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Clarity or confusion? – problems in attributing large-scale ecological changes to anthropogenic drivers

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Abstract

Ways of reducing the drivers of global biodiversity loss and degradation of ecosystem services are needed more than ever before. Policy options must be based on the best evidence of the role of multiple driving forces. Increasingly, a significant part of the evidence base comes from attributing signals of biological change detected in large-scale analytical surveys to a range of possible causal factors. We highlight a number of subtle difficulties that can beset the challenge of detecting such correlative relationships. These are as follows: 1. The Modifiable Area Unit Problem. 2. Incomplete explanatory variable data. 3. Lack of control over the replication and crossing of driving variables. In most cases these problems can be avoided by more careful specification of the scientific question and application of relatively new analytical techniques. Ignoring them can lead to mis-specification of hypothesised

driver-state-impact relationships and flawed conclusions as to the most important causes of change.

1. Introduction

On 29th October 2010, the parties to the UN Convention on Biological Diversity agreed a new 10 year Strategic Plan. This followed failure to meet the Rio Earth Summit goal "to achieve by 2010 a significant reduction of the current rate of biodiversity loss at the global, regional and national level as a contribution to poverty alleviation and to the benefit of all life on Earth" (Butchart *et al*. 2010). Action is needed now more than ever to halt the direct drivers of biodiversity loss and degradation of ecosystem services. This in turn means understanding how the relative importance of different drivers varies across a heterogenous planet. Increasingly, this understanding comes from analysis of the relationship between potential causal factors, such as atmospheric pollutant deposition, climate change and land-use and ecological state indicators (Sala *et al.* 2000; MA 2005). Such research takes time yet, in a rapidly changing world, answers are needed quickly. Policy makers therefore find themselves under pressure to infer possible causes from indicator change in the absence of the evidence base that might emerge from time-consuming statistical and model-based attribution analyses supported by experimentation to establish the plausibility of the underlying mechanisms (Gadbury & Schreuder 2003). Sidestepping the analytical attribution phase is attractive because changes in indicators can appear to be superficially consistent with known changes in driving variables. This invites common-sense interpretations; if on average, the ecological indicator has increased, decreased or remained stable and, on average, the driver has intensified, reduced or not changed, then the two are likely to be causally linked (Bellamy *et al.* 2005*;*

Butchart *et al.* 2010). While demonstrations of these simple and consistent relationships are vital warning signs in the assessment of possible driver-state-impact relationships, in themselves they offer incomplete evidence if the goal is to unravel the relative contributions of multiple drivers across a domain of interest. Although the apparent clarity and policy resonance of the message increases with the size of the spatial units across which indicators are averaged, such high-level reporting can obscure the existence and directions of ecosystem-specific trajectories of change linked to different human-induced drivers (Jelinski & Wu 1996; Anderson *et al.* 2009). A step toward quantification of these relationships comes from analysis of change in state indicators recorded from large-scale ecological surveillance programs. Whilst such analyses maximise the realism and policy relevance of results, they must also contend with a number of problems that serve to separate the detection and attribution of signals in surveillance data from partitioning of the variation in a designed experiment where the observer has control over the identity and arrangement of treatments and covariates (Stow *et al.* 1998; Biggs *et al.* 2009; Wintle *et al.* 2010).

Here we illustrate some of the dangers of over-simplification in the presentation, interpretation and analysis of large-scale ecological change and discuss a number of related problems that are particularly relevant to the identification of cause-effect hypotheses in ecological surveillance data. As a new round of global target setting, indicator development and monitoring begins it is timely to highlight the influence of these problems on the analysis of the impacts of anthropogenic stressors on biodiversity and ecosystem services. By ecological surveillance data we mean large-scale surveys repeated or not, where the primary motivation of the sampling design is to quantify the range of ecological or biological variation in a region and the way it may change over time (Gadbury & Schreuder 2003; Haughland *et al.* 2009). We exclude long-term experiments where ecological change results from designed arrangements of treatments and controls (Lindenmayer & Likens 2009).

2. Three important issues

2.1 The Modifiable Area Unit Problem

Across a heterogenous sampling domain, the relationships between variables can change simply by altering the orientation and size of the areal units measured. In their classic demonstration, Openshaw & Taylor (1977) found that correlations between proportions of elderly voters and Republican voters across the state of Iowa ranged from highly positive to highly negative simply by changing the number and orientation of areal units used to divide up the state. Likewise, by changing the scale over which indicators are averaged, the same phenomenon can readily influence the apparent direction of ecological driver-state-impact relationships (Armhein 1995; Stein *et al.* 2001). We illustrate the problem using recently published ecological surveillance data for Great Britain.

In Britain, national changes in a range of biodiversity indicators have been reported for the 29 year period from 1978 to 2007 based on large-scale ecological surveys repeated at roughly decadal intervals (Carey *et al.* 2008). Each survey collects a large number of biophysical measurements and these have been published as indicators of change in extent and condition of major habitats, freshwaters, soils and plant species (Carey *et al.* 2008 and available on-line at www.countrysidesurvey.org.uk). Following the latest survey in 2007, the results were summarised by ecosystem type and at the national-scale (Carey *et al.* 2008; CEH, 2008). Particular interest focussed on changes in plant species composition that could be interpreted as shifts along the nutrient availability gradient and therefore as a signal of eutrophication. Attribution analyses of previous surveillance data highlighted the separate impacts of intensive agriculture, nutrient surpluses and atmospheric nitrogen deposition on characteristic species diversity between 1978 and 1998 (Firbank *et al.* 2008; Smart *et al.* 2006a; Smart *et al.* 2004). Hence, key questions were whether a signal of increasing eutrophication had continued across British terrestrial ecosystems up to 2007, and if so, what were the likely drivers? Expressing between-survey change in the nutrient status indicator as the cross-ecosystem national average gave the simplest possible answer to the first question: The relative contribution of the more nutrient-demanding plant species increased and then decreased over the 29 year period (Fig. 1). At this scale, the indicator has maximum impact with policy makers because it is communicated as one simple, spatially unified message (CEH 2008). Within-ecosystem changes were also published separately (Carey *et al.* 2008). These showed overall reductions in the indicator in woodland and intensive farmland, but net increases in less fertile semi-natural habitats such as acid, calcareous and neutral grasslands (Fig 2). Because the time series is based on a representative, stratified-random sample of land cover, the national trend was a weighted average of the directions and sizes of change observed across the mosaic of habitats in Britain (Fig 1). The national indicator was correct. It reflected the aggregate pattern of change along the inferred nutrient availability axis but did not convey the net increase in the indicator in heath & bog and the semi-natural grasslands (Fig 2). The national average trend also diverted attention from the fact that different drivers were likely to have impacted different ecosystems and that these different ecosystems vary in conservation value and response to perturbation. For example, the extensification signal on arable land (Fig 2) appeared to reflect a substantial transfer from cultivation to fallow in response to the domestic implementation of Europe-wide setaside mechanisms designed to reduce crop production (Boatman *et al.* 2009). In these highly responsive systems post-disturbance colonisation is rapid (Critchley & Fowbert, 2000). However, the duration of any positive ecosystem effects on soil protection, carbon sequestration and resource availability for invertebrates and farmland birds are dependent on changes in crop prices and can be abruptly reduced if fallow land is returned to intensive arable cropping as happened in Britain in 2008 (DEFRA 2008; Boatman *et al.* 2009). In less resilient semi-natural vegetation, eutrophication can be a much more persistent effect; hard to reverse and cumulative in its impact (Strengbom *et al.* 2001; Dupouey *et al.* 2002). Only when the national trend was disaggregated was it possible to discriminate between divergent ecosystem-specific trends, each associated with different suites of known or possible drivers (Fig 2). Rather than concluding that the same pattern of change occurred across the British landscape and that the most recent period saw widespread recovery, a more detailed assessment revealed an ecosystem-specific mixture of extensification, stability and eutrophication over the 29 year period (Fig 2).

Decisions about the definition and size of sampling units also impact greatly on the detection and meaningfulness of patterns that purport to help understand ecosystem service delivery and the spatial partitioning of biodiversity. Analyses that have sought to identify trade-offs in ecosystem service provision have, for example, shown marked scale-dependence in the direction of correlations (Anderson *et al.* 2009; Naidoo *et al.* 2008). In addition, the measurement of change in species diversity is highly scale-dependent since increasing the size of the units censused confounds beta-diversity with alpha-diversity (Buckland *et al.* 2005; Huston 1999). Partitioning diversity at appropriate scales is especially important if the processes and drivers of change in biodiversity are to be properly understood (Huston 1999; Green *et al.* 2005; Gabriel *et al.* 2010). For example, regional to larger-scale reductions in rare species (gamma diversity) have often occurred in parallel with local increases in small-scale alpha-diversity (Sax & Gaines 2003). While both patterns can be reconciled with the effect of human activities that increase the range of generalists and exotics but reduce the range of scarce specialists, (Olden & Poff 2003; Smart *et al.* 2006b) analysis is required at more than one scale to fully characterise these inter-related trends (Weber *et al.* 2004; Stohlgren *et al.* 2002). Summing changes over fewer larger areas risks averaging out opposing directions of ecological change whose individual trajectories may be linked to local ecosystem-specific sets of drivers and starting conditions (Wright & Jones 2004; Smart *et al.* 2006b).

2.2 Incomplete or missing explanatory variables

Averaging across areal units of different sizes is not the only way in which important relationships with driving variables can be concealed. Previous attribution analyses of British data have shown that large-scale ecosystem impacts can be correlated with spatial gradients of multiple global change phenomena (e.g. Firbank *et al.* 2008; Maskell *et al.* 2010). Yet, if a driver has increased in one place but decreased to an equal extent in another then a cross-ecosystem average could convey no significant mean change in the indicator rather than significant but location-dependent correlation. The same could potentially happen where the impacts of two drivers are negatively correlated in their operation within an ecosystem. This is the same as the absence of significant main effects in an experiment because of the presence of a negative interaction between levels of those main effects (Underwood 2005; Biggs *et al.* 2009a). Such averaging problems will arise where explanatory variables have been omitted from analyses. Therefore lack of an observed change in an indicator averaged across a domain does not mean the absence of regional driver-state-impact dynamics. The challenge is to incorporate appropriate data on covariates and driving variables sufficient to isolate important interactions and main effects for example reflecting geographical variation in starting conditions, land-use and socio-economic context (Van Buskirk & Willi, 2004, 2005; Kleijn & Báldi, 2005) or across species' centres of distribution versus range margins (Oliver *et al.* 2009; Warren *et al.* 2001). An ongoing problem is that in regions where biodiversity is high yet ecosystem degradation severe, adequate explanatory variable data may be sparse or non-existent (MA, 2005).

Analysis of recent large-scale changes in disease prevalence provide some of the best examples of how spatial disaggregation of data can reveal smaller scale trends each associated with different suites of social and ecological covariates. Increased risk of tick-borne encephalitis (TBE) to humans has occurred across parts of Europe in the last thirty years. Increased disease risk was thought to have resulted from improved conditions for natural transmission cycles resulting in higher densities of infected ticks or from changes in human behaviour resulting in greater exposure to ticks (Randolph 2008a). Since both of these factors are climate dependent, TBE is commonly listed amongst those vector-borne pathogens anticipated to become more of a threat to humans as the climate warms (e.g. Lindgren, 1998). Lindgren & Gustafson (2001) were the first to attribute the upsurge in TBE incidence over the past two decades to warming temperatures – namely milder winters and early arrival of spring (conditions favouring early development and extended autumn activity of ticks). However, this study considered only climatic drivers and was restricted to Stockholm county, Sweden, close to the northern range margin of this pathogen. In fact, when a regional level investigation at a pan-european scale was conducted, considerable variation was revealed, both within and between countries, in the timing and extent of changes in TBE incidence. Though biologically-relevant climate warming had indeed occurred since the early 1990s, and may have enhanced transmission, the pattern of these changes was too uniform across the continent to account for the extreme spatio-temporal heterogeneity in the upsurge of TBE (Sumilo *et al.* 2006; 2007; Randolph 2008a; 2008b). Instead a network of interacting biotic and socio-political drivers affecting both risk of infection and exposure of humans, differing in force in space and time, was involved (Randolph 2008a).

A similarly comprehensive analysis invoking multiple and interacting drivers was also necessary to understand the increasing incidence of American cutaneous leishmaniasis (ACL), (a zoonotic vector-borne disease, caused by several species of *Leishmania* and transmitted by sandflies) in the neotropics. In this example inclusion of another covariate revealed an interaction with the primary driver that reversed the apparent direction of the initial driver-state-impact relationship. The emergence of ACL has been associated with changes in the interactions between people and forests. The association between outbreaks and higher rates of infection found in populations living close to forests and lower incidence in urban areas led to the proposal that deforestation could reduce re-emergence of disease. These analyses ignored socio-economic factors. Chaves *et al.* (2008) examined county level incidence rates as a function of social and environmental variables. In common with other infectious diseases, socially excluded populations were most affected by the disease. Once social marginality was taken into account, living close to the forest could actually diminish the risk of ACL infection. In addition, impacts of climate change on disease, due to El Nino Southern Oscillation events (initially described in Chaves *et al.* 2006), interacted with forest cover since resulting increases in incidence were exacerbated by higher levels of deforestation. Including socio-economic covariates thus reversed the original expectation. Poorer citizens were more likely to contract ACL whether in urban or rural areas but were more likely to live closer to forest edges. While overall incidence increased with climate effects, incidence declined with greater remaining forest cover.

Complex relationships between multiple driving variables and responses are likely to be common. Unravelling them may face a simple problem of lack of data that track drivers of interest at the study scale. This can be an insurmountable obstacle (e.g. MA, 2005) in the face of which, the identification of causal hypotheses based on those data that are available, must consider the possibility that other drivers could be equally important and that, if included, they may even alter the direction of the relationship with the principal driver (see Supplementary Material).

2.3 Arrangement of drivers is beyond the control of the observer

A number of analytical and interpretative problems can also arise because of an under-appreciation of the differences between analysis of correlative/causal relationships in ecological surveillance data versus partitioning the variation in a designed experiment (Stow *et al.* 1998). Unlike a designed experiment, the identity, crossing, replication and interspersion of driving variables is, by definition, outside the control of the observer (Stow *et al.* 1998; Gadbury & Schreuder 2003). This makes attribution an analytical challenge since we must apply strong inference (*sensu*

Platt 1964) to weak data. That is we apply the steps involved in inductive inference especially hypothesis construction and testing but to data that reflects accidental arrangements of possible explanatory variables. This creates difficulties in testing competing hypotheses and in gaining clear understanding from the analyses.

Analyses of large-scale survey and monitoring data face well known issues such as regression to the mean (Palmer 1993; Smart & Scott 2004; Biggs *et al.* 2009), pseudo-replication (Hurlbert 1984; Cottenie & De Meester 2003) and spatial autocorrelation (Beale et al 2010; Hawkins et al 2007). Less widely discussed is the influence of differences in the range of variation in driver intensity. This can profoundly affect the conclusions drawn from analyses that seek to detect driver-state-impact relationships. In a large-scale analytical survey potential drivers may show considerable spatial variation in their intensity. Analyses may have high realism but the apparent importance of one driver versus another in driving change, maybe as much to do with the fact that the severity of a driver has been consistently high in most places, while the severity of another driver varies being high in some places but being absent or at low severity in others. If the range of variation in a driver is relatively small despite its severity being high everywhere, then variance partitioning techniques have a lesser chance of detecting a significant effect of the driver over and above residual variation in the response. We carried out a simulation exercise to demonstrate this effect (Fig 3). The driving variable ranged between 1 and 40, approximating the current range of total atmospheric nitrogen deposition (Kg N ha^{-1} yr^{-1}) across Britain as a realistic example of a national-scale driver of ecological change. A realistic relationship with a response variable was specified and the power of detecting a significant relationship tested across different ranges of variation in the driver (Fig 3). The conclusion from such a test is not necessarily that the driver has not or could not induce an ecological impact. If the regression slope coefficients of two driver-impact relationships were equivalent, then their potential for forcing ecological change is estimated to be equivalent if they were applied with equal crossing and replication. The problem can be made clearer by acknowledging that two questions are involved; 1) what is the inherent potency of the driver as a cause of ecological change relative to other potential drivers (best answered via experiment and up-scaled via a model), and, 2) which drivers happen to be most strongly correlated with observed ecological change across the entirety of a region of interest? Statistical attribution relies on being able to evaluate the size of a driver's impact by comparing ecological changes at high and low levels of a driver; either along a gradient or by classifying the sampling domain into control and impacted regions. Taking into account the relative range of variation in possible drivers across the sampling domain is therefore of equal importance in answering the second question as is quantifying variation in the ecological impact. If a driver had operated with constant and high severity everywhere and an associated magnitude of impact had occurred everywhere then the only deviations from the mean of the response will be random about the mean overall level of the explanatory variable. This rules out the possibility of a systematic association between the two and hence a significant detected effect of the driver on the response (Fig 3). It also means that a highly significant average change in a state indicator may defy statistical attribution based on variance partitioning techniques even if the potential driver seems very obvious. Analysis of the ecosystem impacts of reduced versus oxidised atmospheric nitrogen deposition provides a topical example of the importance of separating assessment of the inherent potency of two drivers from an assessment of their relative importance as an additional consequence of their spatial variation in severity. At the resolution of larger grid cells, reduced nitrogen deposits over a much greater range of estimable values across Europe than oxidised. This means that attribution of large-scale change in state indicators to reduced nitrogen is more likely than to oxidised (e.g. Smart *et al.* 2004; McClean *et al.* 2011) even though the biogeochemical mechanisms underlying their impacts, indicate that both can be significant pollutants (Stevens *et al.* 2011). Reanalysis of datasets based on equalising gradient lengths is possible but the constrained subsample of a large-scale survey would no longer represent the realistic, un-designed variation in each nitrogen form as a function of the human activities that control the deposition of each.

3. Conclusions

While the principal objective of many surveillance and monitoring programs is to detect change, the question, why has change occurred?, inevitably follows. Yet, just because schemes are designed to detect change across a region does not mean they are optimised to attribute changes to potential drivers. The basic difference between controlled experimentation and passive ecological surveillance contributes to all three of the problems we have illustrated. We make the following recommendations:

Be wary of narratives that analyse large-scale change in indicators and then invoke drivers operating across large areas but without quantitatively linking them: The statistical power of such an analysis can be high leading to a powerfully simple message but one that may conceal important local differences. High-level results should therefore, be accompanied by an appropriate decomposition into disaggregated trends and a health warning that the simple picture is simple at the expense of potentially critical complexity. Techniques such as random forests (Bradter *et al.*

2011), locally-weighted regression and CART modelling can be used to detect and characterise these variations in ecosystem response thus highlighting the locations of different driver-state-impact dynamics for further hypothesis testing (Smart *et al.* 2003). Techniques such as hierarchical Bayesian modelling (Biggs *et al.* 2009) allow joint analyses of these substrata enabling the detection of overall correlative/causal relationships between drivers and state indicators but conditioned on environmental heterogeneity. Such approaches help avoid excessive belief in a single simplistic unified message about the causes of ecological change. Similar issues of scale-dependence and aggregation or averaging effects on the clarity of higher level messages have also been discussed regarding the calculation and dissemination of environmental sustainability indicators (Morse & Fraser 2005; Parris & Kates 2003).

Analyses of driver-state-impact relationships from large-scale ecological surveillance have high realism and policy relevance. However, the ‘accidental’ way in which drivers are spatially organised means that detection of relationships can be as much an indication of the lack of equal crossing and replication of possible causal factors as of the inherent potency of a driver to cause change. For example to what extent does one driver emerge as more important than another just because it had operated with greater variation in severity across the study region?

With sufficiently large datasets it may be possible to analyse subsets of replicated data where variation in a driver is maximised along a gradient or between impacted and control groups. Other drivers could be equivalently crossed and replicated or held constant along with other additional covariates, such as ecosystem type. However, such designed sub-sampling changes the question. Results will no longer realistically represent the accidental interplay of the range of drivers across the entire sampling domain. Such studies provide the basis for quantifiying the impact of a driver but over a domain deliberately engineered to maximise the magnitude of the driver-state-impact signal. These approaches are therefore most useful in deriving realistic statistical models of driver-impact relationships (Maskell *et al.* 2010; Stevens *et al.* 2009). Predicted impacts can then be usefully compared with observations in other parts of the sampling domain where other drivers may be operating (e.g. Diekman & Falkengren-Grerup 2002; Stevens *et al.* 2004).

Finally, we should be aware that the emerging importance of a driver may simply reflect data availability: If data is only available that tracks one driver but an ecological impact could be more fully explained by the main or interacting effect of other drivers then conclusions need to be accompanied by a qualification that analysis is incomplete. Differences in data quality can also be influential. For example more coarsely resolved explanatory variables cannot explain as much variation as those more closely matched to the resolution of the ecological response data (Smart et al 2006a). Again, this may mean that some drivers appear more important in explaining change in ecological state indicators than others.

Whilst the problems we have described are simple to understand, each demands a cautionary perspective which may be easily overlooked in the rush to interpret indicator change in the light of apparently obvious changes in the most likely drivers. Reducing the decline in global biodiversity and the degradation of ecosystem services is both costly and vital. If cost-effective responses are to be formulated they must be based on an accurate understanding of the role of multiple drivers and the way they interact to cause change. Analyses of ecological surveillance data should support the elucidation of causal-correlative links between drivers and state variables. To do this we believe that scientists and policy consumers need to be more aware of the implications of the simple yet subtle problems we have highlighted.

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Figure 1. Change in the vegetation nutrient status indicator across British ecosystems between the Countryside Surveys of Great Britain in 1978, 1990, 1998 and 2007. The score was derived from the mean of individual indicator (Ellenberg N) values assigned to species in the British flora, for species that were found in fixed sampling plots in each survey year. Error bars are the 95% confidence intervals on the within-year means (n=729 repeat plots). Statistically significant changes in mean score occurred between all pairs of years except 1978 to 2007 and 1990 to 1998, based on an autoregressive, generalised linear mixed model. An increase in the index indicates greater representation of more nutrient-demanding species. The scale is such that 1 indicates species associated with the least, and 9, with the most productive conditions.

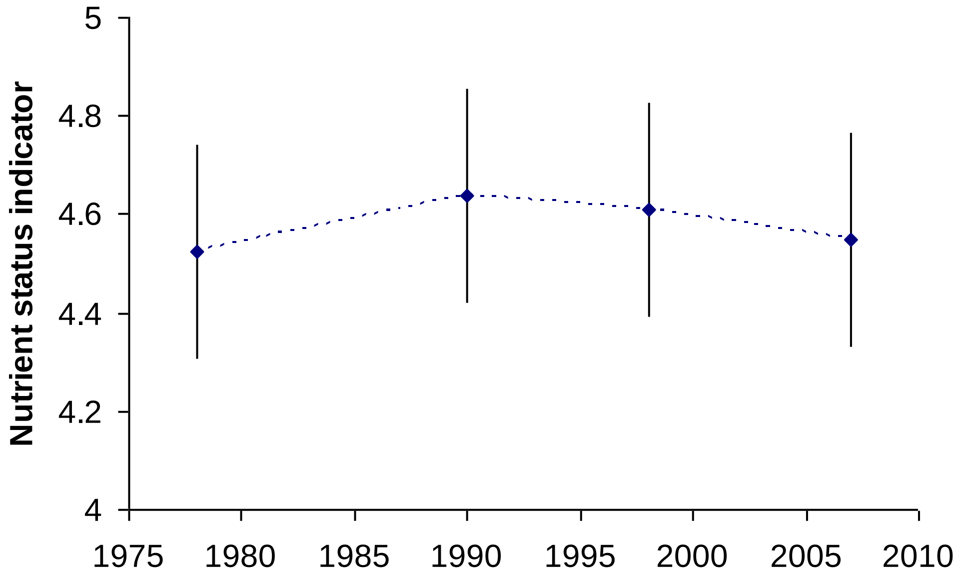
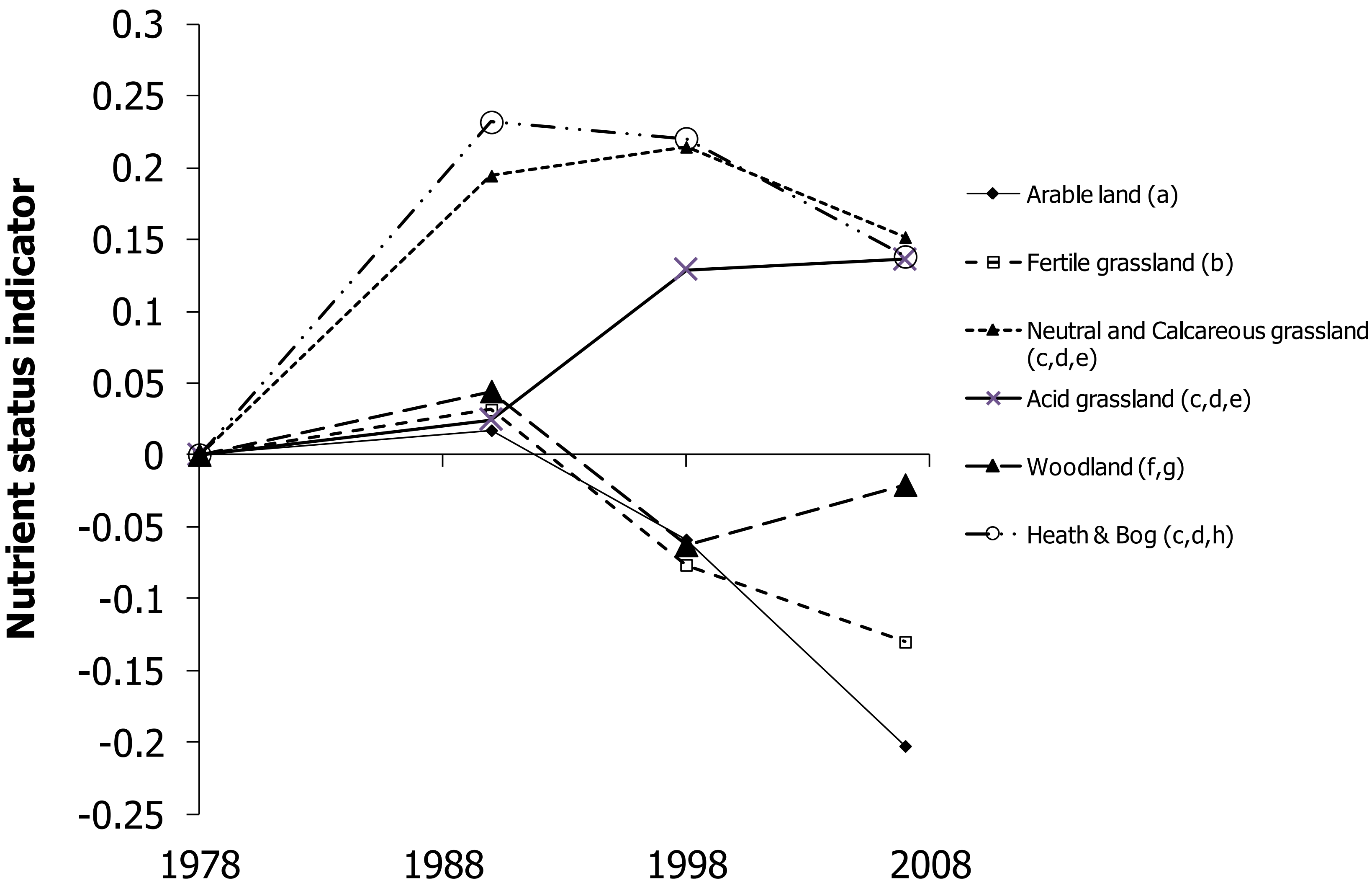
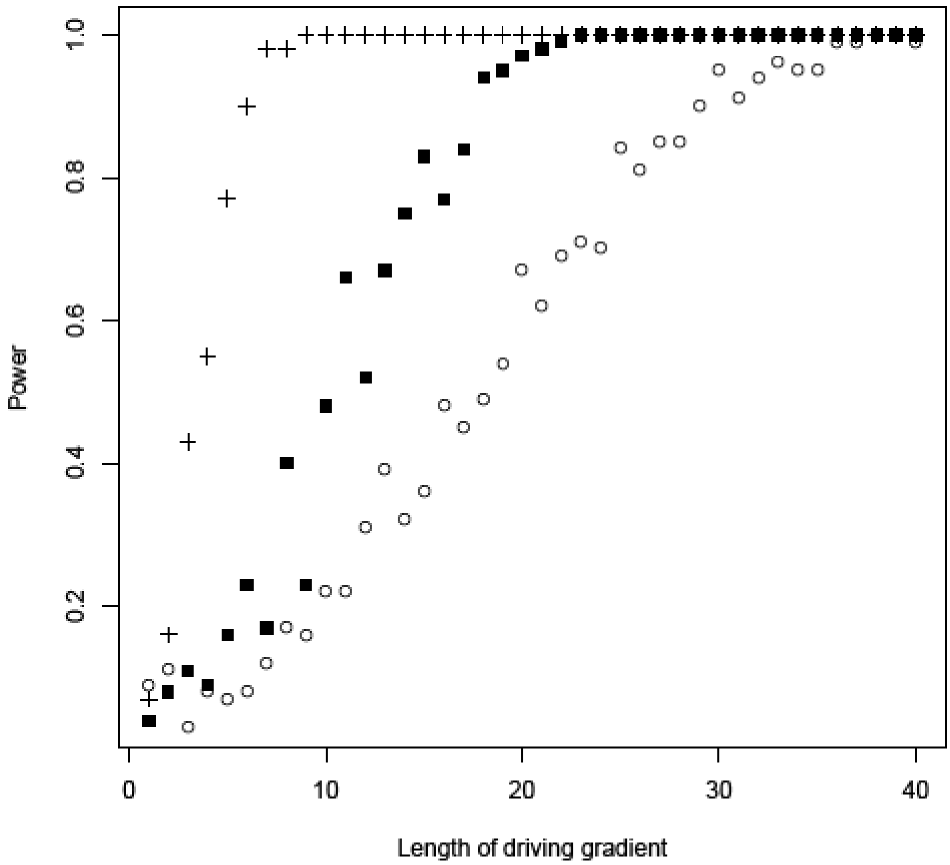


Figure 2. Change in the vegetation nutrient status indicator (see Fig 1) decomposed by ecosystem type. Values are the difference in the indicator from the starting value in the 1978 survey where all ecosystem types start at zero. Letters after each ecosystem type refer to known or hypothesised large-scale drivers of change in the ecosystem type over the survey period as follows; a) removal of land from production post-1988 (Carey *et al.* 2008; Critchley & Fowbert 2000); b) reduced management intensity post-1990 (Carey *et al.* 2008; Smart *et al* 2005); c) atmospheric nitrogen deposition (Smart *et al.* 2004; Maskell *et al.* 2010); d) abandonment (Sketch 1995); e) agricultural intensification (Smart *et al* 2006a; Chamberlain *et al.* 2000), f) woodland succession (Kirby *et al.* 2005), g) lack of traditional management (Kirby *et al.* 2005), h) over-grazing (Fuller & Gough 1999).


Nutrient status indicator
0.3
0.25
0.2
0.15
0.1
0.05
0
-0.05
-0.1
-0.15
-0.2
-0.25
1978
1988
1998
2008
Arable land (a)
Fertile grassland (b)
Neutral and Calcareous grassland (c,d,e)
Acid grassland (c,d,e)
Woodland (f,g)
Heath & Bog (c,d,h)

1

Figure 3. Effect of the range of variation in a driver of ecological change on the probability of detecting a significant correlation with an ecological indicator. Data were simulated based on the linear regression model y=0.7*x, with random residual error $e \sim N(0, \sigma^2)$ where σ^2 was set to each of three values; + = 5, ■ = 15, ○ = 25. For each error variance, forty datasets were generated each of which consisted of 200 values but with x made to vary in range within each dataset between 40 and 1 but always with a maximum value of 40. The slope parameter was therefore the same in each dataset but the range of variation over which non-random deviations from the mean response were quantified, varied from large to small. The power of detecting a significant x parameter (p<0.05) was expressed as the number of significant linear regression results out of 100 random draws from each error distribution for each dataset. Power was then plotted against the range of the driving variable x. As the range of x was reduced the chance of detecting a significant regression of y on x also reduced. Power declined more quickly if the response variable had greater residual error. Note that since the maximum value of x was always 40, low values on the X axis represent situations where the driver operated with high severity everywhere.



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