
REJOINDER: MISREPRESENTATION AND DISTORTION IN DEFENDING FAIRTRADE

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Abstract

In my comment I showed that throughout Smith's (2009) paper, he had made errors in economic theory, logic and fact, and that had misused evidence. His reply does not address these criticisms but accuses me of misrepresenting the body of literature on agricultural economics, of distorting what he said on four points, and of being wrong in one sentence. It is shown here that this is not so: any misrepresentation and distortion is being done by Smith.

Introduction

In my comment on Smith's (2009) attack on Henderson and Sidwell I showed that he had made errors in economic theory, logic, and fact and that he had misused evidence, cherry picking from case studies to make generalizations, and these errors covered virtually everything he said. (Griffiths, 2010). In his reply, Smith (2010) does not challenge these criticisms. Instead he makes the very serious allegation that I have distorted the meaning and relevance of 'a variety of contributions'; that I have distorted what he said on four issues; and that I was wrong in one sentence. It is shown below that these allegations are incorrect, and that it is Smith who has been misrepresenting and distorting. Most of the Smith's reply consists of statements of what he believes, but since these beliefs are not supported by argument, they do not advance the discussion, and I shall not be considering them.

Distorting the Agricultural Economics Literature

Smith makes the very serious allegation that I have 'significantly distorted the meaning and relevance of a variety of contributions, not least that literature which he accuses others of failing to acknowledge,' (2010, p. 50) and that 'the above provides a distorted account of mine and other authors' contributions and continues to reflect the many inadequacies of critical discourse, even to the extent of misrepresenting the very literature Griffiths accuses others of missing.' (Smith A. , 2010, p. 50). Such a damaging

allegation should be proved to the hilt, but Smith has not attempted any justification at all.

It would be a major achievement to misrepresent the literature that I accuse others of missing, the literature of agricultural economics, which constitutes 5% to 10% of the economic literature. Smith does not provide any further discussion or evidence of this allegation. His opinion might carry some weight if it was a literature he had studied throughout a long career, but it would appear that he had no acquaintance with it until, in response to my comment, he produced the references to his reply: one journal article and three internet discussion papers.

Nor does he give any examples of my misrepresenting ‘other authors’ contributions.’

Misuse of evidence

My comment showed that Smith’s empirical evidence consists of anecdotal evidence with no statistical or other evidential value. He cherry picks parts of non-random case studies to support his argument. Smith accuses me of distortion on this. He produces a list of what he calls ‘sound bites’ from various papers and web documents of his to show that he was quite well aware that this use of data is improper. But this does not imply that I was guilty of distortion: I did not discuss what he knew or believed, still less what he had written elsewhere about what he knew or believed; I discussed how he used data in this paper, and my criticisms stand.

Smith is unrepentant about this misuse. He says, ‘With no statistically representative evidence available it is necessary to give up an empirically grounded discussion on impact, or employ the other resources that are available. It has been the latter option that is clearly being employed. .’ (2010, p. 51) I do not expect the statistically representative evidence that he specifies to be available in my lifetime. This does not mean that rigorous use of the evidence is impossible: economists and other social scientists have developed various respectable approaches which they use routinely – generalizing from case studies is not one of them.

Bias in the literature

Smith complains that my reply, ‘Twists the original statement that ‘there *might* be a bias in the current literature which favours research of more successful examples’ (Smith, 2009a, p. 34) into ‘Smith (2009, [a] p. 34) admits *that researchers choose* the more successful Fairtrade co-operatives for their case studies’. (Smith A. , 2010, p. 51) In normal academic parlance, the statement says just that. However, I accept that the statement could be read literally to mean, ‘I, Alistair Smith, have considered the

possibility that there might be a bias in the current literature which favours research on the more successful cooperatives, but so far I have not seen any evidence which would suggest that this is so', and since he is indignant, I now have to accept that this is what he meant. An odd statement in an academic paper, and a surprising one, given that the literature, and the case studies to which he refers, concentrate on the high profile, successful Fairtrade cooperatives, ignoring those that manage to sell only a small proportion of their turnover through Fairtrade and may be losing money from it, or given that it is widely believed by academics that nobody publishes negative results.

This bias is evident from Nelson and Pound (2009) who have identified what they believe to be all of one section of the literature – the impact studies carried out on Fairtrade in the last ten years. They identified 23 reports covering 33 case studies (There are 5000 suppliers) and analysed them anecdotally in much the same way as Smith. These studies are concentrated on high profile cooperatives, and appear to be studies of cooperatives which sell more than the median proportion of their output to Fairtrade, to be more successful than the average in fact. It is difficult to quantify this bias, as the studies themselves do not give the basic data one might expect, and almost none of the authors responded to a request for the standard survey report, or information on sampling, questionnaires used and data tables. There is substantial duplication, with perhaps 18% of the studies being duplicates, (Nelson and Pound are not clear just how much duplication there is, though they admit it is a problem.) This is hardly random, with 33 studies covering 5000 member cooperatives. It is compatible with the hypothesis that researchers concentrate on the most successful, high profile cooperatives, perhaps because research on cooperatives which lose money from Fairtrade membership is not publishable, a hypothesis which would explain a similar bias on the selection of case study cooperatives elsewhere. There were only two studies which were statistical sample surveys comparing Fairtrade and non-Fairtrade cooperatives. One, Arnould, Plastina and Ball (2009), had a highly biased sampling method to exclude those Fairtrade cooperatives which were not doing very well out of their membership, and this invalidates their study, as do the other errors in it (Griffiths, 2010). The other, Bacon (2005) is also cited by Smith (2010 p51) as being “‘perfectly conducted” academic and peer-reviewed analysis’. In fact, Bacon states that his selection of cooperatives for his survey was non-random (p. 503). As Bacon has not responded to repeated requests for the standard information, on sampling, questionnaires, etc., and as Arnould, Plastina and Ball (2009, p. 186) thank Christopher Bacon for providing ‘outstanding support in design and field implementation of’ their flawed survey, the presumption must be that *all*, not just half, the statistical impact studies of Fairtrade have this bias.

Statistical evaluation

Smith complains of distortion in my reply saying that it 'Notes that I [Alistair Smith] do not "present any statistical evaluation" of the suggestion that Fair Trade purchases and charitable giving should not be seen as substitutes, despite the source of these very statistics being easily accessible to those following the clear citation (to my other paper and then to an online report.' (Smith A. , 2010, p. 51)

There is nothing remotely resembling a statistical evaluation to be found on the page referenced (Smith A. , 2008, p. 18). Smith's argument, in its entirety, is the following two paragraphs:

'Despite Sidwell's claim, there is no empirical evidence that those who buy Fair Trade products make conscious or unconscious decisions to then reduce the amount of money donated to charity.

'In fact, in considering the empirical evidence on charity giving in the UK, it is discovered that there is no indication that charity giving has declined with the rise of Fair Trade. The National Council for Voluntary Organisations, which has been researching charitable giving since Fair Trade entered UK markets, shows that despite fluctuations in the patterns of giving, overall trajectories have remained constant. Again there is the argument that giving would have been higher if Fair Trade had not developed, but there is clearly no evidence to corroborate this argument. On the contrary, statistics show that donation to overseas causes tend to cluster: those who support one overseas cause are most likely to give to the others. Thus, an alternative position might be that those who have supported Fair Trade as one overseas "charity", are also likely to have given to similar causes.'

The statistical analysis he claims to have done amounts to quoting two sentences from a single web page, a briefing note or press release (National Council for Voluntary Organizations, 2008), namely 'There is little difference in the average amount given over time – at £16 per head of population in a four week period.' 'People who donate to either overseas/disaster causes or religious causes are most likely to give to the other – these two causes 'cluster' together more strongly than other causes.' These sentences stand by themselves in the note, with no further explanation, no statistics.

This failure to engage with the statistics is difficult to understand, as the National Council for Voluntary Organizations, which produced the web page to which he refers, does research and publishes it. Unlike Smith, the Council is very well aware of the limitations of its data, and the meaning of the results, and it discusses them. For example it states that 'Estimates of charitable giving can vary widely – using three different methods produces results ranging from £5.4 billion to £11.3 billion in a year.' (National Council for Voluntary Organizations, 2009) – which would seriously limit the possibility of producing any meaningful results on this question.

That is to say, Smith's statement that 'there is clearly no evidence to corroborate this argument' should be 'There exists at least one page on the World Wide Web that does not provide any evidence to corroborate this statement.'

Yes indeed, there has been serious misrepresentation and distortion, but not by me.

Evidence and substantiation

Smith complains that my reply, 'Implies that I [Alastair Smith] have questioned "whether there *is any evidence* for statements by Sidwell (2008), [and] Henderson (2008)" when the obvious reality is that I have specifically highlighted the lack of substantiation in these particular instances.' (Smith A. , 2010, p. 51).

The nice distinction between lack of evidence and lack of substantiation is never discussed in his paper, nor is it made clear why lack of substantiation is an issue when people disagree with Smith, but not when they agree, and not when he cherry picks evidence in support of his argument. Sidwell and Henderson expressed the general consensus opinion on quality in cooperatives and gave specific evidence, from Fairtrade insiders, to show that the problems applied in Fairtrade cooperatives. Should Smith want further evidence, there is no shortage in the literature on cooperatives or in the literature on Fairtrade, and it is surprising that Smith is not aware of this, as it appears to be his research subject.

Quality

Smith states that my reply, 'Claims that I [Alistair Smith] present "a concept of quality and value unique to" myself and asserts that the provided references do not support my position. While this is ultimately a matter of subjective interpretation it should be sufficiently clear that the reference to Zeithaml relates to the background theory about the socially constructed elements of physical goods (given where citations are placed); that the very premise of Mann's paper is that "Fair Trade" is about "the generation of a market where special social, relation-related product attributes play a dominant role" (2008, p. 2034); and that Golding and Peattie consider *inter alia* how, "The primary goals of FT reverse the traditional notion of an ethical product . . . in which ethical dimensions are an augmentation, and by implication are seen as "added", to the product . . ." (2005, p. 157). (Smith A. , 2010, p. 51)

He goes on to claim, 'Griffiths implicitly accepts my supposed 'unique' view of quality. . . .' (Smith A. , 2010, p. 51). I do not. My considered judgement that his view was unique was not arrived at lightly; it was based on my reading of 1190 books and papers on quality. The relevance of the papers on quality that he cites is not 'a matter of subjective interpretation': if a theoretical model is being used to explain Fairtrade,

that model must have assumptions that approximate closely to the relevant aspects of Fairtrade – sprinkling a model with words like ‘Fair Trade’, ‘social, relation-related’, ‘ethical product’ is no substitute. For an alternative approach, see Daviron and Ponte (2005), who have read the literature on quality, who are deeply immersed in the literature on and practicalities of agricultural marketing, who have studied the coffee market, and who write in clear, readable, logical English.

On Coffee

Smith also states that I was wrong in one sentence. In his original paper (2009, p. 32) he dismissed the economist’s overproduction argument on the grounds that he knows of no research that supports this proposition. I pointed out that the evidence is in the agricultural marketing literature (and Fairtrade is agricultural marketing): the close parallel of the collapse of the world market when Vietnam paid its farmers more than the world price is part of any study of the coffee market written over the past 20 years. (Griffiths, 2010, p. 46). International Coffee Organization figures show that Vietnam increased its coffee production a hundredfold in the eighteen years 1980/81 to 1996/7, with trees planted at the high, pre-liberalization prices, and that the rate of increase dropped sharply, with production increasing 2.5 times over the next twelve years, as prices moved towards world prices.

Smith refers to only four publications in the agricultural marketing literature, one journal article which makes no reference to coffee or Fairtrade, two internet discussion papers and a workshop paper. He also refers to a book by a sociologist and a paper in an environmental science journal. In his response makes a series of false statements, citing these in support.

‘While Vietnam was already increasing its output, production increased significantly in 1996–97 in response to (1) a generalised and significant increase in world prices after the Brazilian coffee frost in 1994 (Eakina *et al.* 2009, p. 399)’ (Smith A. , 2010, p. 52). That is to say he claims that Vietnamese farmers observed an increase in prices, took the decision to plant, planted at the appropriate time (a six week window of opportunity), and harvested a significantly higher crop, all in two and a half years. It is normally taken that coffee trees have their first real crop after four years and do not reach full production until year six.

Smith claims, ‘While Vietnam was already increasing its output, production increased significantly in 1996–97 in response to. . . a structural shift by the limited range of coffee processors to the type of coffee produced by Vietnam (Lewin *et al.* 2004, p. 6)’ (Smith A. , 2010, p. 52). Again, the trees had been planted before this happened: Vietnam sold largely to the Soviet Union and Warsaw Pact countries and was outside the ICA before liberalization.

Smith says too, 'While Vietnam was already increasing its output, production increased significantly in 1996–97 in response to . . . massive funding given by the World Bank and Asian Development Bank for the expansion of export agriculture (Jaffee, 2007, p. 44).' (Smith A. , 2010, p. 52). In fact, the US veto on multinational loans to Vietnam meant that Vietnam was excluded from World Bank and ADB lending until 1993 (ADB, 2010) (US Department of State, 2010). The World Bank only began lending again in the rural sector in Vietnam after 1996. (World Bank, 2010). Any new price incentives would not have affected the crop size until the next century.

Smith appears to argue that my statement about high prices leading to increased production is incorrect because some of the Bank money may have been used not for higher government prices but for preferential credit and a range of input subsidies and that parallel markets had an impact. I quote his exact words as they are far from clear and I do not want to be accused of distortion: 'Although the Vietnamese government capitalised on this opportunity using funding to *provide preferential credit and a range of input subsidies* (Nguyen and Grote, 2004; [Dominic] Smith, 2003), this case study is not accurately represented in the statement "Vietnam paid its farmers more than the world price" as it ignores the contextual peculiarities of the situation – including the hypothesis that the parallel market liberalisation intensified incentives structures.' (Smith A. , 2010, p. 52). In fact preferential credit, input studies and parallel market prices are *always* taken to be part of the price package by farmers, traders and economists. They are *always* integrated in price policy analysis.¹

It is perhaps not necessary to state that Smith misrepresents what these researchers say. Eakin, Winkels and Sendzimir (2009) [all of whose names Smith misspells], Jaffe (2007), Lewin, B., D. Giovannucci and P. Varangis, and Dominic Smith (2003) do not make the statements that Smith claims they do, or anything remotely like them. Nguyen and Gross (Nguyen, 2004) and Dominic Smith (2003) make it clear that they are perfectly well aware of the complexity of agricultural price policy and the need to integrate all price incentives in the analysis. I see no reason to believe that any of the authors cited shared Smith's belief in an instant increase in crop size: several discuss the delay between price signal and increased crop. Che, Kompas and Vousden (2001) present an assumption-laden mathematical model of the market for rice, a short-season food crop; they make no mention at all of coffee, a cash crop, a tree crop with a twenty year life and a different price regime. This paper has no more relevance to Vietnamese coffee than to Scottish strawberries, so it is misrepresentation to cite it.

Smith (2010, p. 52) accuses me of not accurately representing a case study though it is not clear whether he is referring to Nguyen and Grote, (2004), Dominic Smith, (2003) or Che, Kompas and Vousden (2001). I went further than that: I made no reference at all to any of them, as I had not heard of them.

Methodology

Underlying what Smith says is a serious misunderstanding of scientific method. There is a large literature on agricultural economics, so there are certainly papers that disagree with my conclusions, or anybody else's. It is easy to cherry pick these to produce a dozen citations that challenge my conclusions, or anybody else's, and easier still to cite papers that disagree on a small point. For this reason counting citations has no place in academic or applied economics. Our method is to improve our models by constantly challenging the realism of our assumptions, the logic of our economic analysis, the meaning and validity of our facts, the coherence of the facts and theory throughout the model, the consistency of the model with the facts (including the inconvenient ones) and, in principle, the predictive accuracy of the model. Citing papers that do not do this is considered improper.

In view of my comprehensive criticisms of everything in his paper, it is surprising that Smith should say, 'The methodology employed in both of my papers is specifically endorsed by Griffiths. . .' (2010, p. 51). It is not. The methodology used by me and my colleagues around the world is based on hard theory, hard fact and rigour: Smith's methodology is about as far from this as is possible.

Envoi

This is not an academic game: not the published research on Fairtrade, nor the criticisms of it, nor Smith's attack on the critics, nor my response to him. Bad research can kill. If money that the public believes is going to help the poor in the Third World instead ends up as increased profit for British firms, people die. If the money is used for low impact aid, rather than high impact, people die. A lot of people.

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<http://web.archive.org/web/20080509110840/http://www.ncvo-vol.org.uk/research/index.asp?id=7662>

¹ When I was working on coffee in Vietnam in 1988, the price incentive was achieved by using an exchange rate of 6000 dong = \$1 for coffee produced by farmers who had not borrowed money, and 4000 dong = \$1 who had borrowed money, compared with the normal rate of 250 dong = \$1 used for exports of other products such as timber. This, combined with obsessive secrecy, made the calculation of the actual subsidy very difficult indeed.