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Short communication

Comment on "Indirect land use change for biofuels: Testing predictions and improving analytical methodologies" by Kim and Dale: statistical reliability and the definition of the indirect land use change (iLUC) issue

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ABSTRACT

"Indirect land use change for biofuels: Testing predictions and improving analytical methodologies" by S. Kim and B. Dale [1], presents a principal inference not supported by its results, that rests on a fundamental conceptual error, and that has no place in the current discussion of biofuels' climate effects. The paper takes correlation between two variables in a system with many interacting factors to indicate (or contraindicate) causation, and draws a completely incorrect inference from observed sample statistics and their significance levels.

The article, "Indirect land use change for biofuels: Testing predictions and improving analytical methodologies" by S. Kim and B. Dale [1], presents a principal inference not supported by its results, that rests on a fundamental conceptual error, and that has no place in the current discussion of biofuels' climate effects. As it is already receiving press attention as though its findings are considerable, it requires a detailed refutation. Briefly, the most important errors in the paper are

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- taking correlation between two variables in a system with many interacting factors to indicate (or contraindicate) causation,
- drawing a completely incorrect inference from observed sample statistics and their significance levels, and
- misunderstanding "the question" about iLUC, which is not whether biofuels production causes total deforestation to increase, but whether it causes it to increase more, or reforestation to increase less, than would happen without the biofuel program.

The authors hypothesize that if the United States biofuel production from the annual crops soybeans and corn is causing natural land plant carbon to be released by conversion to agriculture somewhere in the world, production area of those crops in the US and/or other regions will be observably correlated with US biofuel production a year previously. This conversion would be caused by the *indirect land use change* (iLUC) mechanism shown by a variety of economic models in the last few years to be an important climate effect of biofuel use [2–5]. They indeed observe this correlation in most of the regions of interest including the US, but then apply a set of four additional 'iLUC criteria' that are not logically justifiable; incorrectly apply a standard statistical test to their data; and finally misapply the concept of statistical significance to reject their hypothesis.

The authors search for regions in which US biofuel production can be shown to have caused land conversion from forest. To do this, they first implicitly substitute regional increases in corn and soybean planting for deforestation as an iLUC indicator. Then they filter the regions according to four tests applied to land use data in the 1990s compared to the 2000s, and finally calculate the correlation between percentage changes in US harvested land areas for corn and soybeans for biofuel production with the change in corn and soybean production in eighteen world regions a year later over a six-year period. In fact, what they observe is almost exactly what one would reasonably expect from a small sample for each region ($N = 6$ years) if US biofuel production, along with many other factors, does increase corn/soybean production elsewhere: mostly positive bivariate correlations with low statistical significance.

Almost none of these correlations is significant at the 0.05 level. (The authors calculate Pearson correlations, and for reasons that are not given and escape us, given that the data are all ratio-scale, Spearman rank-order correlations as well.)

From this experiment they conclude that either (i) "biofuel production in the United States up through the end of 2007 in all probability has not induced indirect land use change" or (ii) "this empirical test simply fails to detect ongoing indirect land use change from the historical data (as implicitly defined by our proxy measure, at the $p < 0.05$ or $p < 0.10$ levels"). The second is the only admissible inference from this research, and should have cautioned the authors (and reviewers) to put the project in a drawer and try something else.

For the United States itself, the authors do find correlations at the 95% confidence level, and are only able to challenge iLUC by asserting that iLUC cannot have occurred if overall arable area has shrunk, or if forest area has grown. That these criteria are entirely inappropriate can be made clear with a simple thought experiment – imagine that, in a region where afforestation projects, yield increases, and market conditions have caused an overall increase in forestry and reduction in arable area of several hectares, a biofuel producer nevertheless buys 1 ha of land and deforests it to produce corn. By the Kim and Dale criteria, this direct land use change would not exist, clearly an incorrect conclusion.

In fact, nearly all the correlation coefficients between US ethanol increases and corn/soybean land areas in places where iLUC might be expected are indeed positive. These include: the United States, Brazil, China, India, East Asia, Malaysia and Indonesia, Middle East and North Africa, Rest of East Europe and Former Soviet Union, Rest of South Asia, Russia, Other South America. For all countries except the United States, the inference of 'not induced' rests entirely on their failure to find more than a few values with 95% confidence.

Unfortunately, the results reported in their SOM raise some additional questions about the authors' statistical methodology. For example, consider the result for Brazil in SOM table A-2; the observed correlation coefficient is 0.17 and the p-value is 0.54. Their implicit hypothesis is one-tailed (negative correlation would indicate no iLUC in their scheme), but this is a two-tailed test value: a likelihood function centered at 0 cannot have more than half its mass above 0.17.

It is a fundamental error to take failure to confirm a hypothesis at a given probability level as "in all probability" confirming the alternative hypothesis. The sample statistic is the best estimator of the correlation; failure to meet a 0.05 test with a positive sample statistic doesn't mean one is 95% sure the parameter is zero or less, it means one is more than 50%, but less than 95% sure it's positive.

Corn and soybean planting in country A is affected by many things in addition to US biofuel production one year previously, including not only variations in weather and yields but also trade patterns that involve countries B and C, dietary habits, local agricultural policy, local economic conditions, and commodity prices more than one year earlier. Indeed, forest clearing and other crop-related investment decisions are typically made more than one year before new crops are planted, and must be therefore affected by longer-lagged price signals. To choose one example of the complications the authors ignore, consider the use of corn and soybean production in Southeast Asia with corn and soybean biofuel production in the US as a land use change indicator. Soybean oil is a byproduct of soybean meal production; when it is withdrawn from the food oil market for biodiesel, it does not cause planting of soybeans, much less corn. The marginal food oil in the world market is palm oil, planted mainly in Indonesia at the expense of forest, but this paper's crude indicator of iLUC misses this conversion completely.

The correlation coefficients at the heart of this paper are each calculated from six data points, so a little noise (non-US-biofuel-related effects like those listed above) can do a lot of damage to p-values. To illustrate the folly of demanding that such a short series be held to the $p < 0.05$ criterion: we constructed two identical data series comprising the integers 10 through 15, series that are in fact perfectly correlated. To the second series, we added random noise in the form of rectangularly distributed error terms lying between −5 and 5, and calculated the correlation coefficient and its p-value (one-tailed). Out of ten such repetitions, only two produced p-values below 0.05. Clearly, the odds that the crude experiment

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in the Kim and Dale paper could see the real correlation in the mechanism we constructed is only one in five, and this is in the context of perfect underlying correlation.

It is folly to try to infer cause from correlation between two variables in a world of multiple causes, or to refute it by not finding it; as Ambrose Bierce observed, one who had never seen a dog except in pursuit of a rabbit might infer the rabbit as the cause of the dog [6]. The classic classroom illustration of this principle is the inference, from a simultaneous increase in police funding and crime rates, that police cause crime. It is profoundly wrong to think that observing the hypothesized correlation at a low significance level is evidence against the hypothesis.

Over the period of this paper's concern, biofuel production in the US, yields of corn and soybeans, world population, and world per capita meat consumption (to list only a few relevant variables) were all increasing together (see, for example [7–9]. No pairwise comparison of any of them can mean anything either way. The approach attempted here must be constructed as a multivariate analysis that generates partial coefficients showing an effect independent of other variables, but with six data points this is simply not possible.

That diversion of US crops from food to biofuels caused forest conversion elsewhere in the world to be greater than it otherwise would have been, or to have occurred at all in some places where it might not have occurred, or to have delayed reforestation on unneeded cropland, or all three, is completely consistent with the results of the crude analysis reported in this paper.

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