Ridge National Laboratory in Tennessee. For SO_2, we use the annual dataset from Stern (2005) on 85 countries over the 1960–2005 period. Table 1 reports summary statistics of emissions data. GDP_{it} measures purchasing power parity corrected per capita income in thousands of constant USD with 2000 as the base year.

Tables 2, 3 report estimates for the emission Eq. (2.1). For presentation clarity, we report the estimates of the parameters of interest β_j, $j = 1, \ldots 5$; complete results are available upon request. Since sign restrictions have not been empirically imposed, interpretation of the tipping point with respect to an inverted U-shaped curve make sense when $\beta_1 > 0$ and $\beta_2 < 0$. So cases where the estimated β_1 and β_2 are significant at the 5 % level and both are correctly signed are reported in bold characters. Except for a few illustrative cases, our analysis will focus on these cases, mainly for concreteness. In our discussion from there on, statistical significance implies a 5 % level. Tipping point estimates are reported in Tables 4, 5, and 6.

From Tables 2 and 3, we see that a statistically insignificant β_2 occurs quite often with both emission series. As argued above, despite no clear consensus, a linear EKC is not necessarily at odds with the current literature. Problems with the Delta method for inference on the tipping point would occur if the true β_2 is zero, so a significant β_2 does not necessarily guarantee identification. We nevertheless view these results as a motivation in support of the Fieller method whose accuracy does not depend on a non-zero β_2. Indeed, unbounded confidence sets are quite prevalent in Tables 4, 5, and 6, which confirm that the tipping point is indeed hard to pin down from available data.

Another point worth emphasizing concerns the heterogeneity of results across regions, with all estimation methods and both emissions data. Our disaggregate estimation is thus more meaningful than the full sample case, which we nevertheless report for completion and possibly for comparison with available literature. Our discussion will thus focus on our regional estimates. Confidence sets in what follows, unless indicated otherwise, are at the 5 % level.

4.1 Methodological Pathology

A few methodological comments emerge from Tables 4, 5, and 6 that are worth pointing out, given that to the best of our knowledge, identification problems have not been formally discussed in this literature.

1. Conforming with econometric theory, the Fieller and Delta method provide comparable confidence bands when the Fieller set is bounded and tight [as in e.g. Table 6 for the OECD], suggesting strong identification. In this case, the Fieller sets are wider to some extent yet they convey conformable economic content.
2. When the Fieller sets are unbounded and/or very wide suggesting weak identification (which occurs most prominently but not exclusively when a linear curve cannot be refuted) then the Delta and Fieller sets can be very different and imply very different economic conclusions. For example, they may provide conflicting evidence regarding the statistical significance of the tipping point which may be tested [given the duality between the confidence intervals and Wald tests] by checking whether the reported sets cover zero. Examples of such a conflict include the case of Asia with Carbon and the 2SLS method,

Table 1 Summary statistics—emissions data

Variable	Observations	Percentile	
SO_2 per capita	5,385	5	0.0004569
		25	0.0020361
		50	0.005487
		75	0.020822
		95	0.082269
	5,385	Mean	SD
		0.0229701	0.073423
Variable	Observations	Percentile	Value
CO_2 per capita	7,117	5	0.05249
		25	0.31365
		50	1.2358
		75	5.5888
		95	13.4872
	7,117	Mean	SD
		3.765413	6.034905

Table 2 Carbon emissions equation

	All	OECD	Asia	SS-Africa	M. East	S. America	C. America
2SLS							
GDP	0.550	**1.262**	0.427	0.336	0.883	0.497	−0.007
	(0.00)	(**0.00**)	(0.00)	(0.00)	(0.00)	(0.00)	(0.95)
GDP^2	0.002	**−0.113**	0.065	0.128	0.011	−0.037	0.362
	(0.633)	(**0.00**)	(0.00)	(0.00)	(0.47)	(0.52)	(0.00)
CIE	0.349	0.352	0.434	1.152	0.085	0.093	−1.134
	(0.00)	(0.00)	(0.00)	(0.00)	(0.24)	(0.15)	(0.00)
EFF	0.755	0.698	1.084	0.432	0.533	0.795	2.587
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
IND	0.123	0.237	0.337	0.244	−0.035	0.176	0.809
	(0.00)	(0.00)	(0.00)	(0.00)	(0.70)	(0.01)	(0.00)
DLSDV							
GDP	0.208	**0.347**	0.114	0.251	0.170	0.049	0.313
	(0.00)	(**0.00**)	(0.00)	(0.02)	(0.00)	(0.58)	(0.00)
GDP^2	0.002	**−0.039**	0.014	0.044	0.017	0.033	0.092
	(0.69)	(**0.00**)	(0.01)	(0.23)	(0.23)	(0.36)	(0.01)
CIE	0.182	0.083	0.090	0.597	0.197	0.050	0.136
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.08)	(0.00)
EFF	0.295	0.148	0.320	0.304	0.019	0.240	0.365
	(0.00)	(0.00)	(0.00)	(0.00)	(0.95)	(0.00)	(0.00)
IND	0.046	−0.005	0.078	0.033	−0.040	0.048	0.050
	(0.02)	(0.82)	(0.04)	(0.56)	(0.45)	(0.34)	(0.28)

Table 2 continued

	All	OECD	Asia	SS-Africa	M. East	S. America	C. America
DFE (A)							
GDP	**0.934**	**2.837**	1.097	1.020	0.758	1.033	1.496
	(**0.00**)	(**0.00**)	(0.00)	(0.011)	(0.00)	(0.04)	(0.00)
GDP^2	**−0.115**	**−0.530**	−0.045	0.331	−0.044	−0.086	−0.139
	(**0.00**)	(**0.00**)	(0.59)	(0.13)	(0.44)	(0.69)	(0.41)
DFE (B)							
GDP	0.619	**1.645**	0.494	0.379	0.758	0.532	0.356
	(0.00)	(**0.00**)	(0.00)	(0.014)	(0.00)	(0.18)	(0.18)
GDP^2	−0.007	**−0.191**	0.047	0.070	−0.048	0.049	0.199
	(0.66)	(**0.00**)	(0.14)	(0.38)	(0.38)	(0.77)	(0.04)
CIE	0.331	0.30	0.270	0.885	0.307	0.121	0.165
	(0.00)	(0.03)	(0.02)	(0.00)	(0.00)	(0.36)	(0.09)
EFF	0.768	0.913	1.047	0.457	2.847	0.741	0.830
	(0.00)	(0.00)	(0.00)	(0.00)	(0.08)	(0.00)	(0.00)
IND	0.154	0.403	0.594	0.049	−0.307	0.034	0.028
	(0.02)	(0.02)	(0.00)	(0.66)	(0.16)	(0.86)	(0.79)

2SLS: Baltagi and Li (1992); Eq. (2.1), with time dummies; GDP, GDP^2 , CIE instrumented using first lags. DLSDV: Kiviet (1995); Eq. (2.5) with time dummies and $\rho_j = 0, j = 1, \ldots 5$. DFE: Pesaran et al. (1999); Eq. (2.7) with $\beta_j = 0, j = 3, \ldots 5$ (Case A) and relaxing the latter constraints (case B). In bold: β_1 and β_2 significant at 5 % with $\beta_1 > 0$ and $\beta_2 < 0$

Table 3 Sulphur emissions equation

	All	OECD	Asia	SS-Africa	M. East	S. America	C. America
2SLS							
GDP	0.819	1.062	0.784	−0.723	2.184	0.252	−3.046
	(0.00)	(0.01)	(0.00)	(0.03)	(0.00)	(0.18)	(0.00)
GDP^2	−0.054	−0.327	0.016	0.038	0.286	0.305	1.537
	(0.06)	(0.14)	(0.66)	(0.01)	(0.00)	(0.46)	(0.00)
CIE	−0.099	1.829	0.184	−1.056	−1.902	−0.213	0.766
	(0.43)	(0.00)	(0.13)	(0.78)	(0.00)	(0.07)	(0.32)
EFF	0.449	−0.846	0.719	1.772	1.184	0.850	2.027
	(0.04)	(0.13)	(0.00)	(0.00)	(0.50)	(0.00)	(0.00)
IND	0.052	0.190	0.668	3.556	−1.006	0.583	−0.469
	(0.73)	(0.65)	(0.01)	(0.00)	(0.03)	(0.011)	(0.00)
DLSDV							
GDP	0.231	**0.621**	0.244	−0.096	0.270	−0.096	−1.998
	(0.00)	(**0.03**)	(0.011)	(0.66)	(0.37)	(0.62)	(0.02)
GDP^2	−0.021	**−0.127**	0.007	−0.048	−0.044	0.058	0.980
	(0.14)	(**0.03**)	(0.72)	(0.56)	(0.44)	(0.54)	(0.00)
CIE	−0.041	0.180	−0.002	0.174	−0.131	0.008	0.063
	(0.37)	(0.05)	(0.96)	(0.08)	(0.21)	(0.89)	(0.74)

Table 3 continued

	All	OECD	Asia	SS-Africa	M. East	S. America	C. America
EFF	0.053	−0.187	0.274	0.374	−0.374	0.177	0.604
	(0.60)	(0.33)	(0.02)	(0.14)	(0.83)	(0.28)	(0.28)
IND	0.060	−0.101	−0.041	0.117	0.035	0.070	0.209
	(0.41)	(0.60)	(0.77)	(0.42)	(0.89)	(0.58)	(0.51)
DFE (A)							
GDP	**0.825**	**3.115**	0.513	**1.194**	0.934	−0.423	−2.196
	(0.00)	**(0.00)**	(0.03)	**(0.00)**	(0.03)	(0.68)	(0.03)
GDP^2	**−0.155**	**−0.666**	−0.118	**−0.313**	−0.172	0.574	1.209
	(0.00)	**(0.00)**	(0.21)	**(0.04)**	(0.06)	(0.26)	(0.01)
DFE (B)							
GDP	**0.564**	**3.092**	0.169	−0.065	1.256	−1.199	−2.073
	(0.00)	**(0.00)**	(0.41)	(0.88)	(0.02)	(0.21)	(0.06)
GDP^2	**−0.081**	**−0.627**	−0.041	−0.088	−0.170	0.850	1.037
	(0.05)	**(0.00)**	(0.59)	(0.53)	(0.06)	(0.05)	(0.02)
CIE	−0.065	1.058	0.114	−0.054	−0.620	0.170	0.370
	(0.57)	(0.00)	(0.57)	(0.80)	(0.00)	(0.59)	(0.39)
EFF	0.896	−0.680	1.095	1.861	−4.030	1.973	0.456
	(0.00)	(0.18)	(0.01)	(0.00)	(0.20)	(0.04)	(0.63)
IND	0.193	−0.301	0.182	−0.011	0.244	−0.327	0.785
	(0.32)	(0.52)	(0.073)	(0.97)	(0.52)	(0.68)	(0.14)

For definitions, see notes to Table 2

Table 4 Set estimates for the tipping point using panel 2SLS

Region	Tipping point	Delta method	Fieller method
Carbon dioxide			
All	$2.6E+21$	$(-2.2E+23, 2.2E+23)$	$(-\infty, 0) \cup (1.04E+08, \infty)$
OECD	95.76	(46.687, 144.835)	(61.35, 176.06)
Asia	.039	(−0.011, 0.091)	(0.006, 0.107)
SS-Africa	.269	(0.054, 0.485)	(0.061, 0.477)
M. East	$5.52E-18$	$(-6E-16, 6E-16)$	$(-\infty, 0.0001) \cup (8E+10, \infty)$
S. America	797.92	(−12,965.8, 14,561.6)	$(-\infty, 0.211) \cup (11.17, \infty)$
C. America	1.009	(0.729, 1.29)	(0.708, 1.279)
Sulphur			
All	1,854.92	(−10,137, 13,846.8)	$(-\infty, 0) \cup (73.40, \infty)$
OECD	25.86	(−27.92, 79.64)	$(-\infty, 0.07) \cup (9.49, \infty)$
Asia	$8.02E+10$	$(0, 8.073E+12)$	$(-\infty, 0.0004) \cup (78.90, \infty)$
SS-Africa	$8.02E-05$	(−0.005, 0.0054)	$(-\infty, 0.44) \cup (3.16, \infty)$
M. East	45.22	(−11.62, 102.06)	(19.82, 824.63)
S. America	0.66	(−0.32, 1.647)	$(-\infty, 1.47) \cup (1.21E+08, \infty)$
C. America	2.69	(2.489, 2.899)	(2.49, 2.91)

Estimating Eq. (2.1), with time dummies. Method: error component 2SLS from Baltagi and Li (1992). *GDP*, *GDP^2*, *CIE* are instrumented using the first lag of each. All confidence sets are at the 5 % level

Table 5 Set estimates for the tipping point using dynamic bias-corrected LSDV

Region	Tipping point	Delta method	Fieller method
Carbon dioxide			
All	$1.45E + 21$	$(-127.35, 2.57E + 23)$	$(-\infty, 0) \cup (46, 852, \infty)$
OECD	56.924	(2.613, 138.23)	(19.47, 814.4)
Asia	0.036	$(-7.44, 0.184)$	$(-\infty, 0.336) \cup (9E + 9, \infty)$
SS-Africa	0.059	$(-9.93, 0.481)$	$(-\infty, 1.02) \cup (29.66, \infty)$
M. East	0.008	$(-20.61, 0.146)$	$(-\infty, 1.07) \cup (149.1, \infty)$
S. America	0.765	$(-4.722, 4.178)$	$(-\infty, \infty)$
C. America	0.199	$(-3.80, 0.637)$	(0.00004, 0.724)
Sulphur			
All	223.19	$(-1{,}100.2, 1{,}546.6)$	$(-\infty, 6.69E - 05) \cup (9.34, \infty)$
OECD	11.44	$(-4.42, 27.29)$	(1.81, 10,390.11)
Asia	$1.12E - 08$	$(-1.1E - 6, 1.13E - 06)$	(0.14, 11.26)
SS-Africa	0.36	$(-1.98, 2.71)$	$(-\infty, \infty)$
M. East	21.68	$(-52.1, 95.49)$	$(-\infty, \infty)$
S. America	2.29	$(-2.23, 6.81)$	$(-\infty, \infty)$
C. America	2.77	(1.42, 4.11)	(1.36, 4.40)

Estimating Eq. (2.5) with time dummies and $\rho_j = 0, j = 1, \ldots 5$. Relaxing the latter constraints increases uncertainty with both emission series. Method: bias-corrected LSDV with bootstrap standard errors from Kiviet (1995) and Bruno (2005). All confidence sets are at the 5 % level

the case of Central America with Carbon and the LSDV method, and the noteworthy case of the OECD with Sulphur and the LSDV method. In the latter case, the Delta confidence set is tight and covers zero, whereas the Fieller set although very wide excludes zero. Since β_1 and β_2 are significant at the 5 % level and both are correctly signed in this case, results with the Delta method with regards to the tipping point seem puzzling. In contrast, the Fieller method reveals that estimation uncertainty is severe in this case, which undermines the usefulness of the estimated curve with this method and the Sulphur series.

3. Other "pathological" results include cases for which the Delta method based sets are very tight [examples occur more prominently in Table 4] while their Fieller counterparts are unbounded. Econometric theory suggests that such cases illustrate [again, on recalling the duality between confidence intervals and Wald tests] severe spurious rejections with standard methods that do not cater for weak identification. In other words, econometric theory suggests that identification concerns conveyed via unbounded Fieller sets implies that the Delta method interval may be tightly centered on "wrong" values.[14]
4. Our non-parametric intervals for the turning point (see Sect. 4.3 for further discussion) are not at odds with their tightest parametric counterparts which occur here with reference to (2.7). The former are nevertheless narrower than the latter, even when the estimated shape does not disprove the quadratic model. One of the criticisms routinely advanced against non-parametric estimation is that its limited-information foundation may overstate estimation uncertainty relative to a parametric setting when the latter does not lack credibility. We do not find support for such warnings here. Recall that the sets

[14] Indeed, the above cited econometric literature provides many convincing simulation studies documenting this problem with standard Wald-type tests.

Table 6 Set estimates for the tipping point using long-run dynamic fixed effects

Region	Tipping point	Delta method	Fieller method
Carbon, Case A (no long-run controls)			
All	52.34	$(-8.69, 113.4)$	$(21.34, 296.5)$
OECD	17.22	$(9.75, 24.67)$	$(11.78, 33.45)$
Asia	227.3	$(-1{,}553.5, 2{,}008.1)$	$(-\infty, 0) \cup (7.44, \infty)$
SS-Africa	0.214	$(-0.332, 0.761)$	$(-\infty, 0.786) \cup (421.4, \infty)$
M. East	2,620.5	$(-34{,}028, 39{,}269)$	$(-\infty, 0.08) \cup (26.25, \infty)$
S. America	391.96	$(-9{,}220.7, 10{,}004.5)$	$(-\infty, 0.91) \cup (5.83, \infty)$
C. America	218.96	$(-2{,}054.9, 2{,}492.8)$	$(-\infty, 0.13) \cup (9.45, \infty)$
Carbon, Case B (with long-run controls)			
All	53.33	$(-10.9, 117.6)$	$(21.25, 330.1)$
OECD	16.59	$(10.69, 22.48)$	$(12.05, 26.89)$
Asia	36,605	$(-925{,}231, 998{,}443)$	$(-\infty, 0.0005) \cup (15.53, \infty)$
SS-Africa	0.266	$(-0.264, 0.796)$	$(-\infty, .8) \cup (675, 938, \infty)$
M. East	66.49	$(-136.2, 269.2)$	$(-\infty, 0) \cup (12.17, \infty)$
S. America	12.87	$(-15.95, 41.71)$	$(-\infty, 0.11) \cup (4.84, \infty)$
C. America	10.39	$(-5.67, 26.47)$	$(-\infty, 0) \cup (4.54, \infty)$
Sulphur, Case A (no long-run controls)			
All	14.27	$(-0.15, 28.69)$	$(6.42, 73.43)$
OECD	10.38	$(7.35, 13.4)$	$(7.84, 14.59)$
Asia	8.80	$(-31.59, 49.2)$	$(-\infty, 0.008) \cup (1.10, \infty)$
SS-Africa	6.74	$(-6.34, 19.83)$	$(2.04, 1.61E+22)$
M. East	15.09	$(-9.64, 39.82)$	$(-\infty, 8.39E-07) \cup (2.41, \infty)$
S. America	1.45	$(-0.43, 3.32)$	$(-\infty, \infty)$
C. America	2.48	$(1.41, 3.54)$	$(1.25, 4.33)$
Sulphur, Case B (with long-run controls)			
All	33.16	$(-54.95, 121.26)$	$(-\infty, 0) \cup (6.89, \infty)$
OECD	11.77	$(7.22, 16.31)$	$(8.46, 20.29)$
Asia	7.67	$(-71.68, 87)$	$(-\infty, \infty)$
SS-Africa	0.69	$(-3, 4.39)$	$(-\infty, \infty)$
M. East	40.05	$(-43.8, 123.89)$	$(-\infty, 0) \cup (6.84, \infty)$
S. America	2.03	$(0.63, 3.41)$	$(0, 7.01)$
C. America	2.72	$(1.21, 4.22)$	$(0.87, 6.28)$

Estimating Eq. (2.7) with $b_j = 0, j = 3, \ldots 5$ (Case A) and relaxing the latter constraints (Case B). Method: dynamic fixed effects applied to the error correction form (2.7), from Pesaran et al. (1999). All confidence sets are at the 5 % level

we back-out, although based on Ma and Racine (2013), have not formally been shown to control coverage for inference on zero-derivative sets. Our proposed application of Ma and Racine (2013) bands is also new to the literature. Analyzing its coverage rates is a worthy further research objective.

5. Our non-parametric 2SLS estimation did not produce notable differences relative to Least Squares, so Figs. 1, 2, 3, 4, 5, 6, 7, 8, 9, and 10 and related discussions are based on the latter. Whether this suggest that instruments are weak is unclear, as weak-instruments robust counterparts to Ma and Racine (2013) work is unavailable.

Table 7 Results focusing on Europe

	Tipping point	Delta method	Fieller method
CO_2			
Panel 2SLS	106.38	(13.42, 199.35)	(53.25, 355.08)
Dynamic bias corrected LSDV	83.69	(−14.77, 182.15)	(32.49, 445.03)
Dynamic fixed effects—with long run controlls	50.81	(−0.12, 101.74)	(25.65, 436.67)
Dynamic fixed effects—no long run controlls	13.67	(8.31, 19.04)	(9.28, 26.38)
SO_2			
Panel 2SLS	7.64	(5.29, 9.98)	(5.81, 12.01)
LSDV	21.52	(−2.11, 69.16)	(3.38, 11,703.3)
Dynamic fixed effects—with long run controls	14.43	(5.11, 23.74)	(8.81, 55.62)
Dynamic fixed effects—no long run controls	12.5	(6.08, 18.91)	(7.84, 29.29)

Refer to Tables 2, 3, 4, 5, and 6 for the definition of estimation methods. European countries are selected out of the OECD list reported in the Appendix for each emission series

6. While comparing results with (2.7) versus its parametric deterministic trend based counterparts, recall that the latter impose cross sectionally constant time effects.

4.2 Findings and Outlook: The Parametric Case

Tables 4, 5, and 6 suggest further substantive conclusions. When referring to the "existence" of the EKC, a broad definition that prevails in the literature entails the following: emission levels initially rise with per capita income but then eventually fall as per capita income exceeds some threshold level. Viewed collectively, our results suggest that conforming with this definition, the estimated β_1 and β_2 are significant at the 5 % level and both are correctly signed mainly in the OECD region. This conclusion while not at odds with the literature needs to be qualified, when interpreting results on the tipping point estimates. Except with the long-run dynamic fixed effects method applied to the OECD region, estimates of the tipping points are either extremely imprecise (practically uninformative), or suggest economically implausible values. Although quite wide, the Delta method does not convey how seriously uninformative these sets truly are.

Consider for example the case of Carbon with the 2SLS estimate form Table 4, in the OECD region. In this case, both estimation methods support an inverted-U curve, yet the confidence intervals suggest a lower bound of at least 46.687, which is disconcerting given our measure of per capita income in thousands of constant 2000 USD. It may be argued that from a purely statistical perspective, both set estimates are not too wide, indicating that δ can be pinned down with enough precision. From an economic perspective, these estimates are much too high to reconcile with meaningful useful theory or policy. It is interesting to note that using Sulphur for this same region and this same method rejects the EKC form, which is reflected via highly imprecise estimates of the tipping point. Although wide, the Delta method based bands understate the severity of estimation uncertainty in this case. With the bias-corrected LSDV method, we find support for the curve with both emission series for the OECD countries. Yet the estimate uncertainty regarding the tipping point is much more pronounced than with the 2SLS method, so for all practical purposes, LSDV-based confidence intervals are non-informative.

On balance, results via our long-run approach in Table 6 for the OECD are informative and consistent with EKC predictions. Confidence bands suggest, in addition to statistical precision, turning points that are economically reasonable given our measurement scale for GDP. These results may be attributed to various methodological considerations. First, it matters importantly to account for dynamics in estimating the EKC. Second, avoiding methods that are not designed for fixed n is commendable. The bias-corrected LSDV method is in principle applicable, yet the bias-correction assumes strictly exogenous regressors. The pooled long-run inference methods are designed for fixed n and large T. "How large is large" is of course a usual question with annual data. The fact remains that fixed n-and-T panel data methods are unavailable to date, so given the emissions series at hand and the importance of a regional analysis, one may argue that dynamic fixed effects are, among available methods, best suited for our purpose. Perhaps more importantly, in contrast to other cointegration methods, dynamic fixed effects do not require one to take a stand regarding the I(1) properties of regressors. Given available mixed results in this literature, this is worth pointing out. Of course this presumes that the considered long-run relations are stable and that estimations with further lags (to control for potential endogeneity of regressors) provide conformable results. Our results for the OECD region do not seem to refute these assumptions.

It is worth noting that our estimated turning points are generally lower with SO_2 than with CO_2. This suggests that results with local pollutants may be more relevant from a policy perspective. Since European countries share some common regulations with regards to local pollutants, we revisit our analysis of the OECD countries with focus on Europe. Results reported in Table 7 support our main message: policy-relevant estimates of the tipping point are recovered via a dynamic long-run econometric perspective. From a technical perspective and comparing Table 7 to the OECD results from Table 6, note that a decreased sample size costs statistical precision with the CO_2 data. With this series, we find sizable differences in confidence bands when including and excluding the long run control variables. Interestingly, the SO_2 case is more stable, which supports our reliance on local pollutants in analyzing this sub-sample. This also leads us to revisit the Central America results, since a local pollutant argument may be relevant for this sub-sample with SO_2 data. Indeed, Table 6 suggests evidence in favour of an EKC with reasonable tipping points in this case as well.

Although consensus on chosen controls is rare in empirical work, one of our findings in this regard seems new to this literature: if we compare set estimates of δ in (2.7) imposing and relaxing $\beta_3 = \beta_4 = \beta_5 = 0$ [with short run coefficients $\rho_j \neq 0$] we find more precise set estimates when long-run controls are suppressed. This suggests that controls though statistically relevant for short run adjustments, are not required for the postulated relation between emissions and income to be stable in the long-run.

A key tricky ingredient remains regarding the role of technology. Our panel approach, except for the non-deterministic trend based (2.7) which is incompatible with time dummies, follows the usual practice of introducing the latter to capture technical progress. Such an interpretation has to be qualified despite our regional analysis since time dummies are cross sectionally constant. In this respect, results with (2.7) may prove informative, if they differ from their deterministic counterparts. And we do find important differences in precision as emphasized above. We do not claim that ruling out estimation uncertainty on δ evacuates the deep interpretation issues arising from subsample heterogeneities, nor we do argue in favour of stochastic trends in general. Empirically, the differences in precision we find are new to this literature and reinforce existing results on the importance of unobservable heterogeneity.

4.3 Discussion and Perspective: The Non Parametric Checks

Our non-parametric analysis is summarized via Figs. 1, 2, 3, 4, 5, 6, 7, 8, 9, and 10. These report partial regression surfaces for CO_2 and SO_2 for the OECD countries, for the full as well as subset time periods. On the whole, with non-OECD countries,[15] observed best fit curves deviate arbitrarily and dramatically from an expected EKC. Even if a formal statistical test is not intended, such inconsistencies [between the postulated parametric quadratic form and its non-parametric best-fit counterpart] may justify—at least in part—the severe uncertainties we find via parametric estimates of the tipping point. We thus focus on the OECD countries for further analysis.

With CO_2, we fail to confirm an inverted-U shape from a full and subsample perspective. This is an important sense in which our analysis can be seen as an exploration of the pervasiveness of shape assumptions. Figures 1, 2, 3, 4, and 5 suggest that past 1972, linearity is not necessarily incompatible with CO_2 data. This suggests one possible reason for why we obtain wider parametric confidence sets with CO_2, since as argued above, imposing a quadratic curve weakly identifies the tipping point when the curve is more or less linear. More subtle arguments can be raised that question structural stability, for both emission series. Although formal tests are not conducted, Figs. 1, 2, 3, 4, 5, 6, 7, 8, 9, and 10 reveal noticeable shape difference across subperiods for all covariates (that is, not just for GDP), enough to suggest that time effects that are often considered with Panel data models may just not depict the whole story. Admittedly, the observed inconsistencies across subsamples may be linked to the usual culprits, that is, they may have more to do with unsuitable subsidiary assumptions (e.g. stationarity, exogeneity) than with the structure of the EKC itself. Results remain noteworthy, in the sense that they suggest a more critical assessment of technological effects.

In contrast, and although inconstancies across sub-periods cannot be ruled out, our non-parametric results with SO_2 allow us to formulate realistic conclusions on functional shape, and to obtain precise predictions for the tipping point that are not at odds with EKC principles. Indeed, and more accurately since the mid-eighties which possibly reflects policy harmonizations, non-parametric curves for the OECD countries are globally in line with our parametric results. While asymmetry cannot be ruled out, it does not seem pathological; we thus calculated the above described confidence set for the tipping for SO_2 in the OECD global sample. We obtain 10.83 as a point estimate, and (9.07, 11.71) as a confidence band. Focusing on Europe produces 14.77 with (12.38, 15.91) as bands. These sets compare favorably with counterparts based on (2.7) which emerged as the tightest within our parametric estimates. Since econometric foundations of the former have yet to be confirmed, we do not take a stand on their relative worth. While the non-parametric sets are narrower, both parametric and non-parametric sets produce informative results and precise enough conclusions regarding an EKC with SO_2 for OECD countries.

To conclude, the partial control surfaces (2.10–2.12) reveal intriguing information. With CO_2 data, the curve with the industry/GDP ratio control IND_{it} [reported in Figs. 1, 2, 3, 4, 5, 6, 7, 8, 9, and 10 under the heading "Industry"], displays, as expected, an upward slope as of 1973. A flat curve before the first oil shock may be attributed to world-wide inexpensive oil. The partial surfaces pertaining to the CO_2 intensity of energy CIE_{it} [reported under the heading "CO_2 intensity"] is rather flat, except in the 1960–73 subsample in which case the expect upward slope is confirmed. The curve with the fossil-fuel content of energy EFF_{it} [reported under the heading "Fossil"] is also rather flat, except in the 1973–1985 period, where an upward slope is suggested. While an upward slope is expected for CIE_{it} and EFF_{it}, somewhat inelastic partial curves are not incompatible with a more or less linear EKC as observed with CO_2 , with suggests that emissions do not seem to level-up as GDP increases. As a matter of fact, results differ with SO_2, as the partial surfaces pertaining

[15] Results are not reported for space considerations but are available upon request.

to CIE_{it} do slope upward except for the most recent subsample. With EFF_{it} and SO_2 emissions, a downward slope is suggested after the first oil shock, and more markedly from the mid-eighties and onward. This may be attributed to shifts from dirty to cleaner coal, and from coal to natural gas as well as to the important effect of regulations and emission exchange markets. The IND_{it} control, post 1973, slopes as expected, although rather non-linearly. As economies adjusted to the first oil shock, energy-consuming industries relocated towards non-oil energy rich countries. For some countries, this translated into substantially more polluting energy use, until technology and regulations gained momentum. Non-linearity in slope may contribute to reinforcing the EKC we observe with SO_2 for the OECD countries.

5 Conclusion

Despite some overemphasis on methodology in recent works, important advances in econometrics have made empirical work on the EKC seem more credible than it was in the early nineties. Our contributions to estimating the EKC focus on the precision of the tipping point estimate, under various assumptions regarding endogeneity and persistence, and functional form. Taken collectively, our results suggest that except from a local-pollutant long-run or non-parametric perspective, confidence sets around the tipping point are sufficiently wide that the policy relevance of the EKC is greatly undermined even in the OECD. From a constructive perspective, we view these results as a motivation for further work aiming to improve identification of the curve, and for finite sample motivated panel data methods.

The fact that a long-run approach holds promise—although noteworthy—should not be viewed as evidence in favour of a cointegration approach to the EKC. In the same vein, our non-parametric estimations—although informative—are not intended to disqualify parametric estimations (recall that as considered, the former are not necessarily less restrictive than the latter). Rather, our main conclusion is that regardless of the statistical assumptions one is comfortable maintaining in this context, interpreting the shape of the curve should not be the whole story. We should and do ask whether data supports a plausible tipping point. To do so, methods that account for a weakly identified tipping point should be preferred, because of the nature of the problem under study. Indeed, if the question taken to the data is whether a non-linear effect is present, then methods that impose the linear case away—which causes weak identification—cannot be adequate.

Our companion non-parametric analysis is viewed as illustrative in various respects. Results are nevertheless informative. As non-parametric methods gain popularity in EKC contexts, a convincing inferential perspective in still lacking. Again, we view our results as a motivation for further work aiming to minimize the effects of subsidiary modelling assumptions for inference on tipping points.

6 Appendix 1: List of Countries

6.1 Countries Used for the CO_2 Equation

OECD.[16] (27 countries). Albania, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Hong Kong, Hungary, Iceland, Ireland, Italy, Japan, Malta, Netherlands,

[16] The list of OECD countries includes countries that have been in the OECD for the majority of the time frame of this study, with the exceptions of Albania and South Korea. The latter two are included because, in our judgement, are anomalies with respect to their geographic peers and Albania is included because this group corresponded closest to its characteristics.

New Zealand, Norway, Portugal, South Korea, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States.

Asia. (17 countries) Bangladesh, China, India, Indonesia, Kazakhstan, Kyrgyzstan, Malaysia, Mongolia, Pakistan, The Philippines, Singapore, Sri Lanka, Tajikistan, Thailand, Turkmenistan, Uzbekistan, Vietnam.

Sub-Saharan Africa. (16 countries) Angola, Benin, Botswana, Cameroon, Congo, Cote d'Ivoire, Gabon, Ghana, Kenya, Namibia, Nigeria, Senegal, South Africa, Togo, Zambia, Zimbabwe.

The Middle East & North Africa. (16 countries) Algeria, Bahrain, Egypt, Eritrea, Iran, Jordan, Kuwait, Lebanon, Morocco, Oman, Saudi Arabia, Sudan, Syria, Tunisia, United Arab Emirates, Yemen.

South America. (11 countries) Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Guatemala, Paraguay, Peru, Uruguay, Venezuela.

Central America & The Caribbean. (10 countries). Costa Rica, Dominican Republic, El Salvador, Haiti, Honduras, Jamaica, Mexico, Nicaragua, Panama, Trinidad & Tobago.

Other. (17 countries) Armenia, Azerbaijan, Belarus, Bulgaria, Croatia, Czech Republic, Georgia, Latvia, Lithuania, Macedonia, Moldova, Poland, Romania, Russia, Slovakia, Slovenia, Ukraine.

6.2 Countries Used for the SO_2 Equation

OECD. (27 countries). Albania, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Hong Kong, Hungary, Iceland, Ireland, Italy, Japan, Malta, Netherlands, New Zealand, Norway, Portugal, South Korea, Spain, Sweden, Switzerland, Turkey, United Kingdom, United States.

Asia. (12 countries). Bangladesh, China, India, Indonesia, Malaysia, Mongolia, Pakistan, Philippines, Singapore, Sri Lanka, Thailand, Vietnam.

Sub-Saharan Africa. (11 countries). Botswana, Cameroon, Cote d'Ivoire, Gabon, Ghana, Kenya, Senegal, South Africa, Togo, Zambia, Zimbabwe.

The Middle East & North Africa. (13 countries) Algeria, Bahrain, Egypt, Iran, Jordan, Kuwait, Morocco, Oman, Saudi Arabia, Sudan, Syria, Tunisia, United Arab Emirates.

South America. (10 countries). Argentina, Bolivia, Brazil, Chile, Colombia, Guatemala, Paraguay, Peru, Uruguay, Venezuela.

Central America & The Caribbean. (7 countries). Costa Rica, Dominican Republic, El Salvador, Honduras, Mexico, Panama, Trinidad & Tobago.

Other. (2 countries). Bulgaria, Romania.

7 Appendix 2: The Fieller Solution

The Fieller method requires solving inequality (3.5) for d_0, which may be re-expressed as

$$A d_0^2 + 2B d_0 + C \leq 0 \tag{7.1}$$

$$A = \hat{g}_2^2 - z_{\alpha/2}^2 \hat{v}_2, \quad B = -\hat{g}_1 \hat{g}_2 + z_{\alpha/2}^2 \hat{v}_{12}, \quad C = \hat{g}_1^2 - z_{\alpha/2}^2 \hat{v}_1. \tag{7.2}$$

Except for a set of measure zero, $A \neq 0$. Similarly, except for a set of measure zero, $\Delta = B^2 - AC \neq 0$. Real roots equal to $\frac{-B \pm \sqrt{\Delta}}{A}$ exist if and only if $\Delta > 0$. Let d_{01} refer to the smaller root and d_{02} to the larger root, then

$$FCS(d;\alpha) = \begin{cases} [d_{01}, \quad d_{02}] & if \;\; A > 0 \\ ]-\infty, \quad d_{01}] \cup [d_{02}, \quad +\infty[& if \;\; A < 0 \end{cases}. \tag{7.3}$$

Bolduc et al. (2010) further show that: (i) if $\Delta < 0$, then $A < 0$ and $FCS(d;\alpha) = \mathbb{R}$; (ii) $FCS(d;\alpha)$ contains the point estimate $\hat{g}_1/\hat{g}_2$ and thus cannot be empty, and (iii) asymptotically, Fieller's solution and the *Delta* method give similar results when the former leads to an interval, *i.e.* when the denominator is far from zero. Taking the exponential of the limits of $FCS(d;\alpha)$ provides a confidence set for $\exp(d)$.

8 Appendix 3: B-splines

The method from Ma and Racine (2013) uses a B-spline function for $f(.)$, which is a linear combination of B-splines of degree m defined as follows

$$\mathcal{B}(x) = \sum_{c=0}^{N+m} b_c B_{c,m}(x), \quad x \in [k_0, k_{N+1}]$$

where b_c are denoted "control points", $k_0, \ldots, k_{N+1}$ are known as a knot sequence [an individual term in this sequence is known as a knot],

$$B_{c,0}(x) = \begin{Bmatrix} 1 & k_c \le x < k_{c+1} \\ 0 & otherwise \end{Bmatrix}$$

which is referred to as the 'intercept', and

$$B_{c,j+1}(x) = a_{c,j+1}(x)B_{c,j}(x) + [1 - a_{c+1,j+1}(x)]B_{c+1,j}(x),$$
$$a_{c,j+1}(x) = \begin{Bmatrix} \frac{x-k_c}{k_{c+j}-k_c} & k_{c+j} \ne k_c \\ 0 & otherwise \end{Bmatrix}.$$

The unknown function $f(.)$ is estimated by least squares as

$$\hat{\mathcal{B}}(GDP_{it}; covariates_{it}) = argmin_{\mathcal{B}(.)} \sum_{i=1}^{n}\sum_{t=1}^{T} [EM_{it} - \mathcal{B}(covariates_{it})]^2 .$$

Explicitly, this requires the estimation of the control points b_c. If covariates are considered endogenous and instruments provided, 2SLS is also possible. Underlying best fit parameters are selected by cross-validation; see Ma et al. (2012) for further details. Further description of this R-package is available at: http://cran.r-project.org/web/packages/crs/crs.pdf.

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