Macro-economic rebound estimation using Granger causality tests

Blake **Alcott** & Tyler James **Marangi**

blakeley@bluewin.ch & tjmarangi@gmail.com

Keywords: **energy efficiency**; **rebound**; **Jevons' Paradox**; **Granger causality**; **caps**; **world data**

Abstract. Do energy-efficiency increases – changes in processes that lower the amount of energy input required to produce a unit of output – contribute to the environmental-policy goal of reducing energy consumption and/or emissions? How big is the undeniable latent demand in society for increased output due to decreased input costs? Since 1980 attempts to answer this 'rebound' question have depended on theory and micro-economic empirical work tracing household expenditure reactions to price changes induced by efficiency increases, with no resulting consensus on total rebound's size. We instead take world data on Total Primary Energy Supply *TPES* and World Product *WP* (the GDPs of 190 countries) from 1971-2009 and perform regression and Granger causality tests using change in *TPES* and *WP/TPES* as the only two variables. Rebound may be around 100%, casting doubt on the efficacy of the environmental energy-efficiency strategy. Caps and energy taxes are well-known alternatives that are by definition efficate.

1 Introduction and Rebound

Assuming that energy-resource depletion and/or the pollution resulting from combusting energy resources are unsustainably high, we search for strategies or policies to reduce rates of depletion (*ipso facto* reducing pollution) of carbon-based energy resources. A popular strategy is to increase energy efficiency, defined as the ratio between economic outputs and energy inputs. A given number of lumens produced by fewer kilowatts is an example of an energy-efficiency increase and can be expressed as a percentage change. The strategy prescribes policy-induced efficiency increases to supplement the business-as-usual ones taken by firms and consumers to lower production costs. *If* the number of outputs remains the same, an amount of energy is saved called *engineered* (or *engineering*) *savings.*

Since, however, it is just as accurate to describe efficiency increases as increased production of outputs while consumed inputs remain the same (conserving no energy) economists endeavour to measure effective demand for outputs and energy inputs subsequent to energy-efficiency increases. This is known as *rebound* and is measured as a percentage of engineered (engineering) savings as defined above. If latent demand is such that output increases by a percentage less than the percentage increase in efficiency, some input is saved and rebound is < 100%; some energy is conserved. If the market *supply* of energy resources remains the same (Turner & Hanley, 2011), and consumer demand for outputs is high enough that all of this previous amount is still consumed, rebound = 100%. If the efficiency increases cause even more energy to be consumed, rebound is > 100% – the case known as Jevons' Paradox (Jevons, 1865; Alcott, 2005). While there is some evidence for this last case, from the point of view of *policy* it need not detain us here: at rebound of 100% the energy-efficiency strategy is ineffective (and as it approaches 100% it becomes *cost*-ineffective).

Let us embed the energy-efficiency strategy in the $I = PAT$ formula:

- Environmental impact, *I*, is in this paper the depletion of fossil fuels and biomass encapsulated in the metric Total Primary Energy Supply, *TPES*, and is a function of
- the number of consumers or the population, *P*,
- the amount of output of goods-and-services per person or affluence, *A*,
- and the technological efficiency, *T*, with which, on average, the relevant energy resources produce goods-and-services.

In its simple, inaccurate form the formula suggests that reducing any right-side factor reduces the left-side factor. However, changes in any right-side factor affect the other right-side factors, meaning there is no *necessary* reduction in impact. These interdependences can be called rebounds, and the formula must be written $I = f(P, A, T)$ as illustrated by **Figure 1**. (Alcott, 2010) Thus, the study of energy-efficiency rebound tries to measure the effects of lowering *T* on raising some combination of *P* and *A*. Theoretically and plausibly, increased efficiency has enabled more people to satisfy more demand for economic outputs, and as shown by Khazzoom (1980), *any* price elasticity of demand means that rebound is greater than 0%. But is it 100%? To answer this question, this paper statistically analyses only the relationship between *T* and *I*, ignoring for the moment population and per capita GDP (the numerator of the affluence factor). The research question is: How does observed percentage increase in *T* (rising efficiency or falling energy *intensity* of the economy) affect observed percentage increase in *TPES*?

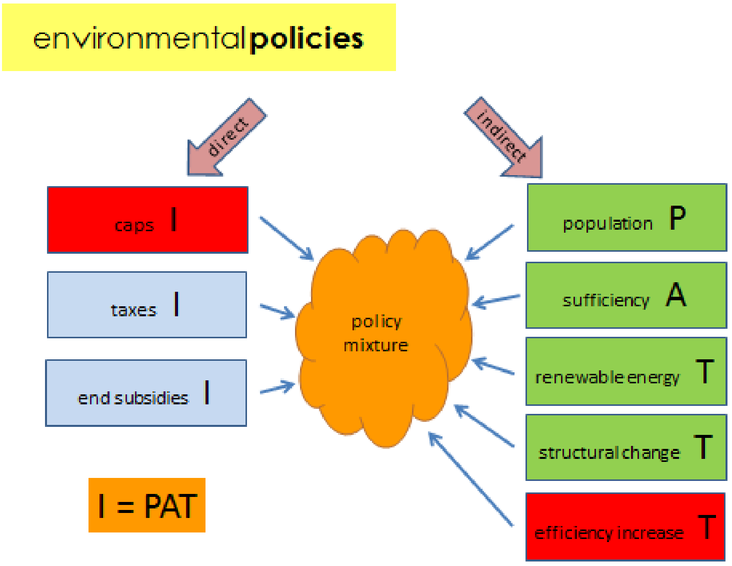

Figure 1. A menu of policies to *lower* environmental impact (I) defined as combustion of fossil fuels. The indirect ones are on the right side of I = PAT [accurately: I = f(P, A, T)] and hope to lower impact by lowering population size, consumption of goods and services per person, and/or fossil fuel amounts needed to produce goods and services. The direct ones render the indirect ones superfluous.

Actually, the quantity on the left side of $I = f(P, A, T)$ can be reduced directly by the policy of *capping* energy consumption. By contrast, policies to reduce right-side factors are indirect (and hopeful). Capping is either by physically defining overall, politically desired amounts of resources that are allowed to be mined or harvested, or by taxing their consumption so high as to result in the desired amount. (Weitzman, 1974; Daly, 1974; Tickell, 2008) While the indirect measures are uncertain, the effectiveness of the left-side policy is 100% certain in the case of caps, and reasonably controllable in the case of taxes. This argues for first attempting to enact the caps or high taxes. The complex task of measuring the interactions between population size, goods-and-service consumption per person, and technological efficiency (and *ipso facto* of measuring rebound) is necessary *only if* caps are politically rejected – in which case we want to know if efficiency increases move towards less resource consumption or not.

Hundreds of rebound studies have been undertaken since the groundbreaking work of Brookes (1978; 1979) and Khazzoom (1980). Most work has been micro-economic, seeking by various means to see how an autonomous energy-efficiency increase works its way through all the sectors of the economy. Employed are consumer interviews and sectoral data using time series and input-output analysis to determine elasticities and cross-elasticities of demand given the *income effect* that emerges when consumers find themselves with leftover purchasing power after paying less for the energy needed for their lifestyle heretofore. Efficiency has increased the economy's *production possibilities frontier* (how much output it can produce from given combinations of inputs), but to what extent are these possibilities availed of? For the same amount of car-kilometres, electronic gadgets, house-heating or illumination one will pay less because efficiency increases induces price decreases both for goods-and-services and the energy inputs themselves. The literature distinguishes between *direct* and *indirect* rebound, totalling to *total* rebound. Driving farther with the same amount of fuel after buying a more fuel-efficient car is an example of direct rebound. Spending the money saved on anything else is indirect rebound. Total rebound is the only quantity of interest from the environmental perspective.

It is of interest that there is no methodology for deriving indirect or total rebound from direct rebounds, however accurately they are measured. Indecisive, moreover, are known micro-economic methods for measuring indirect rebound and macro-economic methods of measuring total rebound without taking the detours of measuring direct and indirect rebound. The unfortunate result is that estimates of total rebound vary from about 15% to about 350%. (Dimitropoulos, 2007, p 6360; Sorrell, 2009) Total world rebound was estimated at 51% by 2030 by Barker et al. (2009, p 425), at 58% in Germany by Frondel et al. (2008, p 154) and at over 100% at the macro level – levels at which efficiency policy becomes environmentally harmful, counterproductive – by Brookes (1990; 2000), Saunders (1992; 2000), Roy (2000), Fouquet & Pearson (2006), Polimeni (2009) and Hanley et al. (2009).

Formally, the search is for the *efficiency elasticity of demand* (Sorrell et al., 2009, p 1359) for the input that is newly being used more efficiently or productively in the technical sense. This can be broken down into finding the efficiency elasticity of the *prices* of (1) goods-and-services and (2) energy itself; and the price elasticity of *demand* for these two categories of things. But these elasticities remain elusive, and indeed theory rather than empirical findings dominates rebound research (Birol & Keppler, 2000): While it is uncontested that energy consumption has increased alongside efficiency increases, it is claimed by some that consumption *would have* increased even more without the efficiency increases (Howarth, 1997, p 3; Schipper & Grubb, 2000, p 370) – claims that are undoubtedly purely theoretical. Most partisans of high rebound of around 100% likewise acknowledge their reliance on theory. (Saunders, 2000; Sanne, 2000)

Transdisciplinarity would welcome non-economists to address the question. Economic history, anthropology and psychology, for instance, should be allowed to research the topic. (Moezzi, 2000; Sanne, 2000; Tainter, 2008) Indeed, before describing our study, in **Figure 2** we show the basic rebound question as seen from the point of view of such disciplines: What do human beings do with resources that momentarily lie fallow, due to technological efficiency increases, when the conditions of their supply remain the same? What do they ‘demand’? What effects does the larger production possibilities frontier have on population and affluence?

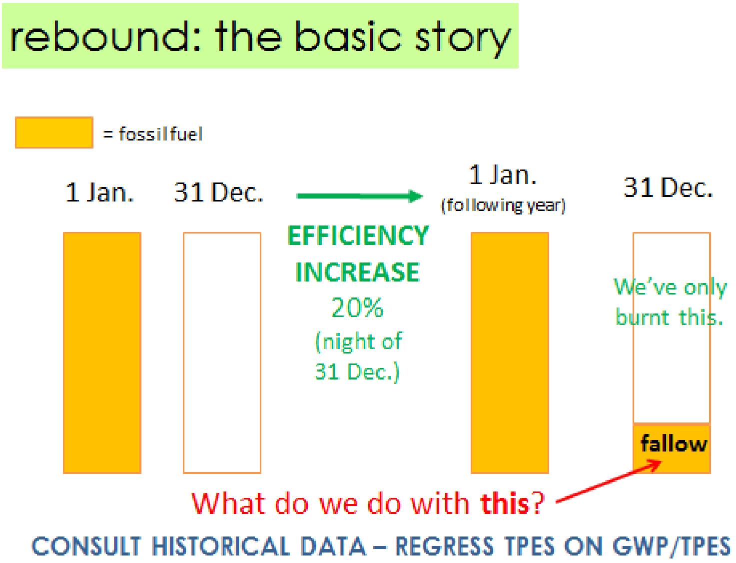

Figure 2. If economic agents leave the initially fallow amount lying fallow, rebound is 0%; real savings = engineered savings. In this case they would produce less and have more non-working time. If all the initially fallow amount is used up (perhaps with a slight delay) by more production of goods and services and/or more people, rebound is 100%; efficiency increase has had *no effect* on consumption or environmental impact. It is this hypothesis we test.

2 Research question and metrics

To date rebound research has thus produced no results offering robust guidance in choosing environmental policies. We now suggest an approach to the question of whether efficiency increases in and of themselves cause the rate of increase in resource consumption to slow, in which case rebound would be < 100% and the environmental efficiency strategy would be effective. We are looking for the direction of the causal arrow – or the absence of a causal arrow. An ‘absence of a causal arrow’ would describe the case where efficiency increases do not cause resource-consumption rates of increase to change; the trend stays the same, and any temporarily fallow-lying amount of the resource (input) in question gets mined and combusted.

The method here is thus not micro-economic like the research described in the previous Section, nor does it apply macro-economics by means of a multivariate model of energy consumption (*TPES*). Instead, we attempt two statistical analyses, using world data, taking *TPES* as our dependent variable and as our *single* independent variable *TPES* efficiency in producing goods-and-services. The metric for the latter is the sum of the GDPs of the 190 countries in our database (IEA, 2012) divided by *TPES*. This we call World Product (sometimes referred to here and in other research as World- or Aggregate-GDP), meaning our independent or causal variable is *WP/TPES*.[1] To our knowledge, aside from Jevons (1865, pp 261-280, 387-388), only one other study has adopted a similar approach: Polimeni (2009) regressed *TPES* on the *several* independent variables including population, population density, urban population, rural population, GDP, exports, imports, household consumption, government consumption and energy intensity, this last being merely the inverse of our variable, energy efficiency.[2] Polimeni's study, although achieving more depth, is moreover at somewhat less than world scale, covering the four economic data units U.S., Brazil, sixteen European countries and twelve large Asian countries. Our time series covers 38 years, from 1971 through 2009.

Using world data avoids a problem with country or country-group (e.g. OECD) data. These must namely grapple with so-called *leakage*, for the measurement of which there is as yet no consensus. Leakage occurs when a given country's *TPES* is underreported because the energy embodied in its net imports goes uncounted, or overreported if that embodied in its net exports is ignored. (Helm et al., 2007; Peters, 2008; Alcott, 2012) We are moreover focusing on energy inputs alone, while other similar yet more complex studies, also at world scale, have asked after both energy and *material* consumption or focused on emissions rather than depletion; *see* Krausmann et al. (2009)[3], Luzzati & Orsini (2009), and Steinberger et al. (2010).

Our regressand or *explicandum*, *TPES,* includes the fossil fuels coal, oil and gas. It also includes combustible renewables and waste, nuclear, hydro, geothermal, and solar. (IEA, 2012)[4] In future work we will adjust this variable in three ways: (1) To focus on our metric for environmental impact we will deduct from TPES all sources except fossil fuels and biomass. (2) Next, we will add biomass used as *food* for both humans and work animals, i.e. include animal *metabolism*. (3) Finally, we will deduct biomass in order to focus even more narrowly on *non-renewable* as opposed to renewable sources. A very sophisticated study would even take into account changes over time in the *composition* of TPES, i.e. proportions of coal, oil, gas, etc.

Our regressor or *explicans* is a ratio, *WP/TPES*. To derive an extensive or absolute number (e.g. *TPES*, our 'impact' factor in terms of $I = f(P, A, T)$) from an intensive or fractional number (the efficiency ratio which is our 'technological' factor), one needs a further absolute quantity by which to multiply the fraction. This quantity would have to be independently measured rather than derived from *WP/TPES* in order to avoid a *reductio ad absurdum*. In terms of $I = f(P, A, T)$ some combination of increases in population and total goods and services – proxied by GDPs – provide these absolute amounts. But our statistical methods of regression and testing for Granger causality avoid this requirement because a model of *TPES* is not being attempted. Further statistical work could regress either *WP* (goods and services) or population size on efficiency using well-known models of economic growth. (Cleveland et al., 1984; Stern, 2000; Sorrell et al.,2009; Steinberger et al., 2010; Kümmel, 2011) We suggest that a major shortcoming of rebound research to date has been to treat population

[1] Aesthetically, we would like to use *GWP*, Gross World Product, but of course there is no net world product, so we are adopting this unusual numerator.
[2] We suggest adding to the model's factors discoveries of energy resources, decreases in the energy cost of obtaining useful energy, and efficiency increases in the use of the other major factor of production, labour.
[3] The time period covered by Krausmann et al. (2009) was very long, namely 1900-2005, and, like Steinberger et al. (2010) the materials whose consumption was measured were biomass, fossil fuels, industrial minerals and metallic ores, and construction materials. These studies included not only material/energy productivity and total material/energy consumption but also income, income per capita, population and biomass alone.
[4] Computations of TPES in joules from data on hydro, geothermal, etc. and nuclear electricity is explained in the IEA *Energy Statistics Manual*. (IEA, 2012)

fully exogenously.[5] Yet surely increased technological-efficiency increases in agriculture, home-heating, and transport, and the effect of rising affluence (enabled in great part by efficiency increases) on medical progress, for instance, have constituted necessary or even sufficient conditions for the seven-fold rise in population during the last 200 years. (Giampietro, 1994)

A further challenge is to endogenise our exogenous variable *WP/TPES*. For instance labour efficiency as *WP/work-hours* is usually increasing due to organisational, institutional or infrastructure changes rather than technological ones[6], causing *WP* to rise, and thus causing a change in our own *explicans*, *WP/TPES*, which has nothing to do with *technological* efficiency. We could perhaps arbitrarily assume that change in technological efficiency causes only half of the change in *WP/TPES* – the other half being caused by non-technological efficiency increases. We would then run an analysis using *WP/TPES divided by 2*. Further fine-tuning would include the effects of labour-efficiency and energy-efficiency changes on each other, sometimes partly subsumed under the concept of rebound effects with respect to time. (Binswanger, 2001)

We measure *WP* in Geary-Khamis dollars, which is using purchasing-power parity (PPP) rather than currency exchange rates, and the base year for adjusting for general price changes is 2000. Of course GDP measures only (the prices of) those goods and services recorded through monetary transactions, leaving barter, unpaid work and many illegal activities unmeasured. Thus, some real efficiency changes escape the GDP metric. We assume, however, that the *proportion* of production not included in GDPs remains about the same, and since we compare *change* in *WP/TPES* with *change* in *TPES*, we believe *WP* remains a good proxy for change in total output. We believe the main weakness of our monetary metric is that it is not the one used in defining policy-induced (mandated) efficiency increases. These are defined purely physically, e.g. joules per lumen or per ton-kilometre, not per unit of GDP. However, there is no reliable way of measuring change in aggregate output in physical units, and so we have opted to use *World Product*.

The hypothesis that the current paper intends to investigate is whether, on a global scale, changes in energy efficiency, measured as *WP/TPES*, can be shown to correlate to, either positively or negatively, and even cause, changes in total energy usage, measured as *TPES*. The null hypothesis that we ultimately test is that energy efficiency change has no causal effect on changes in total energy usage.

3 Methods and Results

Does a change in our independent variable *cause* a change in our dependent one? It is uncontested that empirical observation reveals rises in both, as shown in **Figures 3** and **4**. To answer this, within our bi-variate limitations, we first regress energy consumption on energy efficiency using ordinary least squares (OLS). Moving from correlation to causality tests, we then test for Granger causality. (Granger, 1969)

---

[5] GDP (and *ipso facto* WP) should also not be an exogenous factor (independent variable) in modelling energy consumption, as this ignores efficiency increase’s effects on GDP. (Madlener & Alcott, 2009)

[6] We have in mind factory- or office-floor efficiencies, smoothly functioning banking and court systems, and transport and information networks.

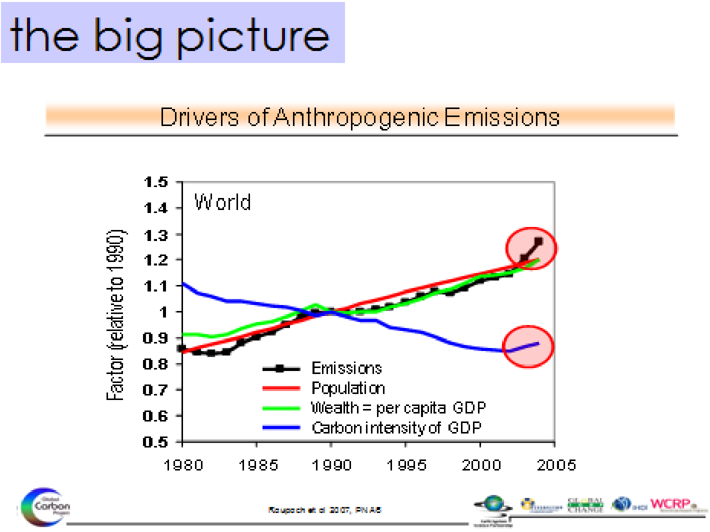

Figure 3. Emissions have risen at similar rates to fossil-fuel consumption and both similar to efficiency, shown here as its inverse, with the downward-sloping line. Thus, decoupling is consistent with increasing emissions and input consumption. Source: CGI based on Raupach et al., 2007: http://www.pnas.org/content/104/24/10288.abstract The graph is consistent with countless studies.

3.1 Regression analysis with Ordinary Least Squares

Our preliminary and tentative regression analysis tries to glean some further understanding of the apparent high correlation between *WP/TPES* and *TPES*, for which the basic data are given in **Appendix 7** and "TPES & WP Data Analysis, Time Series". Taking *WP* in its Purchasing Power Parity rather than nominal GDP form,

1. with OLS regression of the absolute values, R squared values coalesce around 97%, and around 92%[7], although much of this is down to shared general trends. Analysing real (percentage-based) change is necessary to get a better picture of correlation.

2. when *percentage change* in the values is regressed, R squared is 0.24. This suggests a degree of correlation, although it highlights the expected result that energy efficiency change, as measured by *WP/TPES*, cannot fully explain variations in *TPES*.

From the discovery of not insubstantial correlation between changes in aggregate energy efficiency and aggregate energy use, we were motivated to further tease out, if possible, whether, crucially, a direction of causality can be established, namely via Granger analysis for the time being. Although we could continue with added variables in OLS, and we may well do so in the future, our primary concern initially was to seek out causation. There is a high range, however, in which the degree of causation could be determined to fall. The two questions are (1) whether greater macroeconomic energy efficiency is indeed a causal factor in, i.e. driving, growth in primary energy supply (i.e. rebound of at least 100%) and (2) what fraction of total growth the efficiency increases are responsible for – perhaps up to a quarter, or even greater? We speculate that the degree of causation, if it exists, between such individual values lies somewhere well south of 100%, likely much closer to the 24%

[7] Looking at a somewhat different metric for environmental impact, Total Material Extraction or *TDMC,* Krausmann et al. (2009) found somewhat somewhat weaker correlations (between *WP/TDMC* and *TDMC* than in the case of energy alone.

proportion of response variation. Certainly there are other drivers of the observed increase in energy consumption, but we cannot yet conduct a fuller multivariate analysis and are mainly trying to see if technological efficiency increase is one of them.

3.2 Granger causality

Granger analysis relies on a time lag between the hypothesised causal and hypothesised dependent variables. If the lagged, independent variable can be shown to accurately forecast the future values of the other variable, changes in it *Granger cause* changes in the second variable. Both uni- and bi-directional causality can result; in our case we are hypothesising that changes in efficiency cause changes in consumption and therefore take efficiency as the lagged (in our case 'independent') variable. Because the Granger test cannot evaluate more than two variables at a time other independent (causal) variables can go undetected; therefore, suggested for the future are more complex vector autoregression (VAR) tests that are beyond the scope of the present paper. As shown in **Figure 4**, the two quantities *TPES* and *WP/TPES* themselves have changed at different rates.

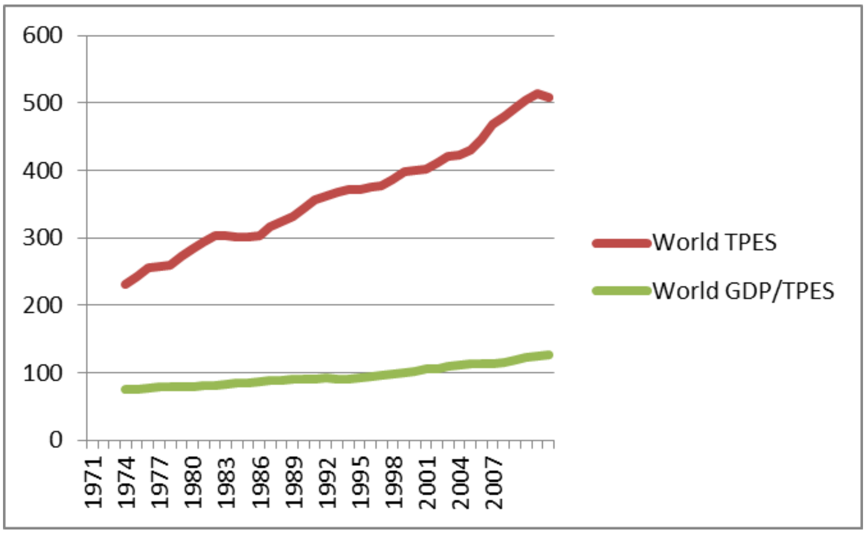

Figure 4. World TPES in Exajoules, World Product/TPES in Bn USD (PPP) per EJ. *See also* Appendix 1.

Four conditions must be met before the Granger causality test can be done: (1) The data set must be stationary, i.e. not show significant shifts in the mean and the variance over time. (2) An appropriate length of the time lag must be decided upon. (3) Unit roots should not be present over time. (4) There should be no or little autocorrelation. We briefly describe these tests in order.

3.2.1 Detrending

The issue of trend is one of the most common and simplest challenges in dealing with raw data in time series. In order for many statistical tests to be performed, the data being investigated must display a *stationary* or strongly stationary process. Stationarity is a quality whereby a time series, in particular, tends to carry a joint probability distribution wherein the average value and variance does not significantly change across the length of the data set. This is a problem for time series that involve growing or shrinking values over time – trends – such as *TPES* or *WP/TPES*. Our solution is to perform a detrending transformation through *first differencing*, i.e. simply changing the data from raw unit values into values that measure the difference between one value and the previous value to arrive at a data set that displays *changes in* the data for each variable rather than the data itself, as shown in **Figure 5**. It is in fact these changes in *WP/TPES* and *TPES*, and how they affect each other, that we are interested in.

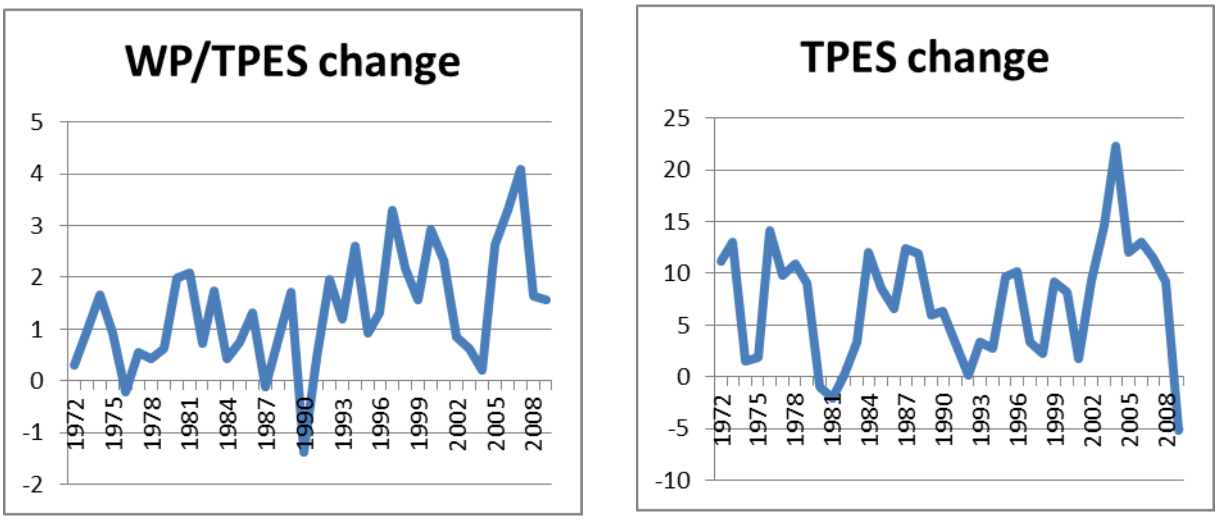

Figure 5. Changes per year in Bn USDs (PPP) per EJ, and EJs respectively. *See also* Appendix 2.

To better illustrate this concept, assume that G_T is a value in time series G at time T. To first-difference transform time series G into time series H, follow the formula below for every time T:

$H_t = G_t - G_{t-1}$

This method is easily applied in a spreadsheet application or in a statistical program such as *R* by recreating the equation above and applying it to every value. The only downside to *first* differencing is that the time series sample size is reduced by *one* value since the first value has no prior one from which to difference itself. Further degrees of differencing are of course possible, each time recalculating the difference between the value at time T and time T-1. To decide on the best degree of differencing one calculates the variance of the differenced time series as well as the original and chooses the one displaying minimal variance. In our case this was first differencing, which also fits well with the narrative of what is being investigated. Of course, with trend largely removed the problem of the presence of unit roots remains, dealt with in the Dickey-Fuller test in section (**3.2.3**) below.

3.2.2 Lag specification

Lag interval is a crucial value for forecasting when attempting a Granger causality test. Lag determines the order to which a time series variable will be shifted back in relative comparison to another, years in the current case because we are dealing with annual data. Specifically, change in total energy use (*TPES*) from a given year will be analyzed relative to the change in energy efficiency (*WP/TPES*) from some prior year. The independent variable should be lagged behind the dependent variable. The lag of suspected cause behind – that is, earlier than – suspected effect allows for study of the hypothesis that the independent variable (*WP/TPES*) can predict the future fluctuations in the dependent variable (*TPES*).

The *R* program *var.lag.specification* (Brandt) in package MSBVAR, based on the prevailing literature (Lutkepohl, 2004), was utilized to generate measurements based on the sample size and content. *See* **Appendix 3** for values of input and output. The program tests the suitability of each proposed lag length, here held to a reasonable maximum of 6, which can be evaluated using Hannan-Quinn (HIC), Bayesian (BIC), and Akaike (AIC) information criteria (Hannan & Quinn, 1979; Schwartz, 1978; Akaike, 1974). Results of the analysis indicate via all the criteria that a lag length of 1 is the best option. (NB: for the information criteria, lower values are preferable.) This lag length is a crucial value with significant impact on the output of the following calculations, and to help avoid statistical bias its selection must have the demonstrated empirical backing.

3.2.3 Unit roots/non-stationarity

With the detrended time series assigned an appropriate lag order of 1, an augmented Dickey-Fuller test to assess the presence of unit roots can be carried out with the null hypothesis that unit roots are present (Said & Dickey, 1984). The presence of unit roots is an indication of non-stationarity, and since the Granger Causality test requires a stationary process it is necessary to validate whether or not the first differencing has indeed transformed the original time series of raw values into a relatively stationary time series. If the null hypothesis can be rejected, it can be assumed with high certainty that the time series has been transformed into a stationary or strongly stationary process.

The *R* program *adf.test* (Trapletti) in package *tseries*, designed after best practices (Banerjee *et al*., 1993), was employed to run our unit root tests. Inputs and outputs for the procedure are found in **Appendix 4**. The purpose of this augmented version of the Dickey-Fuller test with lag order set to 1 as prescribed is to determine whether either of the specified time series, change in *TPES* or change in *WP/TPES*, is highly indicative of further increases *in its own value* beyond what is evidenced in the lag period, suggesting a strong trend. This would indicate a unit root and therefore the presence of non-stationarity. (Dickey & Fuller, 1979) A Dickey-Fuller value is returned with sufficiently negative values indicating a high degree of confidence that the null hypothesis of the presence of unit roots can be rejected, and this confidence is also relayed as a p-value. For changes in both the TPES and WP/TPES time series the Dickey-Fuller statistics are -3.7339 and -4.4242 respectively, both translating into a p-value below the significance level of .05, justifying rejection of the null hypothesis. We can proceed under the assumption that the time series data sets are unlikely to have serious issues with non-stationarity.

3.2.4 Autocorrelation

A Durbin-Watson autocorrelation (or ‘serial correlation’) test is undertaken as the last step before the Granger test itself. It looks at the degree to which values in a time series may be positively or negatively correlated to future values by examining the residuals over time. (Durbin & Watson, 1950; Durbin & Watson, 1951) Durbin-Watson is interested in fluctuations around a predicted regression fit line (residuals) and how they might correlate to one another. It should however be noted that the presence of autocorrelation is not in-and-of-itself disastrous since it can occur in data sets from which significant information can still be gleaned. (Durbin & Watson, 1971) However, heed must be paid to the type and strength of autocorrelation when interpreting the results, as it may weaken seemingly significant findings. Durbin-Watson test results are typically presented in the form of a Durbin-Watson statistic ranging in value between 0 and 4, with numbers less than 1 representing positive autocorrelation, numbers greater than 3 representing negative autocorrelation, and numbers in between representing minor or uncertain degrees of autocorrelation in one or the other direction. A value of exactly 2 represents absolutely no autocorrelation.

The *R* program *dwtest* (open authorship) in package *lmtest*, which was formulated to test the degree of autocorrelation in variables, including those in time series, was utilized to examine the degree of autocorrelation in changes in *TPES* while changes in *WP/TPES* are tested for their Granger-causality with a lag order of 1. Inputs and outputs are available in **Appendix 5**. A Durbin-Watson statistic of 1.1266 is returned, in between the neutral value (2) and the positive autocorrelation value (1), although closer to the latter. This indicates that there is enough evidence to reject the null hypothesis that autocorrelation is non-existent, but there is not enough evidence to prove a strong positive link. This result is not unexpected, since although first differencing has mostly detrended the data, there is still a slight upward slope in the change in *TPES* and slight downward slope in the change in *WP/TPES*, suggesting a gradual acceleration and deceleration respectively. Nonetheless, this Durbin-Watson value does raise concern that the ultimate Granger Causality test could be somewhat affected by autocorrelation, and this is a reason for hesitation when we interpret the potential significance of the results.

3.2.5 The Granger test itself

The Granger Causality test was developed as a means by which to demonstrate not only correlation, but also possible causation between two variables represented in time series through an investigation of whether one variable, when lagged behind a second, can accurately predict future values of this second variable through regression techniques. (Granger, 1969) The Granger test, however, can only be applied to stationary time series data of two variables maximum. The two variables in the current time series analysis have been adapted and verified to meet any preconditions. The stationarity of the data was tended to by a first differencing transformation of the original raw figures for *TPES* and *WP/TPES*, and validated via an Augmented Dickey-Fuller test for unit roots. Lag specification was employed to determine a proper lag period for the Dickey-Fuller Test and subsequent Durbin-Watson test for autocorrelation. The data appears to be applicable for the Granger test, with possible autocorrelation concerns in mind.

We employed a custom, non-packaged program written at the University of Illinois at Urbana-Champaign, the entirety of which has been included in **Appendix 6**. This program was selected for the transparency and robustness of function as well as clarity of output. It is run to determine whether the null-hypothesis can be rejected that a change in *WP/TPES* does *not* Granger-cause a change in *TPES*, or vice versa. The program is able to calculate the F-statistic and determine critical values for the given input parameters and values. Certain critical values associated with the inputs correspond to significant degrees of certainty, and the degree to which the F-stat value exceeds these critical values leads to the p-value. As such, it is ultimately the p-value by which the hypothesis is tested; F-statistics are provided only to allow for manual comparisons against critical value tables if desired.

For the first calculation, namely that a change in *WP/TPES* can cause a change in *TPES* – essentially the macro rebound effect being investigated – the test returned an F-statistic at 5.1 exceeding the critical threshold, and thereby a p-value below the .05 significance level at approximately 0.027, suggesting that the null hypothesis – that WP/TPES does *not* granger-cause TPES – can be rejected with some degree of confidence. The result, if accurate, suggests that there is a significant probability that changes in *WP/TPES* may cause changes in *TPES*. For the sake of thoroughness the reverse causality relationship was tested, but there was not significant evidence to be found of bi-directional causality (from *TPES* to *WP/TPES*) with an F-statistic of approximately 2.2 leading to a p-value of approximately 0.147, not enough to reject the null hypothesis.

4 Discussion

Caution is in order when interpreting the Granger Causality results, however promising they are for the hypothesis that a causal arrow runs from efficiency increase to more, not less, input consumption. Not only are there lingering concerns over the extent and implications of autocorrelation, but we have not tested for further variables that might cause changes in *TPES*, and we have not endogenised our independent variable *WP/TPES*. Our metrics themselves can moreover be questioned as to their adequacy in environmental research. We nevertheless believe our paper puts forward a new methodology to answer a seldom-asked empirical question at world scale. We try to keep our 'eyes on the prize', that is, only test energy efficiency for its effect on energy consumption. Narrowing down imperfections in the methodology, refining the metrics, adding more possible causal variables and endogenising them remains to be done.

More robust and complicated methods for assessing causality await researchers, for example vector autoregressive modeling such as the Toda-Yamomoto test (Toda & Yamamoto, 1995) which enables investigation of such relationships using multiple independent variables, for instance using open-source programs such as *R*. (Pfaff, 2008) Heightened specificity and accuracy are possible, particularly in detecting intermediate mechanisms within the rebound effect, for example efficiency's effects on population and GDPs. Polimeni's study (2009), while only on the basis of several regions, is undoubtedly more statistically robust and reliable than our efforts toward a global analysis, using at it does multiple-regression methods including both OLS and GARCH[8] time-series modeling. Our attempt is a relatively conservative, yet hopefully foundational, step in largely uncharted territory.

Our initial, rudimentary linear regression approach with an eye towards finding correlation led to two issues: that of *trend*, discussed in section 3.1.1 above, which interfered with a clean reading of the correlation; and the likelihood that even an involved correlation analysis would

[8] Polimeni used OLS to find "first-order correlation… among the disturbances" and a GARCH (1,1) to correct for possible "heteroscedasticity and autocorrelation." (2009, p 149)

yield little of academic value. With numerous possible intermediate mechanisms, which we hope to eventually tease out and quantify, even the strong correlation, taken alone, would only confirm what almost all researchers already know. Whilst the high co-efficient of correlation might shift the burden of proof from the task of proving rebound to the task of proving savings (i.e. that rebound is less than 100%), which after all have not materialised, we decided to test causality itself by means of a Granger test.

While the Granger results do suggest that efficiency increase causes increased consumption of the input used more efficiently – meaning that the real, universally attested rises in energy-input consumption are *not* higher than they would have been without the efficiency increases, as often claimed. (Howarth, 1997, p 3; Schipper & Grubb, 2000, p 370) Indicated is high rebound, but the Granger test cannot quantify it as a percentage of engineered savings. It also gives scant indication of the percentage of increase in energy consumption that might be caused by efficiency increase as opposed to other causal factors yet to be rigorously tested for, aside from the potential 24% proportion of response variation suggested by OLS analysis. Moreover, while the test results do show that change in *WP/TPES* may indeed alter the trend of change in *TPES*, we must still rely on theory to judge what *would have happened* had there been no increase in efficiency.

In terms of **Figure 2**, our data is consistent with society's indeed consuming all of the input that could be saved, at the *same level* of input and an *increase* in output. Until we test a full energy-consumption model including non-technological efficiency increases and population or labour-hours, we likewise do not know if there is backfire, i.e. rebound > 100%. Earlier research has shown that if greater efficiency does cause greater population, and affluence remains unchanged, we likely have backfire, or at least little hope of saving energy. (*see* Giampietro, 1994)

Increasing energy efficiency is of course the same as declining energy intensity of the world economy ('decoupling'), and while this has not yet been followed by any reduction in absolute amounts of environmental impact, the jury is still out on the direction of efficiency increase's causal arrow. We however go ahead and interpret both the raw data, e.g. in our Figures 3 and 4, as offering more support to the high-rebound rather than a low-rebound hypothesis of, say, < 50% of engineered savings.[9] **Figure 3** showed the big picture, with our dependent variable represented by 'Emissions' and our independent variable by 'Carbon intensity of GDP'. Bearing the burden of proof, In light of this high first-order correlation, we believe the burden of proof should rest with the low-rebound hypothesis which claims that efficiency in and of itself causes real savings. Note that that hypothesis must show what factors *do*, then, cause the undeniable increase in energy consumption, and even show that these have the strength to *counteract* the allegedly consumption-reducing effect of likewise empirically undeniable efficiency increases.

5 Conclusion: the efficiency strategy vs caps

In the Introduction we noted that there are direct, *necessarily effective* policies to reduce impact, the quantity on the left side of $I = PAT$. They define in physical terms the overall rates of depletion and levels of pollution deemed acceptable to society, then either cap them

[9] Even this low estimate of total rebound should replace the estimate used universally by national and international energy agencies – namely, zero. Khazzoom (1980) already proved that this estimate, which simply equates engineered savings with real savings, ignoring rebound altogether, is a crass scientific mistake.

at those levels or, to theoretically achieve the same result, tax them at an appropriately high rate (Weitzman, 1974; Daly, 1974; Tickell, 2008) – options we have not explored here in any detail. By contrast, right-side measures, including legislating technological efficiency, can only with a very large element of uncertainty be said to work in the direction of reduced impact. Certain is only that real savings cannot be in the same proportion as the efficiency increases or, derived from them by holding output constant, the theoretical quantity of engineered savings. We therefore advocate the implementation of caps or taxes without waiting to compute total rebound to the fourth decimal place.

To achieve the environmental goals of less depletion and pollution, the efficiency strategy is not *necessary*; if rebound is high, it is also not *sufficient.* However, the jury is still out on whether rebound is greater than or less than 100%, and thus whether efficiency increases might be sufficient for reaching environmental goals. We believe our statistical tests tend to support the hypothesis that rebound is around 100%. Our regression analyses show reasonable correlation between growth in energy efficiency and growth in energy consumption, while our Granger tests suggest that energy efficiency increases are drivers of energy consumption. Given the lack of scientific consensus on whether efficiency increases in and of themselves lower, raise, or leave unchanged the trend of energy consumption, and given that caps or high taxes meet with more political resistance than mandated efficiency increases, we hope our work will inspire others to perform deeper and more sophisticated empirical tests using our data and further plausible variables.

If the day ever comes when governments worldwide decide to limit the consumption of non-renewable resources, the rebound effect, in so far as it is of *environmental* concern, then becomes a non-issue. Technological efficiency as well as increased reliance on renewable energy – and on lifestyle changes in the direction of ‘sufficiency’ – would decentrally follow on the heels of the implemented caps or taxes because we will try to maintain our affluence within our shrunken energy budgets, but the tail (efficiency) cannot wag the dog (reduced fossil-fuel combustion). The demonstration that we can become more efficient can at most help win over voters to support direct measures to limit depletion. We believe rebound research shows the environmental efficacy of the efficiency strategy to be highly *uncertain*, whereas that of caps and taxes are absolutely certain.

References

Akaike H. 1974. A new look at the statistical model identification. *IEEE Transactions on Automatic Control* 19(6): 716-723.

Alcott B. 2005. Jevons’ Paradox. *Ecological Economics* 54(1): 9-21.

Alcott B. 2010. Impact caps: Why population, affluence and technology strategies should be abandoned. *Journal of Cleaner Production* 18(6): 552-560.

Alcott B. 2012. Mill’s Scissors: Structural change and the natural-resource inputs to labour. *Journal of Cleaner Production* 21(1): 83-92.

Banerjee A, Dolado JJ, Galbraith JW, Hendry DF. 1991. *Cointegration, Error Correction, and the Econometric Analysis of Non-Stationary Data.* Oxford U Press: Oxford.

Barker T, Dagoumas A, Rubin J. 2009. The macroeconomic rebound effect and the world economy. *Energy Efficiency* 2: 411-427.

Binswanger M. 2001. Technological progress and sustainable development: What about the rebound effect? *Ecological Economics* 36(1): 119-132.

Birol F, Keppler JH. 2000. Prices, technological development and the rebound effect. *Energy Policy* 28(6/7): 457-469.

Brandt PT, "var.lag.specification". In package: MSBVAR version 0.7-1. R open source programming language: freely available under GNU General Public License.

Brookes L. 1978. Energy policy, the energy price fallacy and the role of nuclear energy in the UK. *Energy Policy* 6(1): 94-106.

Brookes L. 1979. A low energy strategy for the UK. *Atom* 269 (March): 73-78.

Brookes L. 1990. The Greenhouse Effect: Fallacies in the energy efficiency solution. *Energy Policy* 18: 199–201.

Brookes L. 2000. Energy efficiency fallacies revisited. *Energy Policy* 28(6/7): 355– 366.

Daly H. 1974. The economics of the steady state. *American Economic Review* 64(2): 15-21.

Cleveland CJ, Costanza R, Hall CAS, Kaufmann R. 1984. Energy and the U.S. economy. *Science* 225(4665): 890-897.

Dickey DA, Fuller WA. 1979. Distribution of the Estimators for Autoregressive Time Series with a Unit Root. *Journal of the American Statistical Association* 74: 427-431.

Dimitropoulos J. 2007. Energy productivity improvements and the rebound effect: An overview of the state of knowledge. *Energy Policy* 35: 6354-6363.

Durbin J, Watson GS. 1950. Testing for Serial Correlation in Least Squares Regression, I. *Biometrika* 37: 409-428.

Durbin J, Watson GS. 1951. Testing for Serial Correlation in Least Squares Regression, II. *Biometrika* 38: 159–179.

Durbin J, Watson GS. 1970. Testing for Serial Correlation in Least Squares Regression, III. *Biometrika* 58: 1-19.

Fouquet R, Pearson PJG. 2006. Seven centuries of energy services. *Energy Journal* 27(1): 139-177.

Frondel M, Peters J, Vance C. 2008. Identifying the rebound: Evidence from a German household panel. *Energy Journal* 29(4): 145-163.

Giampietro M. 1994. Sustainability and technological development in agriculture. *BioScience* 44 (19): 677-689.

Granger CWJ. 1969. Investigating Causal Relations by Econometric Models and Cross-spectral Methods. *Econometrica* 37(3): 424-438.

Hanley N, McGregor PG, Swales JK, Turner K. 2009. Do increases in energy efficiency improve environmental quality and sustainability? *Ecological Economics* 68: 692-709.

Hannan EJ, Quinn BG. 1979. The Determination of the Order of an Autoregression. *Journal of the Royal Statistical Society B*, 41: 190-195.

Helm D, Smale R, Phillips J. 10.12.2007. Too good to be true? The UK's climate change record. http://www.dieterhelm.co.uk/node/656 15 September 2013.

Howarth R. 1997. Energy efficiency and economic growth. *Contemporary Economic Policy* XV(4): 1-9.

IEA (International Energy Agency). 2012. 2011 Highlights. http://www.iea.org/publications/freepublications/publication/co2highlights.pdf 15 September 2013.

Jevons WS. 1865/1965. *The Coal Question: An Inquiry Concerning the Progress of the Nation, and the Probable Exhaustion of Our Coal-mines*, 3rd edn, ed. Flux, A.W., 1905. Augustus M. Kelley: New York.

Khazzoom D. 1980. Economic implications of mandated efficiency in standards or household appliances. *Energy Journal* 1(4): 21– 40.

Krausmann F, Gingrich S, Eisenmenger N, Erb K-H, Haberl H, Fischer-Kowalski M. 2009. Growth in global materials use, GDP and population during the 20th century. *Ecological Economics* 68: 2696-2705.

Kümmel R. 2011. *The Second Law of Economics*. Springer: New York.

Lutkepohl H. 2004. Vector Autoregressive and Vector Error Correction Models. In *Applied Time Series Econometrics*, Lutkepohl H, Kratzig M (eds). Cambridge U Press: Cambridge.

Luzzati T, Orsini M. 2009. Investigating the energy-environmental Kuznets curve. *Energy* 34(2): 291-300.

Madlener R, Alcott B. 2009. Energy rebound and economic growth: A review of the main issues and research needs. *Energy* 34(2): 370-376.

Moezzi M. 2000. Decoupling energy efficiency from energy conservation. *Energy and Environment* 11(5): 521-537.

Peters GP. 2008. From production-based to consumption-based national emission inventories. *Ecological Economics* 65(1), 13-23.

Pfaff B. 2008. VAR, SVAR and SVEC Models: Implementation Within R Package vars. *Journal of Statistical Software* 27(4): 1-32.

Polimeni J. [2008] 2009. Empirical evidence for the Jevons Paradox. In *The Myth of Resource Efficiency: The Jevons Paradox*, Polimeni J, Mayumi K, Giampietro M, Alcott B (eds). Earthscan: London.

Roy J. 2000. The rebound effect: some empirical evidence from India. *Energy Policy* 28(6/7): 433–438.

Said SE, Dickey DA. 1984. Testing for Unit Roots in Autoregressive-Moving Average Models of Unknown Order. *Biometrika* 71: 599–607.

Sanne C. 2000. Dealing with environmental savings in a dynamical economy: How to stop chasing your tail in the pursuit of sustainability. *Energy Policy* 28(6/7): 487-495.

Saunders HD. 1992. The Khazzoom–Brookes postulate and neoclassical growth. *Energy Journal* 13(4): 131–148.

Saunders HD. 2000. A view from the macro side: Rebound, backfire and Khazzoom–Brookes. *Energy Policy* 28(6/7): 439–449.

Schipper L, Grubb M. 2000. On the rebound? Feedbacks between energy intensities and energy uses in IEA countries. *Energy Policy* 28(6/7): 367-388.

Schwartz GE. 1978. Estimating the dimension of a model. *Annals of Statistics* 6(2): 461-464.

Sorrell S, Dimitropoulos J, Sommerville M. 2009. Empirical estimates of the direct rebound effect: A review. *Energy Policy* 37(4): 1356–1371.

Sorrell S. 2009. Jevons' Paradox revisited: The evidence for backfire from improved energy efficiency. *Energy Policy* 37(4): 1456-1469.

Steinberger J, Krausmann F, Eisenmenger N. 2010. Global patterns of materials use: A socioeconomic and geophysical analysis. *Ecological Economics* 69: 1148-1158.

Stern DI. 2000. A multivariate cointegration analysis of the role of energy in the US macroeconomy. *Energy Economics* 22: 267-283.

Tainter J. 2008. Foreword to *The Myth of Resource Efficiency: The Jevons Paradox*, Polimeni J, Mayumi K, Giampietro M, Alcott B (eds). Earthscan: London.

Tickell O. 2008. *Kyoto2: How to Manage the Global Greenhouse*. Zed Books: London.

Toda HY, Yamamoto T. 1995. Statistical inference in vector autoregressions with possible integrated processes. *Journal of Econometrics* 66: 225–250.

Trapletti A. "adf.test". In package: tseries version 0.10-29. R open source programming language: freely available under GNU General Public License.

Turner K, Hanley N. 2011. Energy efficiency, rebound effects and the environmental Kuznets Curve. *Energy Economics* 33: 709-720.

Weitzman ML. 1974. Prices vs. quantities. *Review of Economic Studies* 41(4): 555-570.

Appendices

Appendix 1

Raw Data from IEA

Year	World GDP PPP Bn 2000 USDs	World TPES EJ	GDP/TPES Bn USD/PJ
1971	17449.755	231.6327	75.33373663
1972	18367.383	242.8225	75.64119002
1973	19602.824	255.8587	76.61582106
1974	20150.065	257.3808	78.28893511
1975	20540.786	259.2889	79.2197009
1976	21602.488	273.4697	78.99406937
1977	22532.475	283.2250	79.5568131
1978	23532.575	294.2000	79.98836231
1979	24451.325	303.2856	80.62144639
1980	24976.192	302.3436	82.6086323
1981	25431.678	300.2675	84.69674974
1982	25661.955	300.4430	85.41372493
1983	26479.920	303.8535	87.14700709
1984	27668.624	315.9320	87.57779492
1985	28661.677	324.5087	88.32329422
1986	29685.704	331.1218	89.65191298
1987	30760.001	343.5178	89.54412674
1988	32110.946	355.4557	90.33741468
1989	33269.554	361.3780	92.06302718
1990	33340.621	367.6963	90.6743382
1991	33798.809	370.8948	91.12775532
1992	34542.165	371.1076	93.07858047
1993	35300.154	374.4715	94.26660229
1994	36538.581	377.1971	96.86867401
1995	37834.165	386.9064	97.7863595
1996	39355.456	397.1109	99.10445425
1997	41002.500	400.4428	102.3929106
1998	42107.139	402.6746	104.5686595

1999	43703.996	411.8218	106.1235735
2000	45799.098	420.0136	109.0419292
2001	46977.068	421.8019	111.3723483
2002	48387.817	431.1469	112.2304659
2003	50304.988	445.7469	112.8554989
2004	52924.525	468.0649	113.0709189
2005	55547.241	480.0840	115.703168
2006	58677.667	493.0885	119.0002788
2007	62111.529	504.6330	123.0825706
2008	64095.275	513.8741	124.7295349
2009	64244.429	508.6897	126.2939408

Appendix 2

First Differenced GDP/TPES and TPES; *compare* **Appendix 7.**

	GDP/TPES change	TPES change
1972	0.307453396	11.1898259
1973	0.974631037	13.0361933
1974	1.673114053	1.522057
1975	0.930765792	1.9081087
1976	-0.22563153	14.1808841
1977	0.562743723	9.7552125
1978	0.431549215	10.9750277
1979	0.633084083	9.0856305
1980	1.987185909	-0.9420069
1981	2.08811744	-2.0761477
1982	0.716975191	0.1755301
1983	1.73328216	3.4104731
1984	0.430787831	12.0784912
1985	0.745499301	8.5767402
1986	1.328618753	6.6131178
1987	-0.107786237	12.3959819
1988	0.793287942	11.9378718
1989	1.725612501	5.9223477
1990	-1.388688985	6.318311
1991	0.453417126	3.1984568

1992	1.95082515	0.2127786
1993	1.188021819	3.363918
1994	2.602071718	2.725601
1995	0.917685492	9.7092858
1996	1.31809475	10.2045035
1997	3.288456394	3.331893
1998	2.175748811	2.2317981
1999	1.554914044	9.1471917
2000	2.918355725	8.1918913
2001	2.330419051	1.7882554
2002	0.858117652	9.3449968
2003	0.625032961	14.5999973
2004	0.215420007	22.3179773
2005	2.632249122	12.0191407
2006	3.297110785	13.0044723
2007	4.082291764	11.5445342
2008	1.646964316	9.2410625
2009	1.564405907	-5.1843631

Appendix 3

Lag Specification

```
> var.lag.specification(diffworld, lagmax = 6)

$ldets

     Lags  Log-Det     Chi^2   p-value
[1,]    6 2.049861 5.3517581 0.2530706
[2,]    5 2.331532 5.8447284 0.2110480
[3,]    4 2.609853 3.7861699 0.4357173
[4,]    3 2.774469 0.7230965 0.9484504
[5,]    2 2.803393 4.5168428 0.3405550
[6,]    1 2.970683 0.0000000 0.0000000

$results

     Lags      AIC      BIC       HQ
[1,]    1 3.345683 3.620509 3.436780
[2,]    2 3.428393 3.886435 3.580221
```

```
[3,]     3 3.649469 4.290728 3.862028
[4,]     4 3.734853 4.559329 4.008143
[5,]     5 3.706532 4.714226 4.040554
[6,]     6 3.674861 4.865771 4.069614


attr(,"class")
[1] "var.lag.specification"


//Lag length of 1 selected, as it has very lowest score in every
test for the reasonable lag lengths tested (1-6)
```

Appendix 4

Augmented Dickey Fuller Tests

```
> adf.test(TPES.change, alternative = c("stationary"), k = 1)

    Augmented Dickey-Fuller Test

data:  TPES.change

Dickey-Fuller = -3.7339, Lag order = 1, p-value = 0.03605

alternative hypothesis: stationary

> adf.test(GDP.TPES.change, alternative = c("stationary"), k = 1)

    Augmented Dickey-Fuller Test

data:  GDP.TPES.change

Dickey-Fuller = -4.4242, Lag order = 1, p-value = 0.01

alternative hypothesis: stationary
```

Appendix 5

Durbin-Watson Test

```
> dwtest(GDP.TPES.change ~ TPES.change)

	Durbin-Watson test

data:  GDP.TPES.change ~ TPES.change

DW = 1.1266, p-value = 0.001738

alternative hypothesis: true autocorrelation is greater than 0
```

//There may be a slight element of positive autocorrelation in the independent variable GDP.TPES.chance, but the Durbin-Watson statistic is still above the critical value of 1, and therefore significant positive autocorrelation of residuals is not certain.

Appendix 6

Granger Causality Test

```
"granger" <-function(d, L, k = 1)
{
of d[,1] and d[,2].
names.d <- dimnames(d)[[2]]
D <- d
for(i in 1:L)
{
D <-ts.intersect(D, lag(d,  - i))
}
dimnames(D)[[2]] <- paste(rep(names.d, L + 1), "_", rep(0:L, times = rep(2, L + 1)), sep = "")
y  <- D[, k]
n  <- length(y)
x1 <- D[,  - (1:2)]
x0 <- x1[, ((1:L) * 2) - (k %% 2)]
z1 <- lm(y ~ x1)
z0 <- lm(y ~ x0)
S1 <- sum(z1$resid^2)
```

```
S0 <- sum(z0$resid^2)
ftest <- ((S0 - S1)/L)/(S1/(n - 2 * L - 1))
list(ftest = ftest, p.val = 1 - pf(ftest, L, n - 2 * L - 1),
R2 = summary(z1)$r.squared)
}
> granger(cbind(TPES.change, GDP.TPES.change), L=1)
$ftest
[1] 5.315634
$p.val
[1] 0.02717995
$R2
[1] 1

> granger(cbind(GDP.TPES.change, TPES.change), L=1)
$ftest
[1] 2.197268
$p.val
[1] 0.1472043
$R2
[1] 1
```

Appendix 7

Simple OLS regression of TPES on GWP/TPES; *compare* **Appendix 2**.

INDEP VAR	DEP VAR	E as % of B eg E3/B2x100	F as % of C eg F3/C2x100
75.33372879	231.6327	i.e. Δindep var **in percent**	i.e. Δdep var **in percent**
75.64119058	242.8225	0.408132983	4.830837788
76.61581959	255.8587	1.288489779	5.368612876
78.28892054	257.3808	2.183753902	0.594898669
79.21968893	259.2889	1.188889026	0.74135289
78.99408234	273.4697	-0.284786000	5.469111867
79.55681310	283.2250	0.712370776	3.567232494
79.98835826	294.2000	0.542436459	3.875011034
80.62145054	303.2856	0.791480528	3.088239293
82.60863468	302.3436	2.464833026	-0.310598327
84.69673874	300.2675	2.527706783	-0.686669075
85.41372240	300.4430	0.846530424	0.058447884
87.14699683	303.8535	2.029269272	1.135157085

87.57778256	315.9320	0.494320804	3.975106425
88.32329303	324.5087	0.851255244	2.714729752
89.65191661	331.1218	1.504273146	2.037880649
89.54412552	343.5178	-0.12023289	3.743637538
90.33741468	355.4557	0.88591983	3.475191096
92.06303095	361.3780	1.910190014	1.666114793
90.67434456	367.6963	-1.508408292	1.748390882
91.12775105	370.8948	0.500038341	0.869875492
93.07857074	371.1076	2.140752593	0.05737476
94.26659706	374.4715	1.276369313	0.906448696
96.86866893	377.1971	2.760332876	0.727852453
97.78635091	386.9064	0.947346541	2.574065389
99.10444664	397.1109	1.347934266	2.637459603
102.3929011	400.4428	3.318170403	0.839035141
104.5686492	402.6746	2.124901381	0.557333032
106.1235612	411.8218	1.48697721	2.271610874
109.0419405	420.0136	2.749982476	1.989161331
111.3723480	421.8019	2.137166173	0.425771927
112.2304648	431.1469	0.770493608	2.215494999
112.8554971	445.7469	0.556918577	3.386316821
113.0709117	468.0649	0.190876483	5.006877221
115.7031707	480.0840	2.327971807	2.567827667
119.0002748	493.0885	2.849622948	2.708796794
123.0825749	504.6330	3.430496333	2.341263282
124.7295301	513.8741	1.338089648	1.831251622
126.2939450	508.6897	1.254245853	-1.00888525
	0.982613424		-0.489098129
	(R for B & C)		(R for H & I)
	0.965529141		0.23921698
	(R2 for B&C)		(R2 for H&I)

Where Y = Column F and X = Column Y, the Line Fit and Residuals present this picture: