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1 Is Radiative Forcing Cointegrated with Temperature?
2 A Further Examination Using a Structural Time Series Approach

3 March 2019

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10 **Abstract**

11 **Purpose**

12 This paper re-examines the long-run relationship between radiative forcing (including
13 emissions of carbon dioxide, sulphur oxides, methane and solar radiation) and temper-
14 atures from a structural time series modelling perspective. We assess whether forcing
15 measures are cointegrated with global temperatures using the structural time series
16 approach.

17 **Design/methodology/approach**

18 A Bayesian approach is used to obtain estimates that represent the uncertainty regarding
19 this relationship. Our estimated structural time series model enables alternative model
20 specifications to be consistently compared by evaluating model performance.

21 **Findings**

22 Our results confirm that cointegration between radiative forcing and temperatures are
23 consistent with the data. However, our results find less support for cointegration between
24 forcing and temperature data than found previously.

25 **Research limitations/implications**

26 Given considerable debate within the literature relating to the ‘best’ way to statistically
27 model this relationship and explain results arising as well as model performance, there is
28 uncertainty regarding our understanding of this relationship and resulting policy design
29 and implementation. There is a need for further modelling and use of more data.

30 **Practical implications**

31 There is divergence of views as to how best to statistically capture, explain and model
32 this relationship. Researchers should avoid being too strident in their claims about model
33 performance and better appreciate the role of uncertainty.

34 **Originality/value**

35 The results of this study make a contribution to the literature by employing a theoret-
36 ically motivated framework in which a number of plausible alternatives are considered
37 in detail, as opposed to simply employing a standard cointegration framework.

38 **Key Words:** Radiative forcing, cointegration, structural time series.

39

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35 1. Introduction

36 There is an emerging consensus that statistical evidence supports the relationship
37 between radiative forcing measures (e.g. carbon dioxide, sulphur oxides, methane, solar
38 radiation) and temperatures. In this paper we follow the established definition of radiative
39 forcing or forcing following, for exmaple, Kaufmann et al. (2010: 398) who define
40 this as “the forcing ... due to carbon dioxide, methane, CFC11 [chlorofluoro-carbons],
41 CFC12, nitrous oxide, sulfur emissions, and solar activity”. The particular statistical
42 framework used by Mills (2009), Kaufmann and Stern (2002) and Kaufmann et al.
43 (2006) is that of cointegration. Cointegration between two variables implies that the
44 variables have stochastic trends, but a linear combination between the variables exists
45 that has no stochastic trend. Equivalently, the existence of cointegration between two
46 variables implies that they share a common stochastic trend. Where one of the variables
47 is weakly exogenous, this variable may be causally responsible for the trend in the
48 other. Thus, the implication that radiative forcing is cointegrated with temperature
49 provides evidence consistent with some scientific models that imply forcing measures
50 play a possible positive causal role in relation to warming trends.

51 While the earlier work of Stern and Kaufmann (2000) employed structural time series
52 models, more recent research has employed conventional tests for cointegration, within
53 an autoregressive framework. Our aim is to re-examine whether forcing measures are
54 cointegrated with global temperatures using the structural time series approach. We do
55 not dispute the methodological rigour or specific findings of the studies above. On the
56 contrary, the previous finding of cointegration between temperatures and global warming
57 are easily replicated. However, there continues to be considerable interest in examining
58 the statistical properties of causal relationships between global temperatures and human
59 activity. For example, there is an ongoing debate as to whether global temperatures are
60 stationary or best represented by some other more complex process. Recent econometric
61 evidence on this is provide by Lai and Yoon (2018). There have also been studies that
62 suggest that the length of dataset in this context matters. For example, McMillana
63 and Wohar (2013) report a weak relationship between temperature and CO2 and no
64 statistically significant evidence of a trend when employing a much longer time series
65 of data. At the same time there are studies, for example, Stern and Kaufmann (2014),
66 that do report causal relationships between certain types of forcing (e.g. natural and
67 anthropogenic) and temperature change. Stern and Kaufmann (2014) arrive at these
68 conclusions by employing Granger causality tests as opposed to developing a time series
69 model of the relationship between the variables of interest. They argue that time series

70 models are dependent on assumptions regarding the time series properties of the data
71 and as is well documented in the literature there is far from a consensus of opinion on
72 this issue. This in part stems from the fact that it is hard (impossible) to know the
73 underlying data generating process which then makes model selection difficult. This
74 has, however, not stopped further developments in this context. For example, an
75 alternative approach to examining this issue is presented by Gallegati (2018) who use
76 wavelet analysis. This approach identifies that different data series can have different
77 time scales that is only partially resolved when employing cointegration analysis because
78 of how the methods deals with non-stationarity arising from stochastic trends. In ad-
79 dition, although our study is conducted at global level there are links with research
80 conducted at the country and regional levels. For example, time series data and meth-
81 ods have been employed in empirical studies of the Environmental Kuznets Curve (EKC)
82 by Cialani (2007) for Italy and Mohapatra and Giri (2009) for India. There is good rea-
83 son to think that such studies consider the type of advanced econometric methodology
84 employed in the current paper.

85 Another example of an econometric development applied to this topic is Chevillon
86 (2017) who employs a procedure that offers a robust test for the rank of cointegration
87 within a VAR that may have misspecified local linear trends. Using this approach it
88 is reported that temperature and greenhouse gases appear to be cointegrated. This
89 paper also provides an overview of the proceeding literature that once again illustrates
90 the ongoing debate regarding the extent to which statistical models can truly reveal the
91 relationships of interest.

92 However, we believe it is worth investigating how robust previous findings are to
93 alternative model specifications. These lead us to our main research question which is
94 investigating the presence or otherwise of a stable long run relationship between radia-
95 tive forcing and global temperature by employing classical and Bayesian methods, and
96 explicitly considering alternative model specifications, both cointegrated and non-coin-
97 tegrated, which is a departure from the prior literature and makes a contribution to the
98 literature. We do this in three ways.

99 First, we conduct cointegration tests introduced by Shin (1994) which adopt coin-
100 tegration as the null hypothesis rather than the alternative hypothesis as is the case
101 on other empirical work carried our so far. Tests that adopt a the presence of a unit
102 root or no-cointegration as the null hypothesis have commonly been found to obtain
103 different findings to tests that have a null hypothesis of stationarity or cointegration
104 respectively (see Maddala and Kim, 1998 Chap 4). Therefore, we believe it would be
105 useful to investigate whether Shin's (1994) approach supports previous findings concern-
106 ing cointegration.

107 Second, by explicitly estimating a structural time series model, alternative model
108 specifications (i.e. cointegrated and non-cointegrated) can be consistently compared by
109 evaluating model performance. Structural time series models are particularly useful for
110 this purpose as they can nest both cointegrated and non-cointegrated models as special
111 cases.

112 Third, we estimate structural time series models using Classical and Bayesian meth-
113 ods. This dual approach to estimating the structural time series model is revealing.
114 Whereas the Classical approach to estimation will be based on only one mode of the
115 likelihood, the Bayesian approach to inference can reflect multiple high density points.
116 As we will explain, there is strong evidence that the posterior density has a number of
117 high density points. This requires us to place important qualifications on the results we
118 report.

119 Overall, the results of our study make a contribution to the literature by employing a
120 theoretically motivated framework in which a number of plausible alternatives are con-
121 sidered in detail, as opposed to simply employing a standard cointegration framework.
122 Our research fits in well within the context of research such as Romero-Avila (2008) who
123 examines convergence within carbon dioxide emissions for 23 countries between 1960-
124 2002, Lee and Chang (2009) who investigate stochastic convergence of per capita carbon
125 dioxide emissions and multiple structural breaks for OECD countries, as well as Ajmi
126 et al. (2013) who study relationships between energy consumption and income for G7
127 countries using nonlinear causality tests. Marrero (2008) considers global greenhouse gas
128 emissions within his study of emissions, growth and energy usage mix for Europe. Anger
129 (2008) generalises this type of analysis by considering the economic impact of emissions
130 trading schemes and likely impact on emissions. Our research approach helps provide an
131 important backdrop to studies in related areas such as decomposition of carbon dioxide
132 emissions (Sun, 1999) and studies of non-CO₂ greenhouse gas emissions (e.g. Shukla
133 et al, 2006). Our research also links with studies outlining policies aimed at reducing
134 CO₂ emissions such as Gerlagh and Zwaan (2006), Lu et al. (2013) who consider CO₂
135 emission efficiency in OECD countries, as well as in developing countries facing issues
136 such as poverty alleviation and growth promotion (Van Heerden et al, 2006). Sam et al.
137 (2009) study the effectiveness of voluntary emissions programmes in the US. Our paper
138 extends work such as that on emission in the US and evidence on convergence patterns
139 for pollutants using unit root tests (see List, 1999).

140 Our paper proceeds by outlining the statistical models we employ in Section 2. Sec-
141 tion 3 describes our approach to model estimation. In Section 4 we briefly discuss the
142 data and present our empirical results. Section 5 provides a discussion of our results and
143 their implications. Finally, in Section 6 we offer conclusions.

144

2. Econometric Models

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As our preceding discussion suggests, there is considerable and ongoing debate with regard to choice of econometric models and use of an appropriate empirical strategy in order to study our main relationships of interest (e.g. Chevillon (2017), Lai and Yoon (2018), Stern and Kaufmann (2014), Gallegati (2018) and McMillana and Wohar (2013)). Based on our consideration of the empirical literature we believe that our results make a contribution to the literature due to our use of a theoretically motivated framework in which a number of plausible alternatives are considered in detail, as opposed to simply employing a basic cointegration framework, as outlined in detail below. As a result, we mainly limit our discussion to the structural time series approach. We also employ a standard vector autoregressive (VAR) approach, where cointegration is treated as the alternative hypothesis so as to ensure that, should our results radically differ from previous findings, this would be due to the modelling approach adopted and not driven by slight differences in the data employed in the analysis. For details on the VAR approach readers are referred to Johansen (1995).

The model introduced in Shin (1994) is of the structural time series form:

$$\begin{aligned} y_t &= \mu_t + \beta_t + x_t\alpha + e_t \\ \mu_t &= \mu_{t-1} + v_t \end{aligned} \quad (1)$$

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where y_t is temperature at time t , x_t is a $m \times 1$ vector of covariates (in this case radiative forcing) and e_t and v_t are stationary innovations that can be serially correlated. The model in equation (1) above contains a time trend (t), but this can be removed from the regression if there is no deterministic trend in the data generating process. If y_t and x_t are integrated of order 1 (see Johansen, 1995, p35), cointegration between y_t and x_t implies the variance of v_t is zero.

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The tests outlined in Shin (1994) do not require explicit estimation of the variance components within equation (1). A test for cointegration can be constructed by obtaining estimates of the long run variance of e_t (ie, ω_e) and then constructing the following test statistic

$$C = T^{-2} \sum_{t=1}^T S_t^2 / \omega_e \quad (2)$$

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where S_t is the estimate of the partial sum process $S_t = \sum_{i=1}^t e_i$. The distribution of this test statistic has been tabulated in Shin (1994), but it can be simulated using Monte Carlo methods.

173

The model in equation (1) can be generalised to allow for autoregressive components

174 with a local linear trend intercept as follows:

$$\begin{aligned}
 y_t &= \mu_t + \sum_{i=1}^p \gamma_i y_{t-i} + x_t' \alpha + e_t \\
 \mu_t &= \mu_{t-1} + \beta_t + v_t \\
 \beta_t &= \beta_{t-1} + w_t
 \end{aligned} \tag{3}$$

175 where y_t and x_t' are as defined above. Because the autoregressive components are as-
 176 sumed to "soak up" any serial correlation, e_t , v_t and w_t are assumed to be independent
 177 normal innovations.¹ The intercept in this model μ_t is able to evolve in a stochastic
 178 manner if either v_t or w_t have non-zero variances. The trend in the intercept at time t
 179 is β_t . Cointegration between y_t and x_t requires that both are non-stationary and that

$$\left| \sum_{i=1}^p \gamma_i \right| < 1 \text{ and } Var(v_t) = Var(w_t) = 0. \tag{4}$$

180 The unknowns within model (1) are of two types:

- 181 i) the 'latents' $\Gamma = (\{\mu_t\}, \{\beta_t\}, \{\gamma_i\}, \alpha)$ (along with the errors that can be con-
 182 structed given knowledge of these quantities); and
- 183 ii) the 'hyper parameters' $\Psi = (\sigma_e^2, \sigma_v^2, \sigma_w^2)$.

184 Additionally, there are initial conditions (priors) for the latents Γ_0 which are the
 185 prior mean and covariance for the value of the latents at $t = 0$.

186 3. Model Estimation

187 3.1. Classical Estimation

188 The test introduced by Shin (1994) only requires a standard ordinary least squares
 189 regression to estimate the null (cointegrated) model. Alternatively, an estimator that
 190 allows for serial correlation in the error and exogeneity can be employed, such as the Fully
 191 Modified (FM) estimator outlined in Phillips and Hansen (1991). The FM estimator is
 192 employed here, since it may yield less biased and more efficient estimates, and it also
 193 requires the component ω_e to be estimated. Therefore, the test statistic described in
 194 Section 2 only needs the additional construction of the partial sum component.

195 Classical estimation of the general model described by equation (3) can proceed in
 196 a number of ways. Harvey (1989) outlines Classical approaches in detail. For example,
 197 the 'time domain' approach outlined in Harvey (1989) employs the Kalman Filter, that
 198 enables the likelihood to be calculated using the prediction error decomposition. Using

¹We also initially incorporated errors of a moving average nature, but found no significant correlation of this form, having allowed for lagged dependents in our covariates.

199 this approach, the likelihood is expressed as a function of the hyper parameters and the
200 priors for the latent components only.² This likelihood is denoted here as $L(\{y_t\}, \Gamma_0, \Psi)$.

201 Classical estimation usually proceeds by finding the estimated value of $\hat{\Psi}$ that max-
202 imises the likelihood. Subject to regularity conditions, inference about the parameters
203 Ψ can then be performed using likelihood ratio, Wald or Lagrange Multiplier tests. Like-
204 lihood ratio and Wald tests have distributions that are non-standard (see Harvey, 1989,
205 p.234). The estimates of the latents (along with their (co)variances) can be obtained
206 (at $\hat{\Psi}$) by the Kalman Smoother.

207 3.2. Bayesian Estimation

208 Bayesian inference uses the posterior distribution of the parameters. Unlike the
209 Classical approach all the parameters are treated as random variables. The likelihood is
210 therefore viewed as the density of the data conditional on these parameters. For example,
211 the likelihood above, can be denoted as a marginalised likelihood $f(\{y_t\} | \Psi; \Gamma_0)$. Con-
212 sequently, using the Bayes theorem implies that the prior distribution for Ψ is $f(\Psi)$.
213 It then follows that the posterior distribution is $f(\Psi | \{y_t\}; \Gamma_0) \propto f(\{y_t\} | \Psi; \Gamma_0) f(\Psi)$.
214 Providing this posterior can be mapped, Bayesians will report the mean and variance
215 of the posterior distribution as point estimates.

216 Bayesian estimation with ‘flat’ priors, delivers a posterior density that is, over a
217 certain range, approximately proportional to the likelihood. Therefore, Bayesian infer-
218 ence can often give results that are similar to those derived using maximum likelihood.
219 However, in some situations, Bayesian and Classical estimates may diverge. For exam-
220 ple, if there are two distinct local maximums for the likelihood, then there may be two
221 distinct parameterisations of the model that equally well represent the sample informa-
222 tion. Unlike Classical procedures, Bayesian inference is not based on the behaviour of
223 the likelihood function locally around a single point where it has been maximised. From
224 a Bayesian perspective, the values of the parameters at the maximum of the likelihood,
225 and the curvature of the likelihood at that point, do not fully reflect the sample infor-
226 mation. Should the likelihood be multimodal all high density points are reflected in the
227 final estimates (the mean and variance of the posterior distribution). Just as impor-
228 tantly, however, we can examine the entire posterior distribution of key parameters to
229 learn about the data generating process.

230 An introduction to a Bayesian approach to estimating structural time series models
231 is presented in Koop (2003). Unlike the approach outlined in Koop, we map the pos-
232 terior distribution for the parameters of the hyper parameters Ψ using a random walk

²The latents have been integrated out of the likelihood function as opposed to the ‘concentrated’ or ‘profile’ likelihood. See Harvey (1989) for details.

233 Metropolis-Hastings algorithm (see Koop 2003, p.92). This is a simple and efficient
234 computational tool for the posterior distribution given that that the number of hyper
235 parameters are few. Given draws of Ψ from the posterior distribution of the hyper pa-
236 rameters, the smoothed estimates of the latents can then be generated using the Kalman
237 Smoother along with an estimate of the covariance matrix for the latents. This could be
238 done by simply plugging in a point estimate $\hat{\Psi}$ and obtaining conditional estimates of
239 the latents at that value. However, using the Kalman Smoother to generate the latents
240 in this way is not fully Bayesian, because a fully Bayesian estimate of the latents would
241 embody the parameter the posterior uncertainty (variability) in the estimates of the
242 latents (Ψ). A fully Bayesian approach requires a draw for each of the latents which
243 needs to be made for every posterior draw of Ψ within the sampler. While we follow
244 this latter approach, we note that it yields similar results in most cases in comparison
245 to where the former approach is followed.

246 **3.3 Bayesian Model Comparison**

247 By employing Bayesian methods we are also able to compare model performance
248 very easily. Working directly with the marginalised likelihood $f(\{y_t\} : \Psi, \Gamma_0)$ has the
249 advantage that the values of $f(\{y_t\} : \Psi^g, \Gamma_0)$ can, for posterior draws of Ψ^g (where
250 $g = 1, \dots, G$), be recorded directly within the estimation process facilitating model com-
251 parison. In order to compare models we use the Deviance Information Criteria (DIC)
252 of Spiegelhalter et al. (2002). The DIC provides a measure of model performance in
253 terms of the balance between goodness of fit and model complexity. In the literature, a
254 model with the smallest DIC is considered the preferred model according the DIC cri-
255 teria. The DIC is a Bayesian analog of the Classical information criteria (e.g. Akaike).
256 Numerically it computes a value of K , which is an estimate of the ‘effective number of
257 parameters’. The DIC rewards a high average log likelihood, but penalizes each model
258 according the effective number of parameters.

259 **3.4. Priors**

260 As discussed above, the use of the Kalman Filter requires priors to be specified for the
261 latents (Γ_0). These need to be specified in both a Bayesian or Classical context. While, in
262 principle, these can be specified using prior information, an alternative, ‘non informative’
263 approach is to use the first few observations of the explanatory variables in order to
264 construct a proper prior, after which we exclude these observations in estimation. This
265 can be done more easily, but equivalently, by setting the mean of the latents to zero and
266 the covariance of the latents can be set to be equal to a very large value (e.g. $I \times 10^8$

267 where I is an identity matrix). The predictive error likelihood (Harvey, 1989, p126) is
 268 then summed from $t = n + d + 1$, where $n + d$ is equal to the number of regressors in
 269 the model and d is equal to the number of non-stationary components in the transition
 270 equation. Within the Classical approach no further priors are required.

271 However, if a Bayesian approach is used, then priors are also required for the hyper
 272 parameters Ψ . These can be set in a reasonably non-informative way by reparameterising
 273 the model as $\Psi^* = (\ln \sigma_e^2, \ln \sigma_v^2, \ln \sigma_w^2)$ and then adopting a flat (improper) prior

$$p(\Psi^*) = I_{[-u, \infty]}(\ln \sigma_e^2) \times I_{[-u, \infty]}(\ln \sigma_v^2) \times I_{[-u, \infty]}(\ln \sigma_w^2) \quad (5)$$

274 where $I_{[-u, \infty]}(x)$ denotes an indicator function which is equal to one if $x \in [-u, \infty]$ and zero
 275 otherwise. The finite bottom bound is required because as the variance goes to zero,
 276 then the logged variance becomes near unidentified (which means that the likelihood
 277 become invariant to smaller values) below a small value $-u$. Here we set $u = 25$ (results
 278 are negligibly different to those we present if we set $u = 10$ or 50).

279 4. Empirical Section

280 4.1. Data

281 The temperature data that we employ in this Section are obtained from the CRU
 282 website. These are global temperature anomalies from 1850 to 2009³. The forcing
 283 measures are those used in Mills (2009) available on David Stern's website⁴ from 1850
 284 to 2000. Therefore, our estimated models are over this shorter time period, 1850 to 2000.
 285 The construction of this data has been discussed in a number of places and therefore we
 286 do not repeat this here.

287 In this paper, as in Mills (2009), we employ the aggregate forcing measure that is
 288 a linear sum of the greenhouse gases, sulphur dioxides, and solar components. The use
 289 of aggregative or total forcing can be justified from a theoretical view since they are
 290 constructed in such a way that the measures should be summable. Moreover, previous
 291 work using total forcing has suggested that this measure is cointegrated with tempera-
 292 ture anomalies, and Mills (2009) also finds that a test for equality of the forcing measures
 293 accepts this restriction. As can be seen from this plot, there is an evident rise in both
 294 radiative forcing and temperatures over most of the later part of last century.

³The series we use is Hadcrut3gl. We note that the variance adjusted version of the series available from the Website <http://www.cru.uea.ac.uk/cru/data/temperature/> gives very similar results in the models we estimated.

⁴<http://www.sterndavidi.com/datasite.html>

Table 1: Null of Cointegration

No Trend	Trend
$C_\mu=0.218$	$C_\tau=0.219$
P value (0.133)	P value (0.005)

4.2 Tests for Integration.

Since the unit root behaviour and tests under the null of no cointegration have been presented in the preceding literature we will not repeat this analysis herein. However, briefly, as in previous work (e.g. Mills, 2009) unit root tests indicated that both temperature and radiative forcing series are non-stationary. Both series (temperature and radiative forcing) are consistent with being integrated of order one according to Augmented Dickey Fuller tests along with other tests including those that adopt a unit root as the alternative hypothesis. The results of these tests are available from the authors on request.

4.3 Null Hypothesis of No Cointegration

Tests for cointegration (Johansen rank test) allowing for a restricted trend and intercept in the long run relationship, indicate that forcing and temperature series are cointegrated. The VAR analysis suggests that, using a model with an intercept and a time trend, two lags are appropriate (on the basis of an F test of the significance of a third lag, and according to both Akaike and Bayes information criteria), and that the hypothesis of no cointegration is rejected at below the 1% level of significance. This was also supported by Bayesian estimation of the VAR with and without rank restrictions. Regardless of lag length, the DIC criteria supported cointegrated models over a fully differenced VAR or VAR without rank restrictions. Again due to length constraints these are not reported here.

4.4 Null Hypothesis of Cointegration

The tests for cointegration, adopting cointegration as the null rather than the alternative, is less definitive. The critical values for the tests of no cointegration C_μ and C_τ are given in Shin (1994). However, we simulated the p-values for our sample size (151) using 10,000 Monte Carlo trials⁵. These results are presented in Table 1.

As can be seen from Table 1, we cannot reject the cointegration hypothesis at the 10% level of significance if a trend is not included in the regression, but if a trend is included,

⁵Our simulated critical values are very similar to those produced in Shin, (1994), therefore, we believe that our p-values should be accurate.

we would reject the cointegration hypothesis at a very low level of significance. The trend in the FM regression is not significant, therefore we may as well conclude that the No Trend result is reliable (i.e. preferred). Nonetheless, the rejection of the cointegration hypothesis when a trend is included needs to be given some weight. Therefore, it is not completely clear that the null of cointegration between the two variables cannot be rejected using these tests.

4.5 Structural Time Series Results

Moving on to an analysis of the structural time series model represented by equation (3) under alternative restrictions, we first discuss the Classical Maximum Likelihood results, before assessing the results from the Bayesian analysis. In all models (containing a random trend β_t), the estimate for the parameter σ_w^2 was indistinguishable from zero and a p-value for this restriction based on an adjusted likelihood ratio test was close to one. Therefore, for subsequent analysis we imposed the restriction that $\sigma_w^2 = 0$ (the trend term β_t in the equation is time invariant) for all models.

Therefore, we have three models:

- M1: σ_e^2 and σ_v^2 (unrestricted model)
- M2: $\sigma_v^2 = 0$ (cointegrated model)
- M3: $\sigma_e^2 = 0$ (random walk error model)

M1 contains both a stochastic intercept and a random error and nests both models M2 and M3 as special cases. M2 is equivalent to a standard regression with a stationary error (a cointegrated model). M3 has a non-stationary error, with only a random walk intercept.

4.6 Classical Results

The results presented in this paper include up to three lags of the temperature variable as explanatory variables in equation (3). A fourth lag is insignificant in all models that we estimated. The significance of the lags depended on the restrictions that were placed on the variance terms. For models that have the restriction $\sigma_e^2 = 0$ imposed, all three lags are highly significant. For models that imposed $\sigma_v^2 = 0$ only the first lag is highly significant. Therefore, we present results for one, two and three lags.

Due to failure of detectability and stabilisability conditions (see Harvey, 1989 for details) if $\sigma_w^2 = 0$, then a formal test of $\sigma_w^2 = 0$ cannot be constructed using likelihood ratio, Lagrange Multiplier or Wald statistics. Thus, for performing a formal Classical

354 test of the cointegration hypothesis, we rely on the Shin tests statistics presented in
 355 Table 1.

356 A valid test can be constructed for $\sigma_e^2 = 0$ using a likelihood ratio test provided the
 357 significance is adjusted to take account that it is on the edge of the parameter space.
 358 The p-values results for testing restrictions $\sigma_e^2 = 0$ are presented in Table 2 for M3
 359 containing one, two and three lags.

Table 2. Likelihood Ratio Tests and Likelihood Comparisons

	1 lag	2 lags	3 lags
Null Model= M3*	1E-5	0.0005	0.1529
$\ln(L_{M2})-\ln(L_{M3})$	8.468	3.25	-0.450

*The alternative model is M1. Values represent P-Values for the null hypothesis

360 As we noted above, the significance of the lags implies that three lags are definitely
 361 required for a valid test of M3. As we can see from Table 1 when the model contains one
 362 or two lags only, $\sigma_e^2 = 0$ is rejected. However, where there are three lags in the model we
 363 cannot reject the null at the 10% level. In other words, provided three lags are included
 364 in the model (all of which are significant), a model with a pure random walk cannot be
 365 rejected.

366 The importance of the number of lags to include is also apparent when comparing
 367 models M1, M2 and M3. Comparing the log-likelihoods for each of the models, the
 368 likelihood function M2 is higher than for M3 for one and two lags, but if three lags
 369 are included then the likelihood function for the pure random walk error model (M3)
 370 is in fact slightly higher than for M2. In summary, if three lags are included, then a
 371 model which has a random walk error cannot be rejected and, this model has a higher
 372 likelihood function than the cointegrated model.

373 Henceforth, we only report the results for models with three lags. The reason for
 374 this is that for the Classical results the coefficients of explanatory variables are almost
 375 identical for models M1 and M2 regardless of whether one, two or three lags are included.
 376 However, as we have outlined above for M3, the third lag is highly significant. Therefore,
 377 results for M3 would be biased unless three lags are included.

378 We now present, in Table 3, our Classical estimates for the structural time series
 379 models.

Table 3. Classical Estimates of Coefficients

	M1	M2	M3
temp _{t-1}	.424 (.0829)	0.541 (0.082)	-.338 (.083)
temp _{t-2}	-.1466 (.089)	-0.105 (0.093)	-.361 (.079)
temp _{t-3}	.0253 (.0829)	.111 (.081)	-.2603 (.080)
forcing	.2548 (.0963)	.2434 (.063)	.164 (.226)
trend	.0011 (.0017)	.0001 (.004)	.0082 (.0091)
σ_e	.0958	.1021	
σ_v	.0170	.	.1073
-2LogL	-642.818	-640.87	-641.77
Prediction error variance	.011795	.011862	.011656
AIC	-4.3209	-4.3152	-4.3327
BIC	-4.1411	-4.1354	-4.1529
Normality P-Values*	0.3829	0.3026	0.6075

Numbers in parentheses are standard errors

*From Bowman Shenton Statistic (Harvey, p.260)

381 Considering the Classical results in Table 2, the coefficients for the explanatory
382 variables are presented along with estimates of the variances of innovations that drive
383 the irregular and random walk components plus other summary statistics. All models
384 appear to have normal errors, and according to both the information criteria used,
385 M3 is the preferred model. In M1 it is evident that the variances σ_e^2 is estimated to be
386 considerably larger than for σ_v^2 . However, this change in M2 and M3 whereby setting one
387 of the variances to zero, yields a variance estimate of a similar magnitude for the other.
388 This may seem surprising, given that the effects of innovations of v_t are cumulative and
389 would generally therefore be expected to have smaller variance. However, examination
390 of the coefficients for the lagged temperatures in the models reveal that coefficients
391 are very different in models M1 and M2 compared with M3. The lag coefficients in
392 M3 are all negative and sum to around -0.96. This means that the apparently irregular
393 component in the series is being captured by negative correlations from period to period,
394 even though each shock is treated as having a permanent impact. Notably, the estimates
395 from unrestricted model M1 are much more similar to M2 than M3. Importantly, both

396 M1 and M2 have a highly significant positive coefficient on the forcing variable (0.2548
397 and 0.2434 respectively).

398 Given the magnitudes of the lag coefficients, the long-run multipliers, which are
399 defined as $\frac{\alpha}{1-\sum_{i=1}^3 \gamma_i}$ for the impact of forcing on temperatures, are approximately 0.34
400 and 0.54 for M1 and M2 respectively. This is in contrast to the smaller and insignificant
401 coefficient from M3 (0.1649) and a corresponding long run multiplier at just over half
402 that value. Therefore, the findings with regard to the impact of radiative forcing are
403 substantively different if we use M3 rather than M1 or M2. Furthermore, as discussed
404 above, the restriction of M1 to M2 cannot be rejected on the basis of a Likelihood Ratio
405 test, and M3 has a slightly higher likelihood (providing 3 lags are included in the model).

406 These results may seem confusing since the unrestricted model M1 yields rather sim-
407 ilar results to the restricted cointegrated model M2, yet M3 which yields very different
408 estimates seems to be marginally preferred to M2 (if three lags are included). The rea-
409 son for this outcome is that there is a global maximum likelihood which has a relatively
410 small variance in the random walk component and a larger variance in the irregular
411 component. However, the evidence here suggests another local maximum with a small
412 irregular component and larger random walk component. Maximum likelihood estima-
413 tion reflects only the former (global maximum). However, from a Bayesian perspective,
414 point estimates should be derived from the full posterior density, not just a single mode.
415 For this reason we now consider Bayesian estimation.

416 4.7 Bayesian Results

417 The Bayesian estimates of all three models above are presented in Table 4. These
418 are presented along with the DIC for each model, which should be at a minimum for
419 the best performing model.

420 First, it is evident that the two restricted models (M2 and M3) yield virtually identi-
421 cal estimates to the Classical results reported in Table 2. This is because by restricting
422 either of the variances the values of σ_e or σ_v derived from the mean of the posterior
423 are almost the same as their maximum likelihood components and we have only a very
424 small standard deviation.

425 Second, the unrestricted model, when estimated using a Bayesian approach, yields
426 quite different results from the Classical approach. Examining the coefficients of M1 it
427 becomes clear that the estimates sit in between M2 and M3. This is because, in effect,
428 it averages over M2 and M3, since both these models have reasonably high posterior
429 densities. This can best be seen by the contour plots of the joint posterior densities for
430 σ_e and σ_v displayed in Figure 1. There are two clear posterior modes, one where σ_e is
431 very small and σ_v is around 0.10 and one where σ_v is very small and σ_e is around 0.10.

432 Between these two modes there is a ridge of a lower density region that highlights the
 433 negative covariance between σ_e and σ_v . The point estimates obtained by taking the mean
 434 posterior values are 0.0614 and 0.0575, but neither of these points are high density points
 435 as such. Rather they sit somewhere in the middle between the two posterior modes.
 436 The rest of these coefficients also reflect this tendency to average between these two
 437 highly competing models.

438 Third, according to the DIC, the models are ranked M3 (top) followed by M1 and
 439 then M2 which concurs with the Classical information criteria. Thus, the cointegrated
 440 model is less preferred as compared to the unrestricted model of random walk errors.
 441 Radiative forcing retains its positive coefficient estimate, but the standard deviations for
 442 this coefficient are as large or larger for both M1 and M3. Thus a (Bayesian) credible
 443 interval would contain considerable mass below zero. Interpreting this in Classical terms
 444 would suggest that the forcing variable is insignificant.

Table 4. Bayesian Coefficient Estimates

	M1	M2	M3
temp _{t-1}	.053 (.351)	0.541 (.082)	-.338 (.089)
temp _{t-2}	-.251 (.128)	-.1056 (.093)	-0.3609 (.0801)
temp _{t-3}	-0.115 (.159)	.1109 (.082)	-0.2603 (.0811)
forcing	.194 (.180)	0.253 (.063)	0.1649 (.229)
trend	.0047 (0.007)	.0001 (.0004)	.0082 (.009)
σ_e	.0614	.1027	.
σ_v	.0575		.1080
DIC	-206.8472	-206.727	-207.60

445

Numbers in Parentheses are standard deviations.

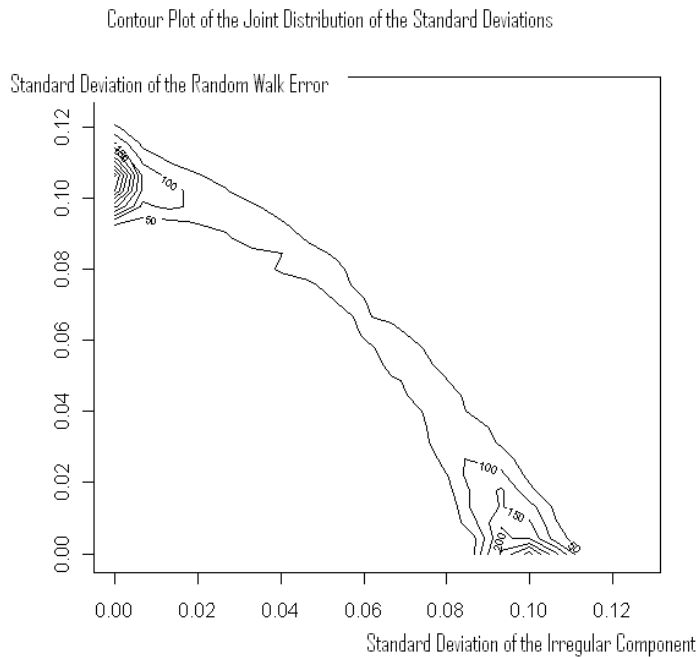


Figure 1: Contour plot for the joint posterior densities

446 **5. Discussion**

447 Most recently Mills (2009) presented statistical evidence that there is a ‘long-run
 448 equilibrium’ between radiative forcing measures and temperature using data from 1850-
 449 2000. Mills (2009) builds upon work by Brohan et al. (2006), Stern and Kaufmann
 450 (2000), Kaufmann and Stern (2002), Kaufmann et al. (2006) that broadly supports the
 451 contention that forcing measures have a quantitative impact on global temperatures.
 452 Dergiades et al. (2016) provide results on long-run changes in radiative forcing and
 453 surface temperature consistent with the theory of anthropogenic climate change. Fol-
 454 berth et al. (2012) find that increased emissions and radiative forcing have a significant
 455 (negative) impact globally on megacities.

456 The results we have presented in our analysis are an example of how there remains
 457 uncertainty about the form and strength of the statistical relationship between global
 458 temperatures and human activity. Indeed, our results are not suggesting that there is no
 459 direct human role in climate change, far from it. But from a strategic perspective they
 460 speak to the idea that there is still uncertainty as to the specific causal mechanisms and
 461 as such we need to be somewhat cautious when it comes to how we might best articulate
 462 the specific type of policy interventions required. This point is also discussed by Tol
 463 (2018) in an excellent overview of the economics of climate change. As is explained,

464 climate change is universally agreed to be a negative externality and that policy does
465 need to be put in place to deal with the effects. However, what remains a hotly debated
466 issues is the impact of climate change. The reason why this is so contentious is that
467 the impact will in turn inform the price that is placed on carbon and differences opinion
468 of this have serious ramifications for policy. Therefore, econometric analysis showing
469 causal links between temperature and human activity still matter as this can inform the
470 focus of such policy interventions and also the likely price of carbon required to induce
471 the necessary changes in behaviour. So remaining uncertainty about the relationships,
472 such as the one examined in the paper matter.⁶

473 The impact of any statistical results in this domain can be considered in terms of how
474 they may or may not influence policy making. Although there is a general consensus
475 about the impact of human economic activity on the climate there still remains much
476 uncertainty as to the precise mechanisms through which this works (for examples of this
477 within areas of environmental policy making, see Touza and Perrings (2011), Eichner
478 and Pethig (2018) and Kersting (2018)). This uncertainty, however, can causes problem
479 for government when it comes to making credible commitments. Clearly, if governments
480 are able to state a credible position regarding climate policy this can reduce uncertainty
481 for economic agents. However, credible commitments by government cannot typically
482 be enforced and as such economic agents will always place some positive probability
483 on a policy change and as a result a loss of some degree of credibility. But there is
484 also the need for policy responses to be flexible especially as new information becomes
485 available. An obvious and well understood consequence of this resulting uncertainty will
486 be impacts in terms of investment directed to dealing with aspects of climate change.
487 Zetland (2017) argues that within the context of group cooperation in the provision
488 of public goods it may be easier to promote cooperation in the provision public goods
489 within a more competitive setting whereby teams (or coalitions) are encouraged to
490 beat other teams (or coalitions) rather than cooperating with them. Of course, there
491 are steps that can be taken to minimize the impact of credible commitment whilst
492 retaining flexibility. But, the econometric results we present and the literature we add
493 to demonstrate clear that there remains aspects of uncertainty and that this means that
494 there must be flexibility in policy making even if this impacts on policy makers ability
495 to credible commitment to policy options today.

496 Mills (2010) provides a useful take on this issue. Essentially, statistically arguments
497 alone will not provide definitive evidence or singularly resolve many of the most highly

⁶We contend that there needs to be less strident expression of opinion about this topic and the research published. This is neatly illustrated in the response to Tol (2016) by Cook et al. (2016) who criticise Tol because of a specific use of data on opinions about climate change. Exchanges such as this distract attention from the very real and important issues that climate change presents.

498 debated issues within the literature. As he neatly explains, when it comes to examining
499 these issues:

500 *“Statistical arguments alone are unlikely to settle issues such as these, but neither*
501 *are appeals to only physical models or the output of computer simulations of coupled*
502 *general circulation models. In such circumstances it would appear that, to quote another*
503 *ageless proverb, ‘you pays your money and you takes your choice’. Indeed, it could be*
504 *argued that such a proverb is particularly apposite given the ongoing debate concerning*
505 *the potential costs of combating global warming and climate change, the most notable*
506 *recent protagonists being Stern (2007) and his reviewers, for example, Nordhaus (2007),*
507 *Tol and Yohe (2006) and Weitzman (2007).”* (p. 424).

508 Finally, there is scope for micro and context specific studies of greenhouse gas emis-
509 sions to provide greater context to the type of global study we have presented here.
510 For example, the single and multiple country level studies, such as those undertaken
511 by Guntin-Araujo et al (1999), Fereidouni (2013), Yusuf et al. (2014) and Raheem and
512 Ogebe (2017) can help to empirically link global carbon dioxide emissions with specific
513 sources.

514 The way to address this is as follows. If going to further our understanding of global
515 temperatures there is a need to examine and challenge existing hypotheses - this is not
516 to refute global warming but to at least raise the prospect that current mechanisms
517 as they are understood may need to be redefined in light of alternative model results.
518 This is a very important result while formulating appropriate environmental policy at
519 national and international levels, to realise more effective outcomes.

520 **6. Conclusions**

521 In this paper, we have presented further empirical investigation of the relationship
522 between radiative forcing and global temperature anomalies. Unlike other recent work
523 exploring this relationship, we used a structural time series approach comparing alter-
524 native models as well as adopting cointegration as the null hypothesis. Our findings
525 suggest that previous findings of cointegration between forcing measures and tempera-
526 tures should be treated tentatively. While the data is consistent with a positive impact
527 of radiative forcing on temperatures, the significance of the impact of forcing was model
528 dependent. While a model that assumes cointegration between forcing temperatures
529 performs reasonably well, a non-cointegrated model performs just as well, or on the basis
530 of the tests conducted here, even better. This was particularly evident when examining
531 the posterior density of the standard deviations in the irregular and random walk errors.

532 The reason for this finding has been explained using Bayesian methods. Specifically,
533 a contour plot of the posterior densities showed two peaks, one in a cointegrated region

534 and another in a cointegrated region. In addition, the DIC model selection criteria also
535 suggested that restricting the model to one with only a random walk error improved the
536 performance of the model. Finally, in models where temperatures and total forcing are
537 not treated as being cointegrated, then the evidence that total forcing has an impact on
538 temperatures is reduced. However, we would contend that given the ongoing debates
539 within the literature regarding how best to statistically capture, explain and model this
540 relationship, that researchers should avoid being too strident in their claims about model
541 performance. This then inevitably implies uncertainty regarding our understanding of
542 the relationship which in turn has implications for how policy makers respond to and
543 use statistical results of this in policy design and implementation.

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