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## **Toxic famine research and how it suppresses its critics**

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# TOXIC FAMINE RESEARCH

## And how it suppresses its critics

**Peter Bowbrick<sup>1</sup>**

### ABSTRACT

Bad economic theory can cause famines or stop governments from taking appropriate action to prevent famines. This can kill millions.

Amartya Sen's theory of the cause of the Bengal Famine, which is the inspiration for his 'entitlement approach', has been refuted again and again, in different ways, by economists of different theoretical persuasions and by statisticians expert in this area. Sen has been shown to systematically misrepresent the evidence, to make repeated, elementary, theoretical mistakes, and to use and misuse 'meaningless' statistics. No attempt has been made by anyone to challenge these refutations: they are incontrovertible. Sen has not retracted his theory, or any of it, which implies fraud. Yet Sen's work is widely believed and used in famine situations. His 'entitlement approach', based largely on his theory of the Bengal famine, is the basis of a research programme.

This paper examines how the research programme suppressed the criticisms, ignoring the normal requirements of academic and professional research and integrity. It also produced new falsehoods.

Q11, Q13, Q18, O12, N5, H56, H84

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# TOXIC FAMINE RESEARCH

## And how it suppresses its critics<sup>2</sup>

### INTRODUCTION

**‘But what a weak barrier is truth when it stands in the way of an hypothesis.’** (Mary Wollstoncroft, 1792, p. 10)

Applying bad economics to food crises or to famines can kill millions. This paper examines how a widely quoted famine research programme has been manipulated to suppress criticisms and propagate bad economics, ignoring the standard approaches of scientific method, economic method and integrity, as used by professional and academics.

Researchers have repeatedly shown that Amartya Sen’s theory of the cause of the Bengal famine of 1943 is based on false evidence, false statistics and wrong or non-existent theory, that, in fact it is refuted on many grounds – and a refutation is not just a routine difference of opinion between academics: it is on another plane altogether. Any one of these refutations is fatal to the research programme as a whole, discrediting it. Neither Sen nor anyone else has attempted to contest these refutations. None of his work has been retracted. Sen and his followers continue to quote the refuted theory decades later. There is a flourishing research programme on ‘Sen and famine’ which suppresses the refutations. It may be argued that researchers who choose to ignore unchallenged refutations are acting fraudulently. This paper shows some ways in which a large number of well-meaning people who are passionate about tackling famine are manipulated into adhering to a research programme is fraudulent.

The academic analysis of famine has become so value-loaded, and so political, that I start by setting out my values and beliefs and the economic tradition I work in. I have spent most of my life working in 38 countries, trying to make the system work

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<sup>2</sup> I would like to thank Mark Tauger and Tim Dyson for their helpful comments. I should also like to thank Denis Segal for his help over the years, providing me with the fruits of years of research in the British Library, finding key documents that were unknown to academia, and his vivid descriptions of delivering food to the starving when working with the Gurkhas in 1943.

better, to benefit producers and consumers – and in many of these countries, both producers and consumers are close to starvation even in normal times. I am constantly aware that if I do bad work, it is likely to cause destitution, hunger and death. And I am squeamish.

I work in agricultural economics, which is the oldest economic tradition, and which has concentrated on hunger and famine from the beginning. It covers all stages of the food market from production, including the sociology of production, to consumption, including the sociology of consumption, and it includes subsistence production and consumption. It has produced a vast amount of theory, based on experience, to tackle real world problems. Even as late as the 1980s or 1990s, one tenth of the world's economists were agricultural economists, and the first professors of marketing, in Britain and Europe at least, were agricultural economists. Agricultural economics treats famine as a normal market under extreme conditions: markets still operate, but more people struggle to buy food, and more people die because they cannot buy enough food. The label 'famine' is irrelevant to analysis: a situation where people starve may be labelled 'a famine', 'a food crisis', or 'food riots' by the press, by governments or by international organizations: often it is ignored or concealed. I am one of the very few economists who combine practical experience with a record of publishing in academic journals – for some years I was a top academic publisher in my field. As a professional economist working on agricultural policy and marketing in poor countries, I have expected that my reports would be reviewed by perhaps a dozen economists, including local economists and economists from the international agencies and bilaterals, as well as by local agriculturists and administrators and politicians, any of whom may have information or analysis that refutes the report and leads to its rejection, while academics only have to get their work past the referees of one journal.

I have worked at all levels from national food and agricultural policy to small local markets and firms, I have worked in India, Pakistan, Sri Lanka and Nepal, in the sub-continent, but also in countries in Africa with similar climates and crops, and a similar level of development to 1943 Bengal. My book, *The Economist's Tale* (which was written under the nom de guerre Peter Griffiths, as I was whistleblowing against the World Bank) gives the only insider's report of an economist tackling a famine situation, and the international, national and institutional politics involved.

## SEN'S THEORY

The orthodox view on famines up to Sen was that famines can be caused by sudden falls in supply, increases in demand or other changes in demand. Supply may fall

because of crop failures, due to drought, flood, locusts, disease etc. Wartime blockades and transport failures have a similar effect. Exporting, perhaps to get a higher price, perhaps as an act of war (e.g. Greece in 1942 or Vietnam in 1944) can reduce the supply. Governments, speculators, or hoarders could remove food from the market, reducing supply. Demand changes could be as devastating: occupation by an army, friend or foe, was one of the four horsemen of the apocalypse. A sudden influx of refugees could strain existing supplies. A boom industry might have a similar effect. Countries which had relied on imports sometimes find that they no longer have the money to do so. The collapse of an industry may mean that its employees can no longer buy food. Macroeconomic policies often mean that some groups of people or the country as a whole cannot buy food. A wave of famines and food crises hit Africa as a result of IMF/World Bank structural adjustment programmes, which had effects on supply and demand. It was recognized that the shock of a crop failure, for instance, had knock-on effects on supply and demand throughout the sector – economists always analyse supply and demand at the same time. The accepted ways of dealing with a famine, of either sort, included that of the Indian Famine Code from the 1870s, which was, first, for the government to ensure that there was enough food available in the affected area, and then to make food available to people who could not buy it, giving it as relief or supplying it through food-for-work programmes. Rationing was also effective. All these might include government controlling stocks.

It was expected that, regardless of the facts, much of the populace would not interpret high prices as being the result of a shortage, but as evidence that greedy grain traders had chosen to put up the prices when there was plenty of food available. Again, some would believe that the only reason that they could not get food was because a group of people – perhaps the rich, perhaps people of another religion or caste – were eating far more than usual. It was understood that governments sometimes acted on such beliefs, with disastrous effects. Accordingly, the consensus was that, in practice, a famine was more likely to be triggered by a reduction in supply than by a reallocation of demand, or, at least, that it was wise to start with this premise.

The post-famine consensus was that the Bengal Famine of 1943 was triggered by the failure of the December 1942 rice crop, which was some 40% less than normal because of drought, then a cyclone, then a major fungus outbreak. Burma, which had supplied most of the rice for Bengal's urban population, was captured by the Japanese in early 1942. Large rice exports in 1942 were significant. Demand factors included population increases, army requirements, and the influx of perhaps 300,000 refugees from Burma. The crisis was handled very badly by the governments, notably the Government of India, the Government of Bengal, the governments of surplus provinces and the Government of the United Kingdom. Corruption was a problem. The Report of the Famine Inquiry Commission (1945) gives details.

Sen's theory of the cause of the 1943 Bengal famine is very different. He believes that there was no major fall in supply, that there was *at least* 11 % more food available in Bengal in 1943 than there had been in 1941 when there was no famine, so the famine could not have been caused by a decline in food availability (Sen A. , 1977, pp. 42, 53; Sen A. , 1980b, p. 80). Instead, it occurred because of a change in the distribution of existing food supplies arising from wartime conditions, particularly inflation and a wartime boom. This meant 'an uneven expansion of income and purchasing power' – that some groups of the population received higher incomes and ate more, leaving little for the rest of the population. At the same time, others had insufficient money to buy food, so they starved. He has been particularly scathing about those who consider a decline in food availability to have caused the Bengal famine of 1943. (Sen A. , 1976; Sen A. , 1977; Sen A. , 1980; Sen A. , 1980b; Sen A. , 1981; Sen A. , 1981b) (Sen A. , 1984).

There are millions of microeconomists around the world whose daily work is studying unique firms and markets and making recommendations on the allocation of scarce resources to meet objectives. They construct economic models by applying their economic logic (theory) to some of the facts of a situation, to produce models which they use to make decisions. These models are 'scientific', in Popper's (1974; 1975) sense, in that they may be tested, and may potentially be refuted. Each time the model is tested but not refuted, our confidence in it rises, but it can never be considered 'proved'. Sen's model, his theory of the cause of the Bengal famine of 1943, is such a model: it is testable, and is refuted if its facts are wrong, if its theory is wrong or if its predictions are wrong.

From Sen's theory of the cause of the 1943 famine, he derives his 'entitlement approach' belief system (which is not the subject of this paper). Among other things it claims that the cause of most, or at least many, famines is a redistribution of supplies resulting from a shift in purchasing power. He has been scathing about those holding the majority view that, in practice, far more famines are caused by a sudden food availability decline, and about those who consider an examination of aggregate food supply before and during the famine – when supply and demand are constantly changing - are of primary importance in the analysis of famine.

Sen's 'entitlement approach' cannot be tested, refuted or proved and is, therefore, 'unscientific'. A scientific theory forbids certain outcomes. A theory which is not disproved whatever the outcome is trivial. The theory that 'a famine occurs when the Famine Fairy waves her wand, but not otherwise' explains all famines, and there is no possible way of proving it wrong, so it is trivial. A theory that says 'x will not happen if a, b and c are the case, *except sometimes*' is trivial. The 'entitlement approach' falls into this category, unscientific, because there is no empirical test possible. Sen's supporters state clearly that they will continue to believe in this belief system

regardless of how many empirical tests refute his *models* (I discuss this with examples below). It is a belief system.

There are other types of economic publications which cannot be tested or refuted and so are 'unscientific'. These may be useful, useless or harmful. Some examples are writings based on social, political or religious belief, such as the Washington Consensus, neo-liberalism, etc. Much of the 'unscientific' literature is musing, pontificating or trying to produce broad generalizations from selected specific examples. Some economic theorists would class such belief systems as 'religious', with discussions on them being 'theology'. Sen's beliefs on 'entitlement' would appear to fall into this group.

There are other theories based on arbitrary assumptions, which might be called 'pure theory'. These cannot be tested by their predictions or by the reality of their assumptions. They are refuted if their logic is wrong. If their assumptions contradict each other, either directly or in their implications, they are illogical and must be rejected. Each explicit assumption has implicit assumptions – the explicit assumption of 'perfect information', for example, has a lot of implicit assumptions about the product, the market, information systems etc. The more realistic the assumptions, the closer to a 'scientific' model. The good 'pure theory' models, which were often developed from scientific models of real-life firms or markets, by making the assumptions less situation-specific and more generally usable, are enormously helpful – where would we be without monopolistic competition? The conclusions are not meant to apply everywhere, but strings of logic from this can be applied to the facts when we are building a testable model for a real situation: if nothing else, they are useful in identifying the factors to be incorporated when we build a model of a real market. The bad 'pure theory' is useless. I believe that it is not possible to produce good scientific models of firms and markets without a large amount of 'pure theory' in one's toolbox.

## REFUTATIONS OF SEN

It is difficult to imagine that a top professional in agricultural and food economics using standard economic method would take as long as twenty minutes to be deeply suspicious of Sen's 'Poverty and Famines' (1981), or as long as an hour to see that Sen's theory of the cause of the 1943 Bengal Famine is logically impossible. Indeed, I have seen two colleagues reaching this conclusion in considerably less time. I read his book when millions were starving around the world, and felt it my duty to write a refutation of his theory of the 1943 Bengal famine (Bowbrick P. , 1986; 1987; 1988, 2008). I discuss the response of other critics of his theory below.

Refutation is considered, epistemologically and methodologically, to be



fundamental to the claim that economics is a science. One refutation can invalidate a thousand papers. Without refutations we would be drowned in rubbish: anything goes. Writing a full refutation for publication takes serious research, though. Accuracy is all important: for example, I checked and rechecked all my statements and all my references against Sen's sources, and I got friends and colleagues, and then referees, to recheck. The analysis required building models of an enormous and complex food sector, checking for consistency and accuracy – something very few people indeed have the time, the skills or the experience to do.

Published refutations must be taken very seriously indeed: they are not the normal sort of research paper churned out as a 'publish or perish' requirement; they are not the normal academic paper drawing different conclusions from the same data, using different theory or statistical analysis, or producing new evidence.

People are often reluctant to accept refutations. While philosophers may talk of a 'crucial experiment' causing a scientific revolution, this does not often happen even in the hard sciences. The sociology of science suggests that both teachers and researchers may refuse to accept the refutation, to the extent that the old belief clings on until the older generation retires or dies off.<sup>3</sup> Where there is a sudden switch in beliefs within the profession as a whole, this may be because the old one goes out of fashion, rather than because it is refuted (Kuhn, 1962), and, of course, this is particularly important with 'unscientific' economics, which cannot be 'proved' or 'disproved, but may become unfashionable.

Some people take a certain delight in believing ideas which are unfashionable and improbable and, important for Sen's work, ideas which are quirky, not immediately obvious. Homeopathy, astrology, anti-vaccination beliefs, holocaust denial and climate-change denial flourish. There are about 3 million people in the US who believe that the world is flat, many of them far more knowledgeable about science than the average person (Westmoreland & McCormick, 2020).

## The refutations

I start by summarizing the errors I found in Sen's work (1986). Nobody has attempted a formal challenge to any of these refutations, not Sen, not anybody else. It is convenient to divide the errors exposed into those that refute the whole of Sen's

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<sup>3</sup> For example, the academic profession immediately accepted the economic criticisms of compulsory grading, but no action was taken until a new generation of civil servants rose to the top ranks after 34 years. Part of the problem was that many civil servants stood to lose their jobs if the economics were accepted. The cost of their refusal to act may be measured in hundreds of billions of pounds. Bowbrick (2012)

thesis and those that refute only part of it. Each of the following errors is, by itself, fatal to Sen's whole thesis. If any one of them is accepted, then his whole thesis must be rejected. It is not, of course, possible to argue in economics that his theory may be right, but his facts are wrong.

- *'Production statistics are not accurate to within  $\pm 50\%$ , and the difference between them (which Sen relies on) is only accurate to  $\pm 3000\%$ .*
- *Sen's assumption of zero carry over conflicts with all available evidence.*
- *Sen's production and import figures do not prove his point that there was food available in 1943, but exactly the opposite.*
- *Apart from the unreliable production statistics, all evidence (including that on speculation and inflation) points to the fact that a) there was a short crop and b) there was a shortage.*
- *If changed distribution caused the famine, some groups of the population ate between two and six times as much as usual and paid between four and 20 times as much as usual to do so. It can be proved both logically and statistically that they did not.*
- *The actions of the government of Bengal were those Sen would recommend. Their failure to have any effect proves the misdiagnosis, and my prediction of the effect of the misdiagnosis.*
- *There are repeated misstatements and misquotations from his sources - on the 'indifferent crop', on his conservative figures, on the number of people covered by relief schemes, on the actions of the government of Bengal, on the Famine Commission's support for his statements about speculation and hoarding, on the rice denial policy, on the size of other famines and on Mahalanobis, Mukkerjee and Ghosh's statistics for instance. In addition, the evidence presented is selective. Taken together they cast the gravest doubt on his rigour and reliability.'*

(Bowbrick, 1986 pp122-3)

The following points taken individually do not disprove the theory as a whole, but only individual hypotheses:

- *On inflation: Sen presents no theoretical explanation of an improbable hypothesis; the evidence he does present is incorrect; and other evidence indicates a shortage. Government procurement plus a shortage is the best*

*explanation of the enormous price rises.<sup>4</sup>*

- *On speculation: Sen presents no theoretical explanation; his thesis conflicts with accepted theory; and his thesis conflicts with the evidence. The evidence plus accepted theory suggests a shortage.*
- *On hoarding: Sen presents no theoretical model and the facts are against him.*
- *Changes in purchasing power are more likely to be the effect rather than the cause of the famine.*
- *Imports were unnecessary under Sen's thesis.*
- *The boat denial policy did not reduce aggregate supply of rice. Failures in regional distribution are irrelevant to Sen's thesis.*
- *Sen's hypothesis and facts on the rice denial policy are contradicted by his sources. If anything, the policy increased total supply.*

(Bowbrick, 1986 p123)

I showed that, in more than thirty instances, Sen had grossly misrepresented the facts in his sources. I showed repeated fabrication by misuse of statistics and by manipulation of statistics. (Bowbrick P. , 1986, p. 117; Bowbrick P. , 1988, 2008).

#### Other critics

A model of an impossible situation is necessarily illogical. It is possible to refute an illogical model in many different ways because there are contradictions everywhere you look. It is not surprising, therefore, that people with different professional backgrounds to mine, different sets of theoretical and analytical skills, and different sources of information and raw data, have refuted Sen's theory on grounds which I could never have identified myself (e.g. (Tauger M. , 2003; Tauger M. , 2006; Tauger M. , 2009), (Goswami, 1990), (Dyson & Maharatna, 1991; Dyson, 1991; Dyson, 1996), (Basu, 1986), Islam (2007)). In fact, since the theory was a rehash of ideas that had been current during the 1943 famine, it was possible to disprove the theory even before Sen published it (notably by the Famine Inquiry Commission (1945)). The statistics that are fundamental to the theory had been discredited by Indian statisticians and administrators well before the famine (See in particular Das (1949), Dewey (1978) and

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<sup>4</sup> I would now take a much stronger line on this, having worked in countries with not just inflation, but hyperinflation far greater than the modest wartime inflation in India, but which did not have famines. Indeed, while it may be argued that famine has triggered a few hyperinflations, it is not clear that the opposite has happened. It should be noted that there are different concepts of inflation, and that often the causes of inflation and its effects on different segments of the population bear little relation to those described in textbooks. Again, rigour requires that we build our models on the reality of the situation being examined, and certainly not on the experience of inflation in Britain.

Mahalanobis (1944) for reviews of the literature. The post-famine consensus examined and discarded Sen's explanations. Again, these criticisms of the theory have not been challenged using the normal criteria of economic method, or of academic refutations and counter-refutations: **they stand unchallenged**. The full power of these refutations is that they are not from one research programme, one theoretical approach, from one country, from one belief system, and that they do not all pick on the same errors. It is not a concerted action by a clique. To the best of my knowledge none of us had met Sen before we wrote. Nor had we met each other. I do not suggest that this list of Sen's critics is complete – on the contrary, I show below that the 'Sen famine research programme' suppresses criticisms both by not citing them and by misrepresenting them, so it is very hard to identify them. I do not attempt to cover similar criticisms of Sen's work on other famines or other subjects: life is too short. Again, the refutations are *independent* – even if one or two refutations were disproved, the rest would remain unchallenged. I cannot discuss all these refutations here, and I would be afraid of misrepresenting complex arguments outside my area of expertise, so I concentrate on what I know.

One cannot, of course, expect most academics to have the necessary skills, theoretical toolbox or practical experience to identify flaws in the same way as the people publishing refutations, but it is reasonable to expect them to understand the factual errors, misrepresentation of sources, wrong economics and bad statistics, when they are pointed out to them. And academic ethics requires that, once they know that very serious criticisms have been made, they should take the trouble understand the criticisms before citing Sen or using his work, and that they should not suppress the criticisms.

### Obvious weaknesses

#### *Enormous consumption by the very poor*

Some of the most glaring weaknesses, which an experienced professional might well pick up in a few minutes' reading, come from Sen's nearest approach to an economic model:

*'In a poor community take the poorest section, say, the bottom 20% of the population and double the income of half that group, keeping the money income of the rest unchanged. In the short run prices of food will now rise sharply, since the lucky half of the poorest group will now fill their part-filled bellies. While this might affect the food consumption of other groups as well, the group that will be pushed towards starvation will be the remaining half of the poorest community which*

*will face higher prices with unchanged money income. Something of this nature happened in the economy of Bengal in 1943.’ (Sen 1980 p 618)*

I have discussed this ‘causal hypothesis’ elsewhere (Bowbrick P. , 1986, p. 117; Bowbrick P. , 1988, 2008) showing that none of the ‘facts’ stated by Sen were remotely similar to what happened in Bengal. Such an enormous, instant, economic and social revolution has never happened anywhere, and contemporary reports give no hint of it happening in Bengal. What needed to be explained is why there was a widespread famine in 1943, but not in 1941, 1942 or 1944, so **only year-to-year changes are relevant**. Sen was claiming that 10% of the population, some six million people, all in the poorest 20% of the population, doubled their income at the beginning of 1943, then halved it again at the beginning of 1944. Since policy makers estimated the population of Calcutta to be four million at the time, and that of Greater Calcutta to be six million<sup>1</sup>, nearly all of those affected would have been rural, but I have found no mention of any such rural boom, rather the opposite. Similarly, there is no suggestion anywhere in the contemporary literature that any consumers, let alone six million people, halved their income at the beginning of 1944: the wartime boom continued into 1945. This means that instead of six million people having their income going up and down like this, *none* did. It will be noted that, even though the wartime boom continued until 1945, Bengal did not have a famine in 1944 or 1945. And the war affected all India, not just Bengal. That is to say, Sen’s basic claim is fiction.

Economists normally cross-check the implications of a model, asking not only if it is internally consistent, but asking whether it is consistent with those facts that were not included in the model. My check showed that if this instant economic revolution had happened, it would have increased aggregate demand by only 1.8% - and this would certainly not have triggered a famine (1986). Again, Sen’s model requires that, in 1943, but not 1941, 1942 or 1944, the people whose income doubled ate enough extra to cause three million deaths, plus serious hunger for 40 million people. This required that the lucky six million people whose income doubled ate three times the normal amount of food per day, a consumption level which no population has achieved. And, of course, if one assumes that only one million people, rather than six million, doubled their income, the amount of food they would have had to eat would have been thirteen days’ normal consumption each day, which would suggest that overeating was the main cause of death, which, again, conflicts with all evidence. Readers may check the credibility of Sen’s theory of the Bengal famine by eating three times their normal consumption for a week, and then eating thirteen times as much for the next week. These calculations are based the scenario most favourable to Sen’s theory, that six million people doubled their

income: if only 100,000 people or nobody did, as appears to have been the case, the amount that they would have to eat approaches infinity.

Any analysis of Sen's model from any new angle produces another impossibility, another refutation. For instance, it is stated that if poor people, who might perhaps spend 50% of their income on food normally, get twice their normal income, they will eat so much more that there will not be enough for everybody else and there will be a famine. Doubling their income means that they would have perhaps three times the money available to buy food. If prices remained the same, they could eat three times as much. However, when the price rose to, say, four times the previous price during the famine, they would have to drop consumption to 75% of the previous level, which was barely enough for survival.<sup>5</sup> When the price rose to the peak level recorded, they would not be able to buy enough for survival. They would be eating less than they need to survive at a time when Sen's model requires that they eat far, far more. Impossibility piles upon impossibility.

#### *Enormous consumption by factory workers*

Similarly, I showed the absurdity of Sen's claim that factory workers ate so much more than usual that very little was left for the rest of the population, that forty million people went hungry and three million people died: it would require that they ate an unbelievable amount of food. It is absurd to suggest that large parts of the population would be able to eat twice as much as normal under these circumstances. It would be absurd to suggest that they would be willing or able to pay four times the normal price and also to buy twice as much rice. It would be absurd to suggest that they had doubled their food consumption in January 1943 then halved it in January 1944.

Some employers paid employees a 'dearness allowance' to enable them to buy food at current rates, others worked in groups of employers who bought rice in bulk at wholesale prices, then distributed rations to employees, which cut costs. It is beyond belief that the employers would have paid enough, in dearness allowances, or for rations, for employees to increase their consumption above the normal pre-famine or post famine consumption. And I presented evidence (Bowbrick P. , 1986, p. 117) from Sen's main source, the Famine Inquiry Commission, that food supplies in Calcutta were very tight indeed, and were often cut because of shortage of food (e.g. pp 31, 32, 63), and that the consumption of

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<sup>5</sup> I assume that prices only rose by a factor of four, so that I cannot be accused of exaggerating. The contemporary literature is filled with claims of much higher price rises, e.g. *'This population structure was based on rice costing from Rs. 2.8 to Rs. a maund, a labourer's wage being four to five annas a day. Contrast these conditions with the official price (during late 1943) of Rs. 22 a maund and open market rates varying from Rs. 35 to Rs. 135.'* (Brown, 1944). I discuss below some of the problems with price statistics and why we are extremely cautious when dealing with them.

Calcutta actually fell by 12% to 15% during the famine.<sup>6</sup>

Clearly, employers who gave dearness allowances or rations found the cost of employing labour shot up during the famine, often doubling or quadrupling. Some went bankrupt, others moved to other provinces where food prices were normal. The bombing of Calcutta in December 1942 also caused a lot of people to leave. This would help explain the contraction of Calcutta's population by 15% during the famine. The high rice prices in famine areas in 1943 must be clearly distinguished from the inflation in India as a whole, with the working class cost of living index rising at about 18% per year throughout the war.

It is basic economics that rational people do not spend all of an increase in income on rice. Nor do they eat all the rice given to them when they are given more than they need. When I was working in Sierra Leone, a very similar food crisis occurred. The government always ensured that the police and army, at least, always at least got their ration, and got an increasingly higher ration than they were entitled to, after some failed coups (Griffiths, 2003). I was informed by the rice trade and people using the retail markets that the spouses of soldiers and police officers sold much of this rice in small quantities on the retail markets, using some of the money to buy other food, like meat, fruit, vegetables and palm oil, and some to buy other necessities which were prohibitively expensive because of the collapse of the currency. That is to say, the army and police families did not change their calorie consumption when they got a higher ration – the demand for food is famously inelastic. This response was economically rational. As the immediate problem for the country was calorie malnutrition, this bribe to the army and police had no discernible effect on the calories available to the rest of the population. The situation in Bengal may have been the same.

While some workers moved from industry or agriculture to work on public projects such as airfield construction in 1942 and 1943, there is no evidence that they ate more during the famine than they had in the years before or after the famine. I have not found any suggestion in contemporary literature that they ate three to twenty times as much per day as in normal years, as would be necessary if this were to be a cause of famine.

The position was that there was not enough food to go round. In such circumstances we can expect that the rich and powerful will be adequately fed, and others will go hungry.

**Sen does not challenge my criticisms:** they are unanswerable. He produces no facts, no theory. What he does instead is to create a diversion, saying that I had

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<sup>6</sup> There was not enough food available for key workers. Coal miners, fundamental to the whole economy and the war effort, were given only 3000 calories a day, far too little to do a day's hard manual work. By contrast coal miners in Britain and Germany got 5000 calories.

made three factual errors (Sen 1986 p31). He is wrong, of course, but even if he had been correct about these, my argument would stand, unaltered. I discuss these [below](#) in the section on his use of lies as a diversion tactic.

In my writings on Sen I concentrated on showing that his research was fraudulent. I was careful not to suggest that he was just a sloppy researcher who did not do any serious reading round his subject, as this would give him an escape. Accordingly, I was careful to confine myself to the sources that Sen used and how he used them. I do not know how many other sources he consulted but refused to cite as they did not support his theory. This means, of course, that I do not present my own diagnosis of the famine, which, as I said, would not overlap with Sen's. It would, for example, have infinitely more complex economic analysis than anything in Sen's writings. It would draw on the wide range of contemporary sources, far too many to mention here<sup>7</sup>, including the post-famine consensus. My market models would make use of the range of evidence on sociology, making them complex and realistic. I would, wish to discuss the Imperial War Cabinet's delay in providing shipping for food supplies to India, which the Famine Inquiry Commission could not get information on because the evidence was in Britain, classified 'Most Secret' until 1970.

### Unacceptable Research Practices

I do not accept that it is legitimate to adopt Sen's practice of routinely accusing people of fraud when they produce facts that do not agree with his claims or when they challenge his economic analysis. Disagreement and argument are fundamental to our discipline. However, it is generally accepted in academia, as in the wider world, that the following actions all constitute fraud:

1. Fabricating evidence or presenting false information as the truth, *suggestio falsi*.
2. Selectively suppressing inconvenient facts, *suppressio veri*.
3. Selectively citing research and evidence that supports you.
4. Selectively suppressing research that disagrees with you.
5. Fabricating the theory to support one particular conclusion.
6. Failing to state the degree of unreliability in your evidence and statistics.

Academic fraud is a major problem today. For example, COPE, the Committee of Publication Ethics, have estimated that one in ten medical papers submitted are fraudulent, though this fraud is likely to kill or harm patients, and the benefit to the fraudster may be minor<sup>8</sup>. Subsequent scandals, some of which are described in the

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<sup>7</sup> I should be pleased to give any researcher a copy of my extensive library on the subject.

<sup>8</sup> We know more about medical fraud than fraud in other disciplines because much of the work is



reports of the Office of Research Integrity e.g. (Research Triangle Institute, 1995) confirm that this is highly likely.<sup>9</sup> There are frequent scandals about this.<sup>10</sup> When research on famine is fraudulent, the consequences are far more serious than with most medical fraud. It is not surprising that some researchers are willing to kill for a minor benefit to themselves. When dealing with famine, we are constantly aware that many people are willing to steal food aid or money, depriving the starving of food, and some people will even create a famine in the certain knowledge that many people will die as a result.

It must be taken therefore that, whatever Sen's motives were for presenting this misinformation, the fact that his publications remain in print after they have been refuted is deliberate fraud, *suggestio falsi* and *suppressio veri*. The inescapable conclusion is that it would be irrational and immoral to place any credence on anything written by Sen on any subject.

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done in teams, and nurses and laboratory staff who do not share the experimenter's ethics or financial incentives blow the whistle.

<sup>9</sup> Most of the evidence on academic fraud is to be found on the internet, mainly press stories published when the fraud was exposed: journals seldom discuss it. Ben Goldman's work on medical statistics and fraud is worth reading.

<sup>10</sup> Interesting web sites include [retractionwatch.com](http://retractionwatch.com) and [pubpeer.com](http://pubpeer.com).

## PART 2. SEN'S RESPONSE TO THE REFUTATIONS

**'It is easy to wake someone who is sleeping: it is hard to wake someone who is pretending to sleep.'** (Navajo proverb)

### CONCEALING THE REFUTATIONS

I discuss here Sen's responses to the refutations, and show how they deviate from standard academic responses. In normal academic argument, someone might (1) make no response, (2) challenge all the refutations, (3) challenge some of the refutations, or (4) retract those parts of the work which had been shown to be wrong, making an attempt to save what is good about their work. In my rejoinder, I showed that Sen did none of these,

*'It is argued that Professor Sen has not attempted to answer most of the criticisms or to defend his misstatements. Where he has, he has introduced new misstatements. There are also some new errors.'*  
(Bowbrick P. , 1987, p. 1)

Sen (1986; 1987), does not even mention nine tenths of the refutations, indeed, he goes to great efforts to conceal them, indicating that he has no answer to them. He states for instance:

*'Bowbrick's disputation is based on arguing that the official statistics are unreliable, and the calculations derived from them erroneous' (1986 p127)*

That is to say, he denies the existence of all the other thirteen refutations. *Suppressio veri*. He then ignores the criticisms of the official statistics made by the Indian statistics profession, just repeatedly stating that his statistics were correct. *Suppressio veri*.

He makes no attempt to challenge other refutations either, let alone to do so in the normal academic way. Academic writing requires that he sets out the refutation he is challenging, then produces fact and hard theory to refute the refutation.

His responses make assertions unsupported by fact or theory.

The quotations in Sen's responses (1986, 1987), do not meet academic or professional standards: they are mainly snippets of a sentence, with almost no complete sentences, let alone the two or three sentences or the whole paragraph which may summarize an argument. He quotes just a few words from a sentence, without any context, theory or analysis. This is what one expects from a particularly dishonest politician. He does not think it necessary to reference them, nor to analyse their relevance.

Accordingly, it was my view, that of my professional colleagues, that of my academic colleagues, and that of the journal's editor (Blackman, 1986), that Sen had no defence at all to my many refutations, and there was nothing more to be said. I reverted to my day job – I was preventing a famine at the time. It now appears that there is a body of famine researchers who chose to suppress or misrepresent the criticisms of Sen's research. I examine below how they do this.

## SEN'S SUPPRESSION OF THE REPORT OF THE FAMINE INQUIRY COMMISSION

I showed that Sen had systematically misrepresented his sources, most particularly the Report of the Famine Inquiry Commission, on 30 to 40 key points, always in a way that would support his theory (Bowbrick P. , 1986; 1987; 1988, 2008). Nobody has made any attempt to challenge me on this.

The academic community has a standard procedure before dismissing or ignoring evidence of fraud. If two people make conflicting claims about what a source says, it is easy to check who is right. Academic rigour requires that anybody citing either of them checks the evidence first. In this case, checking means reading Sen's primary source, almost his only source, the Report of the Famine Inquiry Commission (1945a). It is shown here that, until this century, almost no academics could do this, because it was suppressed, first, by the Government of India, and, second, by Sen himself.

It was widely believed in 1945 that this report was so critical of the governments of Britain, India, Bengal and Punjab, in particular, that it would be politically explosive to publish it in the middle of the war, at a time of fierce conflict over independence and partition. As a result, only a very few copies were published, and members of the Commission were told to destroy all evidence given to the Commission (See Wavell in (Moon, 1973, pp. 36-7); (Aykroyd, 1974); (Bhatia, 1967)). I was able to find only two copies in Britain in the 1980s (and another one came to light thirty years later). Photocopies could not be circulated because libraries would not permit the photocopying of more than 10% of a document. This meant that 99.9 percent of the world's scholars and academics were unable to check Sen's work or my criticisms.

Neville Maxwell of Oxford University wanted to republish the report about 1980, but Sen prevented this from happening.

*‘Some years ago I was preparing to launch a library of books on South Asia . . . I, regarding the Report of the Famine Inquiry Commission as a classic and relevant account, proposed to publish it, and asked Sen to provide an introduction. He replied with a strong recommendation that the Report was so flawed as not really to be worth putting out as a book. At the time I had to take such a view from “the” leading famine authority (as I thought) as decisive: now I wonder if his motive was not rather to make sure that the Report was not widely read by people who could detect what he had done in his writing on it.’ (Maxwell, 1987).*

By contrast, the report was favourably received by an official who bore much of the Commission’s criticism, Binay Ranjan Sen, the official who was responsible for famine relief in the Bengal Government up to September 1943, when he became Director-General of Food for all India, and who later became a very effective Director General of FAO, expanding it greatly and switching its main effort to actually doing something to deal with hunger and famine. Binay Sen said that

*‘Sir John Woodhead, [the Chairman of the Famine Inquiry Commission] a former Governor of Bengal and a man of great integrity and competence, brings out the facts in their stark reality.’ (1982, p. 48) ‘Though I personally was an important administrative figure in the provincial government, I have to admit that, by and large, the observations and judgment of the Famine Commission were objective and correct. As I recall those tragic days I often wonder what more I could have done and did not do. Since I was responsible for emergency relief why did I allow things to get so far out of hand with no protest? Why did I not cry out louder when the aman crop failed and ask for planned supplies from other provinces?’ (1982, pp. 48-49). ‘The Bengal Famine Report came out in 1945 and what the Report brought out about the inadequacies and inefficiencies of the Central and Provincial administrations was already clear enough to us.’ (1982, p. 53)*

I put a copy of the Famine Inquiry Commission Report on the internet when this first became feasible, in the 1990s, but it was some years before many researchers outside the very rich universities had access to the internet. For most of the last thirty-five years, my web page provided the only copy available to most scholars. Effectively, Sen managed to conceal the evidence for 25 to 30 years.<sup>11</sup>

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<sup>11</sup> A problem that remains is that universities, worried by copyright, are reluctant to put sources online. Cambridge, for example, recently refused to keep an electronic copy of my collection of books and papers on the Bengal Famine in their archives. Some universities do make an effort and put the research on the web, but their web pages vanish after a few years, possibly because of this.

This means almost nobody in the academic community could check Sen's work, or the criticisms of it. Indeed, they did not adopt the normal academic procedure of writing to me and asking for a copy of my sources. So they avoided looking at information that might challenge their beliefs.

Clearly Sen had a strong incentive to suppress the report as he misrepresents his evidence on more than 30 key issues. Another incentive may have been that the possible reaction by academics to his overreliance on one source, and failure to reference it fully, giving his books and papers the false appearance of original scholarship. Both of these might be considered to be plagiarism.<sup>12</sup> The combination of plagiarism, misrepresentation of evidence and suppression of evidence is interesting.

I, on the other hand, wanted as many people as possible to read the report, and to check whether I was right. Similarly, on my web page I have always offered to supply copies of other sources to anyone who wants them: indeed, I have given students and researchers copies of the large library of books, papers, statistics, etc. which I have collected over the years. I am not frightened to have my work checked, or to supply evidence that might lead people, including me, to draw conclusions that do not agree with what I thought in the 1980s.

## ABUSE IS NOT EVIDENCE OR THEORY

Sen's response to me is personal abuse. This is of course totally unacceptable in academic discussion: The Economic Journal, for example, requires that *'the manuscript contains nothing that is abusive, defamatory, libellous, obscene, fraudulent, or illegal'* (I do not suggest that there is anything obscene in Sen's responses). The editor of Food Policy wrote to me, apologizing for publishing this personal abuse:

*'While I was at pains to ensure that he had the opportunity to respond to your arguments, I was not at all happy with the way in which he did. I did not think he was helping his case by sinking to snide remarks and these were edited out. Sen insisted that many of these asides were*

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This is particularly important because few academics are willing to share copies of papers, statistics and other evidence they have assembled, so we all have to work on a small subsection of what exists. It is not clear whether this is to prevent criticism of their work, or to prevent competing academics working on the same subject. I am happy to share my research files with anybody.

<sup>12</sup> *'plagiarism is an ethical matter, not a legal one. It is plagiarism to take as one's own the words of other authors ... As the AHA's [American Historical Association's] **Statement on Standards of Professional Conduct** reminds us, plagiarism "takes many forms". These may include "the limited borrowing, without attribution, of another person's distinctive and significant research findings ... or an extended borrowing even with attribution."* (Hoffer, 2004, p. 175)

*reinstated. I don't think Sen can really answer your criticisms so he is trying to mock them.'* (Blackman, 1986)

We learn in nursery school that personal abuse is not a valid argument, and that people resort to personal abuse only when they have nothing to support their argument. The message is repeated throughout our education. Abuse is totally unacceptable in academic or professional economics for this reason. And we are warned that anyone reading our work will reject it if it is full of abuse. Indeed, most of us will look for abuse in political tracts, for example, as a quick way of finding their weak or non-existent arguments.

I examine below the different forms of abuse used by Sen. A sneer or a snide remark is a form of personal abuse. It is a way of attacking a critic's credibility, or the credibility of unwelcome evidence, without having to produce facts and theory.

### Abuse as bullying

An effective way of bullying is to identify a group of people, to give them a name, to attribute certain beliefs to them, and then to ridicule the beliefs, and, by inference, the people, or, alternatively, to ridicule the people, and, by inference, the beliefs. This is often done to build up hatred against, or contempt of, a race, a religion, a nation, a political party and so on. A similar approach is used to create contempt for, or hatred of a belief, a political agenda, or the results obtained by a research programme. The approach is anathema in academic or professional discussion.

Sen has created a 'straw man' claiming that there is a widespread belief that famines are only ever caused by a sudden decline in food availability – a ridiculously simplistic belief. I have never met anyone who had this belief, and I have not come across any such belief in the literature. Sen labels this 'FAD' and shows that it is ridiculously simplistic, using abuse to ram home his point. He then claims that some analyses are the heresy of FAD, and that some authors are heretics, FAD believers. He then denounces the offending analyses or their authors in terms that have no place in academic writing.<sup>13</sup>

*"The view that famines are caused by food availability decline - the FAD view - was questioned on the grounds of cogency in the first chapter of this monograph" [It was not.] (1981 p154).*

*"The FAD approach gives little clue to the causal mechanism of starvation since it does not go into the relationship of people to food. Whatever may be the oracular power of the FAD view, it is certainly Delphic in its reticence." (1981 p 154)*

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*"A food-centred view tells us rather little about starvation." (1981 p154)*

*"The grossest category is, of course, the category of the entire population. It is on this that FAD concentrates, in checking food availability per head, and comes to grief (Chapters 6-9). The entitlement approach not merely rejects such grossness . . ." (1981 p 156)*

*"The FAD approach applied to the food availability for the population of an entire country is a gross approach, lacking in relevant discrimination." (1981 p157) [cf. Sen's analysis of the problems of Bengal, a province of 60 million people, as a single unit, and his rejection of Alamgir's (1980) district by district approach (1981 p63).]*

*"The empirical studies brought out several distinct ways in which famines can develop - defying the stereotyped uniformity of food availability decline (FAD)." (1981 p162)*

*"The FAD approach has led to disastrous policy failures in the past. [Sen's footnote:] The failure to anticipate the Bengal famine, which killed about three million people . . . and indeed the inability even to recognize it when it came, can be traced largely to the government's overriding concern with aggregate food availability statistics." (1984 p477)*

*"Like a Phoenix, the FAD theory arose rejuvenated from the ashes, and it can be found today chirping in the current literature on the food crisis of the world, even making occasional references to the Bengal Famine, 'when floods destroyed the rice crop, costing some two million to 4 million lives'" (1981 p83)*

*"If the FAD approach to famines were to seek refuge in some comforting bosom, it probably couldn't do better in the modern world than choose the Sahelian famine: the food availability did go down, and - yes - there was a famine!" (1981 p118)*

*"As we move away from the gross factual statements to a bit more detailed information, the FAD analysis starts limping straightway." (1981 p119)*

*"Second, the rationale of the FAD approach, concentrating as it does on aggregate supply, rests in ignoring distributional changes" (1981 pl 19)*

*"Thus, despite superficial plausibility, the FAD approach . . ." (1981 p120)*

*"The limitations of the food availability approach - its cluelessness - come out sharply" (1984 p 452, 1981 p434)*

Allan, (1986) adds others:

*'But on the basis of the leading item, Poverty and Famines, and a 1981 Quarterly Journal of Economics article, Professor Sen gives short shrift and is not exactly complimentary:*

*'[If] the approach of entitlements . . . appears odd and unusual, this can be because of the hold of the tradition of thinking in terms of what exists rather than in terms of who can command what. The mesmerising simplicity of focusing on the ratio of food to population has persistently played an obscuring role over centuries and*

The bullying means that readers of his work are frightened of doing the normal economic analysis, looking at a single situation and making a decision on what the problem is and what should be done about it: they are afraid of doing something that may be described as FAD. They avoid the FAD heresy, by trying to make the facts fit a changed demand model, suppressing the knowledge of history that the vast majority of famines have been triggered falls in supply, caused by drought, flood, disease, blockade or invading armies perhaps.

The bullying is reminiscent of the tactics of Lysenko, who did so much to destroy the Soviet Union's plant breeding in particular.

## Rants

In my professional life, I have observed people bursting into a loud, aggressive, rant, filled with personal abuse, when, for example, an examination of the accounts shows that they had their fingers in the till, or when an examination of their work shows repeated elementary mistakes. This is considered the normal way of saving face in some cultures. The authorities in these countries accept that there would be no rant if there was a legitimate defence, so they listen to the rant with due politeness, and ignore it.

## Throwing back the accusation

In some cultures, face-saving does not just require a rant; it requires that the accused throws back the accusation: if someone is accused of fiddling the books, they are expected to accuse the auditor of fiddling the books, for instance; someone who is accused of dishonesty is expected to accuse their accuser of dishonesty; someone who is accused of incompetence accuses the accuser of being incompetent. No evidence is produced, nobody is expected to believe the ranter, nor to pretend to believe them: it is just face-saving. The authorities listen, then ignore the counter-charge. Sen keeps using this face-saving technique.

Sen has attacked critics, using extraordinarily immoderate rants, combining personal attacks with sneering at the critics, at the evidence they present and at the theory used, including me, Tauger and Nolan (Nolan P. , 1991; Sen A. , 1991; Nolan & Sender, 1992; Sen A. , 1992; Nolan P. , 1993; Sen A. , 1993) . He also produced new, false statements, fraudulent evidence, when replying to my criticisms (1985; 1987;

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*continues to plague policy discussions today much as it has deranged anti-famine policies in the past' (Poverty and Famines, p8).*



1986) to Tauger's (2011) criticisms, and to Mukerjee's attack on Churchill (Mukerjee, Madhusree, reply by Amartya Sen, 2011). (see below). Throughout his writings there are abusive comments on economists who had made reasoned arguments which did not support him, or which produced evidence that he found inconvenient. The effect of the personal abuse is to intimidate readers into basing analysis on Sen's belief system, rather than the facts of the particular situation. Which is incredibly dangerous.

### Abuse as a response to my refutation

I examine below the use of abuse, including sneers and snide remarks, by Sen (1986, 1987) in response to my refutation of his theory of the Bengal famine (Bowbrick P. , 1986). His response is by no means the wild unstructured rant one expects from someone who has been shown to be stealing a few tons of rice from a store. On the contrary, it makes use of the sophisticated techniques used by politicians and public relations staff trying to hide a disaster.

### Systems 1 and 2, Pathos and 'dead cats'

I wanted my readers to be operating in the normal academic and professional 'System 2' mode, which is reason-based; it analyses with logic and evidence; action is based on conscious appraisal of events; reality is described in abstract words and numbers; the message takes longer to process. Hard thinking in fact. (See, for example, Epstein S., Slovic, 2007; 1994; Kahneman, 2012, for use of this concept in an economic context).

Politicians and confidence tricksters take a lot of trouble to stop people from using System 2 when examining what they have said and done: it is a lot easier to fool them if they can be manipulated into using System 1 instead. Sen's strategy was to get readers operating in the emotional, experiential System 1, and particularly in the 'affect' version, which has connections by association; it works through images, metaphors and anecdotes; the message can be processed rapidly; it is perceived as self-evidently valid, not requiring any evidence. Slovic talks of '*an affect heuristic in which people let their likes and dislikes determine their beliefs about the world.*' (Kahneman, 2012, p. 103) System 1 is a lot less time-consuming than System 2.<sup>14</sup>

This switch is also explained in the discipline of rhetoric, which makes the distinction between *logos*, providing the logic and information, and *pathos*, developing

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<sup>14</sup> It is not suggested that System 1 is useless. One version is experience, where for example, someone with a lot of experience of agricultural statistics uses this experience to spot immediately where a statistical series or statistical method is suspicious, then moves into System 2 to examine whether or not it is valid.

the emotion that will eventually get action (Leith 2011; Charteris-Black 2014). Dishonest politicians try to get readers to think with *pathos* only, ignoring the *logos* completely. Nowadays, they may, for instance, communicate mainly through Twitter, which does not give the space for logic and analysis, and so forces the discourse into *pathos* mode. Since the facts and logic support me, I write with a style and presentation designed to get readers to use a highly critical *logos* (akin to System 2). Since the facts and logic do not support Sen, he uses abuse as a form of *pathos*, producing emotions that shift readers into an uncritical, emotional, 'affect' approach.

It should be noted that journal editors and referees are also readers, who may well adopt 'affect' heuristics when refereeing, rather than System 2.

Sen also used the diversionary tactic which politicians refer to as '**throwing a dead cat on the table**'. When they are faced with an unanswerable attack on their actions or policy, they make some headline-grabbing statement – 'a dead cat' – so, whatever reasoned argument was going on before, the discussion immediately switches to the 'dead cat', and the logical, evidenced, argument is forgotten. The 'dead cat' may be an outrageous, obviously wrong claim, personal abuse, a highly contentious claim which is completely off the subject, completely new and unsupported claims of evidence which nobody has mentioned before. In politics a sex scandal may be invented, for instance, to divert attention.

An important part of Sen's strategy is to avoid discussing the facts which I presented, as this would give them credibility. The last thing he wanted was for readers to check them. *So he has made no attempt whatsoever to challenge* my meticulously referenced evidence that he misrepresented some 30 to 40 key facts. Any challenge would require extensive, full quotations, and the editor of Food Policy eventually made it clear that if there were any dispute on these, he, himself, would locate the original reference and publish the relevant parts so that readers could see for themselves who was right. Similarly, Sen **makes no attempt to challenge** any of my seven refutations of his full theory, nor of seven refutations of parts of the theory. Indeed, he does not mention most of them. Again, the last thing he wants is for readers to check his facts and theory: he aims instead to arouse their emotions. *Suppressio veri*.

### How Sen's theory can cause famine

I showed, clearly and rigorously, that the diagnosis of the cause of the 1943 famine, which Sen markets as his own discovery, was widely believed in Bengal in 1943, and that it influenced the responses of the elected Government of Bengal (which had direct responsibility for dealing with the famine), the Government of India, the governments of other provinces and the Imperial government. The post famine

consensus was that this diagnosis – a set of myths, of beliefs unsupported by evidence – was a major cause of the failure to take the action that would have prevented the famine. There is a wealth of supporting evidence in contemporary writings. It was also claimed in 1943 that the 1888 Orissa famine had been caused in the same way.<sup>15</sup>

This is fatal to the whole of Sen's theory. His diagnosis was tested in practice and shown not to work. This is unchallengeable, and **Sen does not attempt to challenge it**. Why? It is not just because he was wrong. Any discussion of the facts will show that his theory is plagiarized from the Famine Inquiry Commission report (1945a), and that he suppresses the fact that the Commission analysed the weaknesses of the theory and the effects that bad theory had on policy.

Accordingly, he switches the discussion away from the facts: he claims that I was suggesting that people had read his paper in 1888, and he challenges my sanity:

*'Even the title of the earlier version of Mr Bowbrick's paper, presented at the 1985 annual Conference of Agricultural Economics Society, was: "How Professor Sen's theory can cause famines" (no mean feat that).*

*'There is even a hint of battiness in the way Mr Bowbrick traces the evil effects of my "wrong theory" to periods earlier than its publication. Not only do we learn that during the famine of 1943, the government of Bengal fell to "adopting the measures that Sen recommends" and "the result was a famine in which three million died" (p122), but we are also informed that the entire "Bengal Famine Code" (1895) ". . . appears to be a reaction against the disaster caused by diagnosing the 1883 Orissa famine as a famine as a Sen-type famine and applying the measures Sen advises" (p 188). Given these beliefs, it is easy to understand Mr Bowbrick's intense anger – expressed liberally throughout his heady rejoinder – in the light of his evident conviction that Sen's "recommendations" and "advice" have been stimulating famines for nearly a century before their publication.'* (Sen A. , 1986, p. 125)

The attack has several functions.

First, Sen uses it to change the subject from the facts: that he was wrong, that he plagiarised, that he misrepresented evidence and that he concealed evidence that refutes his theory. Instead readers are led to something different, his personal attack, his sneer. The sneer is of course unfounded: it is normal English to say, for instance,

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<sup>15</sup> (Stevens, 1966) quoting *Government of India (1867), Report of the Commissioners Appointed to Enquire into the Famine in Bengal and Orissa in 1866, Volumes I, II, Calcutta*

'The present Prime Minister's policy on X has been tried out in a dozen countries over the past 150 years' without suggesting that any of these trials were influenced by him.

Second, he changes the mode of discussion from an academic discussion of the facts and theory to a purely emotive 'affect' mode. Nobody thinking in System 2 is stupid enough to believe Sen's attack if they think about it for ten seconds. But the point of the attack is that he gets readers to switch to System 1 'affect' mode, where they do not think about anything for ten seconds – certainly not to the facts and theory. It also acts as a 'dead cat': if anyone does start questioning the validity of the attack, they are certainly not thinking about the facts and economic theory which demolish Sen's theory.

Third it is an attack on my competence and sanity, with no supporting evidence.

Fourth, it changes how readers process what they are reading. This is a tactic often used by politicians when they are losing an argument. I expected readers of a serious economic refutation to be operating in the normal academic System 2 mode, which is reason-based. Sen has got them operating in the affect mode: emotional, pleasure-pain oriented: it has connections by association, it works through images, metaphors and anecdotes; the message can be processed rapidly; it is presented as self-evidently valid.

Fourth, once Sen has switched readers into the emotional, 'affect' mode, it is very difficult for the respondent to move the discussion back into the analytical. It is a 'Catch 22' situation: if I respond to the emotional attacks, I am playing Sen's game; if I stick firmly to hard fact and hard theory, it may appear that I have no answer to Sen's attacks.

And yes, individuals can and do cause famines. Anyone working on policy knows that the Minister and others can be strongly influenced by a rumour or urban myth that they are told in the club or at a party. It is stupid to pretend that Sen's myths are not more influential than the rumours spread by the barman at the club. We professionals may recognize that this is a myth, in that the people propagating the myth have no facts or theory to support it, but we must check the possibility that facts and theory might exist which support the same conclusions. Professionals must work hard to try and get decisions made on hard fact and hard theory.

### A fusillade of lies

Sen adopted the strategy of scattering false statements throughout his replies. A lot of little lies were scattered at random, all with the intent of getting the reader to believe that any of his critics were wrong or careless. Since most of his statements are very short, unargued, unevidenced assertions, they have the effect of switching the reader into System 1, emotional. They are scattered through his replies and are

interspersed through his defences on specific errors.

These are 'dead cats'.

Again, there is the 'Catch 22' problem: I replied to him in academic mode, using System 2, concentrating on his major errors, knowing I could switch top academics and hardened professionals into System 2, and convince them. However, it is extremely difficult to switch some readers back into System 2 mode, when they have just read a reply switching them into System 1 'affect' mode. The problem remained that the little lies remained unanswered, and that this might convince casual readers that I was wrong or unreliable. That is to say that they are personal abuse.

If, instead, I responded to each of these small lies, Sen would have achieved his objective, of diverting the discussion from his major errors. My refutation would be drowned in a discussion of trivia. I could indeed show that they were lies, but I would then appear to be nit-picking about trivia. And even refutations of small lies take time, taking me away from the real-world problems which I tackled as a professional economist in poor countries (I was preventing a famine when I wrote my rejoinder).

Sen's strategy has been adopted widely in recent years for politicians' tweets.

## FRAUDULENT STATISTICS

'Garbage in, garbage out'

One big lie, which Sen's theory depends on crucially, and that he repeats again and again, is that there was *at least* 11 % more food available in Bengal in 1943 than there had been in 1941 when there was no famine, so the famine could not have been caused by a decline in food availability. (Sen A. , 1984), P 477. Sen stated repeatedly that his estimates were both conservative and reliable.<sup>16</sup> He continued to

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<sup>16</sup> I set out some examples in (Bowbrick P. , 1986, p. 115), 'He says that his calculations are based on "a careful tally on food availability in Bengal". He talks of presenting "the results of a food supply calculation, taking into account local production and trade, choosing - wherever the data permit - an assumption as unfavourable to 1943 as possible". He concludes that "Current availability of food was at least 11 per cent higher than in 1941, when there was nothing remotely like a famine,". (Sen A. , 1984, p. 461), P 461 Elsewhere he says: "This is most certainly an over-estimate for 1941 vis a vis 1943, but this is an acceptable bias as it favours the thesis we are rejecting", "To bias the figures as much as possible against 1943 ... ," (Sen A. , 1977, p. 40), He may also be interpreted as claiming a much greater accuracy for them than is justified, because he frequently quotes different secondary sources as giving much the same estimate of total production or import needs. (Sen A. , 1977, pp. 53-4), Since these secondary sources are all based on the same official production estimates, no added confidence is given. His scathing comments on those who consider that the famine was caused by shortages emphasize the impression that he is totally confident of his figures.'

do so for the next thirty years.

This clearly requires that the rice crop forecast figures he used are amazingly, fantastically, accurate, virtually 100% accurate. Any agriculturist, agricultural economist or agricultural statistician would know that no agricultural statistics are that good, and that no economic statistics were that good. Indeed, anyone who has done Statistics 1 as an undergraduate must know that no statistics are that good. I showed that Sen's own source was highly critical of the statistics, and that the Indian statisticians at the time – the ones who created the crop measurement systems used around the world today – believed that the statistics were as bad as statistics can be.

I quoted Mahalanobis, the statistical genius who invented and developed the crop yield measurement system used around the world today, and Desai's review of the state of the Indian statistical revolution which supported him.

*'Desai provides a useful review of the agricultural and other statistics of this period, and his rigorous use of them is exemplary. He compares the official estimates of agricultural surveys with the results of scientific surveys carried out by Mahalanobis. He shows that the discrepancies are large, with survey estimates being between 47% and 153% of the official estimate. The discrepancies also vary from year to year, with the sample estimate of the jute crop being 2.6% above the official estimate in 1941, and 52% above it in 1946. (With jute, where exports provided a check, the sample proved correct.) Since there was no sample survey of the rice crop until after the famine, we do not know how inaccurate the 1942 forecast was.'* (Bowbrick P. , 1986)

I also quoted the Famine Inquiry Commission, whose criticisms of the statistics were damning.

**Sen does not attempt to defend his statistics against these criticisms** – there is no possible defence. Instead, he suppresses these criticisms and instead claims, repeatedly and without evidence or analysis, that his statistics are correct.

He dismisses Desai's review of the state of statistics and statistical work at the time, and the work of Mahalanobis and the Indian statistical revolution in just three words, as 'criticism from others'.

*First, he [Bowbrick] mentions that the Famine Inquiry Commission itself was critical of the official production estimates and the methodology used in that estimation, and he supplements quotations from the Commission with criticism from others, in particular from Professor P.C. Mahalanobis, the noted statistician (pp 111-2). (Sen A. , 1986)*

He then chooses to ignore the criticism, without further mention, neither summarizing it, nor trying to answer the criticism. *Suppressio veri*.

Evidently this dismissal was successful in concealing this large body of work: there are a lot of people who continue to believe that Sen's statistics and calculations were meaningful. Accordingly, I set out in the following sections some of the dozens of sources showing that contemporaries knew, and had known before the famine, that the crop forecasts were 'meaningless and 'worse than useless'. These criticisms are overwhelming, backed by the world famous and extremely influential agricultural statisticians of the time, by the administrators who collected the statistics and by users. Sen, too, knew of the criticisms.

### Production statistics

Contrary to Sen's repeated claim, there were in fact no 'production statistics' for the years up to 1943: there were the far less reliable crop forecasts. There is overwhelming contemporary evidence, from the statistics profession, that the forecasts were not merely '*unreliable*'; they were

*'useless for any purpose'*<sup>17</sup>, *'not merely guesses, but frequently demonstrably absurd guesses'*<sup>18</sup> *'a farce . . . a fraud'*<sup>19</sup> *'blatantly absurd results'*, *'disbelieved by the very government that produced them'*<sup>20</sup><sup>21</sup> *'no meaningful production statistics'*<sup>22</sup> *'not only incorrect but absurd'*<sup>23</sup> *produced by 'a system inherently vicious'*<sup>24</sup>

There were two separate problems discussed at length by the Indian statistical profession before and after the famine and quoted by the Famine Inquiry Commission. First, there was the quality of the raw data which could produce errors of over 50 percent in some years. Second, the raw data was 'adjusted' by administrators and politicians at all levels from the local police upwards. Again 'adjustments' of over 50% were noted. The combined effect of these errors could have been immense. The 'adjustments' varied in size and direction from year to year and were not random.

This implies that, first, we can have no confidence at all that any year's crop forecast lay within 90% of the true figure – except a belief that a further 'adjustment'

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<sup>17</sup> Bowley and Robertson (1934, p. 35)

<sup>18</sup> (Royal Commission on Agriculture in India, 1928, p. 605)

<sup>19</sup> (Dewey C. , 1978) p290

<sup>20</sup> (Mahalanobis, Organisation of Statistics in the Post-War Period, 1943)

<sup>21</sup> (Dewey C. , 1978) p298.

<sup>22</sup> (Bengal Land Revenue Commission, 1940, p. 76)

<sup>23</sup> (F.H. Villiers to Sir Edward Grey, 1914, quoted by Dewey 284.)

<sup>24</sup> (Trevaskis, 1931, p. vol 1 p 200) quoted by Dewey, 1978

would have been made if the forecast was absolutely ridiculous, second, the methodology used varied from year to year in non-random ways, so no two forecasts are comparable, and, third, no statistical series exists. I set out the evidence in more detail below.

It was deeply shocking to the Famine Inquiry Commission, and to the Indian statistical profession as a whole (see (Mahalanobis, 1943; Dewey C. , 1978; Desai R. , 1953) for summaries) that the raw data produced by the primary data collectors, the semi-literate village chowkidars, was altered by level after level of public servants, starting with the local police, and going up to the top. This is anathema to statisticians, outright fraud. The published figures were not statistical crop forecasts; they included some unknown mix of personal belief, keeping one's superior happy, departmental policy, and political interference. These forecasts were aggregated and "adjusted" by successive levels of Department of Agriculture officials. (Famine Inquiry Commission, 1945a, pp. 44, 45). *'All that is done is for the local police officers to make a guess, at which in succession the sub-divisional officer, the district officer, and the director of agriculture guess again'* (Hubback, quoted in Dewey (1978 p304.)) To a large extent, the adjustments' were made to bring the figures in line with the guesses made by Hunter and MacDonnell in the 1870s – seventy years previously (Dewey C. , 1978, p. 296). About half the estimates were 'adjusted' by officials higher up in the bureaucracy; adjustments of 30% to 40% were common, and changes of 60% to 70% were not unknown. (Panse & Sukhatme, 1954c, p. 26; Dewey C. , 1978, p. 305; Desai R. , 1953). We do not even know in what direction the statistical forecasts were 'altered' in any year.

This alone means that the forecasts for different years were not comparable, which invalidates any analysis of time series, and particularly obviously, Sen's comparisons of crop forecasts for 1940/1 and 1942/3. Which refutes his model completely.

The statistical methodology and execution were damned by statisticians, administrators and users for decades before the famine (See for example Mahalanobis's (1943) review of the literature<sup>25</sup> and Dewey (1978)), and by the Famine

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25 *'Mr. H. P. V. Townend has given a most valuable and critical account of the methods used for preparing official estimates of paddy acreage and outturn in the Memorandum submitted by him to the Bengal Paddy and Rice Enquiry Committee and published in Vol. II of the Report (Government of Bengal, 1940). He stated: —*

*'To the best of my belief there is no one who considers the present estimates of the annual outturn of aman paddy to be satisfactory; and the method of preparing them must therefore be condemned. In 1918 and 1919 when I was Director of Civil Supplies and it was essential to know what was the yield of paddy in Bengal, in order that Government might decide*



Inquiry Commission. The critics showed that the methodology was bad, that the chowkidars, the semi-literate village watchmen, who collected the raw data, did not attempt to collect according to the methodology, but invented the figures. Particularly noteworthy was the fact that, even when they did try to do the job properly, there were no crop measurements, no crop cutting and weighing of samples, just eye estimates, by the chowkidars, who were not agricultural experts, of the state of a crop at various stages of growth, who had to guess how the growing crop related to a purely notional 'normal crop'<sup>26</sup> There was no attempt at random sampling or crop cutting. Supervision was impossible when the standard mode of transport was the ox cart.

While it was obvious that the method of collecting raw data was dreadful, it was only about the time of the famine that it was possible to measure how bad it was. A revolutionary system of crop-cutting using random samples was developed, one later adopted around the world. This programme, started by Mahalanobis of the Indian Statistical Institute, produced the system that has been dominant for small-farm crop estimation ever since. Panse's book setting it out, the FAO edition, (1954b) was sent to Ministries of Agriculture and statistical offices throughout the world – and was used. The work of the Indian statisticians<sup>27</sup>, strongly influenced postgraduate courses in agricultural economics and statistics around the world from 1954 at least. Sanderson's 'Methods of crop forecasting' (1954) which showed many of the pitfalls, mainly in the USA, was also required reading.<sup>28</sup> I have not been able to find any evidence of anyone having anything good to say about the Bengal system, though presumably its originator in the 19<sup>th</sup> century thought it to be at least better than nothing.

The official crop forecast system, based looking at a crop and guessing what the yield would be three months later, ran side by side with the new sample system using crop cutting of actual harvest for some years, so a comparison was possible. When the crop was jute, the forecast could be compared with the amount entering factories. These comparisons showed enormous discrepancies which varied from year to year. (Desai R. , 1953; Dewey C. , 1978)

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*how much should be allowed to go out of the province, I found all calculations based on the crop forecasts to be defective; and it may be added the general public obviously share the official view that the crop statistics in general are of little value . . .*

*'as it is, the general public are so accustomed to disbelieve the forecasts that there is a tendency for them to disbelieve, without examination, any official estimates whatsoever as to the yield of aman paddy.'* (Pages 14-15.) Cited in Mahalanobis 1943

<sup>26</sup> While some administrators may have understood the notion and have some concept of what it was, it is difficult to believe that anyone else did, least of all the chowkidars.

<sup>27</sup> E.g. (Sukatme P. , 1954) (Sukatme & Kishen, Systems of Agricultural Statistics, 1951) (Sukatme & Panse, 1951) (Panse V. G., 1954a), (Desai & Shah, 1949)

<sup>28</sup> If anyone is interested in the development of this branch of statistics, I should be pleased to give them copies of relevant papers.

### When did Sen know how bad the statistics were?

Sen certainly knew how bad the statistics were *before* he wrote his books and papers on the famine. It was not just that The Famine Inquiry Commission repeatedly warned that they were unreliable. Tauger (2009) shows clearly that Sen knew Das's book, *The Bengal Famine* (1949) and had cited it repeatedly. Yet Das produced a long analysis (pp 98-105) of the statistics, showing, with quotations from a range of sources, that they were meaningless.

*'Das' arguments and evidence also raise an ethical issue. Sen, Greenough, and other scholars knew his book, cited it, and clearly considered it an important and trustworthy source. Yet none of them so much as mentions these passages in Das's study, let alone the fact that his sources and arguments undermine their evidence and the whole "man-made" or "market entitlement" argument. These scholars either did not read this passage, which would be negligence, or worse chose to ignore it rather than address the challenge that it posed to their own evidence and conclusions. Their writings would then be not an objective attempt to follow evidence wherever it led, but rather an exercise in contrived use of sources to support a preconceived interpretation, which because it blames the famine on British policies would have to be seen as a political argument.'* (Tauger M. , 2009, pp. 172-5)

Sen boasts of having been President of the Econometrics Society, which makes it difficult for him to claim that he did not understand the criticisms. It is difficult to imagine anyone studying economics or statistics in India in the 1940s and 1950s not being aware of the Indian statistical revolution. It is difficult to imagine them being unaware of the problems of collecting data. So it must be concluded that he lied.

### Systematic underestimation of poor crops

Mahalanobis said that the official forecasts had been so obviously wrong, so often, that nobody had believed them:

*'In fact, the official estimate had been quite low on several occasions in previous years, for example, in 1913-14, 1914-15, 1918-19, 1926-27, 1927-28, 1935-36, and in 1940-41, only two years earlier, was only 16.48 crores of maunds (60.43 lakhs of tons) which was much lower than the estimate for 1942-43 and fell short of the ten-year*

*average by nearly 27%, and yet nothing untoward had happened. It was, therefore, not unnatural on the part of the Government of Bengal to take the view that the official estimate for 1942-43 was unduly pessimistic as on many previous occasions. The cry of wolf had been so often raised in vain in the past that it was not surprising that every one was caught unawares when the wolf really did come.'* (Mahalanobis, 1943, p. 4)<sup>29</sup>

*'We thus find that since October 1942 the official estimates clearly indicated a large deficit; and yet we know that the position was not considered serious by Government. The official estimates were evidently disbelieved by the very Government which issued them'* (1944p3). <sup>30</sup>

Sen's statistical analysis is refuted in its entirety by the one fact that the 1940-41 forecast of a famine level crop was perceived at the time to have seriously understated production. The population as a whole was badly fed, with two thirds being malnourished in a normal year.<sup>31</sup> The official availability figures (I discuss the weaknesses of availability figures below) give a fall of 267 calories per head per day below the 10-year average<sup>32</sup> for the population as a whole in 1941. Since the well-off and the powerful would have eaten 'as much as normal' and some farmers ate more

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<sup>29</sup> One explanation put forward was:

*'Bowley and Robertson's considered judgement was that patwaris tended to report (i) no change from the previous year, or (ii) an average crop, when the yield is moderate, (iii) to underestimate a good crop, and (iv) to exaggerate the fall in the case of a bad crop (1934, p. 36). This is as near the mark as we are likely to get.'* (Dewey C. , 1978, p. 28)

<sup>30</sup> Many representatives of provincial governments and the Government of India came away from the First Food Conference on 14<sup>th</sup> Dec 1942, with the belief that the Bengal Government expected that Bengal would have a modest surplus of rice, and this belief was strongly influential in the following months (Pinnell, 1944) (Braund, 1944) (Knight, 1954). The official minutes state that

*"The Director of Civil Supplies, Bengal, said, "We do not require rice in the next few months, but statistically we are heavily in deficit for the coming year" When doubt was expressed about the correctness of the crop forecast, he added, "I should say at once that this is the first forecast. . .and the indications are that the final forecast is likely to be worse."' (Famine Inquiry Commission, 1945) p45*

It is not possible to know the truth at this stage. I must thank Mark Tauger for identifying Knight's work.

<sup>31</sup> See the Famine Inquiry Commission for a discussion of the nutrition status of the population.

<sup>32</sup> The calculation of calories is based on (Famine Inquiry Commission, 1945b, p. 215) (Statement III p215)

than normal, most of the rest of the population would have to reduce their consumption by 500 calories per day or more. They would have gone from malnourished to a state close to starvation. Some would have starved. Very much what did actually happen in 1943, in fact. If one allows for the non-rice foods, affected by the same weather, the situation would have been even worse. The fact that this did not happen supports the widespread belief that the 1940-41 crop forecast was yet another false alarm and the true production was perhaps a little less than average.

If the 1940/41 crop was, in fact, slightly below average rather than disastrous – which fits what was observed – it cannot be claimed that there was less food available in 1941 than in 1943. The error in this particular year may have been due to a bad data collection and analytical system, or to the ‘adjustments’ (fraud) by officials. The understatement did not require any malicious intent, or even the massive ‘adjustments’ (sometimes over 50%) that had been observed in the past. It could be that there was a strong rumour, a myth, that the crop was small, so that people at each level from the chowkidar to the Department headquarters, adjusted the forecast downward by 5%, not realizing that someone else had done the same already.

This alone refutes Sen’s analysis in its entirety – there is no reason to believe that there was more food available in 1941 than in 1943. The official crop forecast, that supplies would be so low that there would be a famine in 1943, cannot be dismissed.

There may have been another, very much greater, bias. The Famine Inquiry Commission is clear that, until 1941-42, crop forecasts were ‘adjusted’ by a series of officials and by the Department of Agriculture. The 1942-43 forecasts were not ‘adjusted’ for the Director of Agriculture’s belief that the true crop was 25% less than the forecast. The first crop forecast for the aman (December) crop, published on 5<sup>th</sup> October 1942 was for a reduced crop, 17% below the ‘normal yield’ because of low rainfall in the first half of the season. The Director of Agriculture believed that this forecast overstated the probable yield by 25%, that is to say it should have been 62.5% of the normal yield, 4.6 million tons in total rather than the actual official forecast of 5.7 million tons. His view of the final crop forecast was:

*‘It was thus considered likely that the main (winter) rice crop in 1942-43 would fall short of the last season’s crop by 22.8% according to reports received or by 38.1% in the opinion of the Director of Agriculture himself.’ (Mahalanobis, 1943, p. 3)*

This means that the forecasts for 1940/41 adjusted by the Director of Agriculture, and that for 1942/3 not corrected by him are not comparable. Sen’s claim that there was more food available in 1943 than in 1941 is, therefore, untenable. And this alone refutes his theory.

There was another change in methodology which had the effect of

overestimating supplies in 1943 compared to 1941<sup>33</sup>, but, as it was not mentioned in Sen's sources, its omission does not imply fraud on Sen's part

The changes in methodology could have been a response to the unnecessary alarm, disruption and expense caused by the underestimation of the 1940/41 crop, and a series of other crops. It may be that the Director of Agriculture decided, or was ordered, to work on the unadjusted reports of the chowkidars, and to present the Department's beliefs separately.

It is a sign of the dreadful state of statistics at the time that there could be very different methodologies used for crop forecasts in 1940/41, 1942/3 and 1943/4, and the figures could be presented as though they were a single series. Statisticians are obliged to mark discontinuities in series clearly in their tables, and to put notes at the bottom of the table to explain the discontinuities.

The reliability of the official forecast is so low that much larger errors could be expected from time to time. It is well within the known margin of error of the forecasts that the 1940/1 estimate could have been half the true figure and the 1942/3 estimate could have been twice the true figure. In the absence of meaningful statistics, we must look at all the other evidence to see if the famine was as bad as this suggests.

Mahalanobis discusses the reasons the forecasts were so bad. (1944; 1945-6; 1946). He was so important as a statistician because he did not just produce the new sampling theory; he ran the surveys, and saw the practical problems that had to be solved before the theory could be applied. He described his work in developing a sample-survey system with crop-cutting (1946), showing that he recognized the multiple weaknesses of the old system, and that his new system, still in the early stage of development, was handling these. He was concerned with checking field staff, to see that they did indeed visit the fields, follow procedures, make correct measurements, and record them correctly. He also had checking procedures for office staff. He

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<sup>33</sup> There was another effect of the change in methodology which is not mentioned in Sen's sources: Dyson (2020) says 'Islam (2007) shows that the figures on harvested area for 1938-41 were treated differently to those for 1942 and 1943. Consequently, the "harvested area" figures for 1938-41 must be **raised** to make them comparable to the figures for 1942 and 1943. When this is done then—solely on the basis of increasing the harvested area—the availability of R&W is lower in 1943 than in 1941.'

Islam concludes that "the shortfall in *aman* production in 1943 [actually late 1942] was not as 'moderate' or 'indifferent' as he [i.e. Sen] would have us believe: the decline was a marked one (30 percent compared to 1942 [actually late 1941] and 20 percent compared to the previous quinquennium). The decline in *aman* production made a more significant contribution to the 'exceptional shortfall in market release' (and the consequent sharp rise in food grain prices) than he seems to concede." (Islam 2007: 437)

Again, Sen's fundamental claim that there was more grain available in 1943 than in 1941 is shown to be false. Islam refutes Sen's theory.

considered it very important indeed that the Indian Statistical Institute, where he worked, was not part of government and so he was able to resist pressure to fabricate the results.

Dewey provides an excellent study of the effect that Indian public servants from the chowkidars to the top had on the quality of statistics in India. The production forecasts were seen to be particularly bad in Bengal, as they were not linked to the land revenue system there. He describes how the Indian statisticians responded to the problem. He reaches similar conclusions to Mahalanobis, from a different perspective.

### The Fungus outbreak

The fungus outbreak shows why agriculturalists, economists and statisticians are deeply suspicious of crop forecasts. The Famine Inquiry Commission believed from 'meaningless' crop forecasts that the fungus had caused more damage to rice crops than the cyclone.<sup>34</sup> There is good reason to believe that the guess at yields was far less reliable than that in normal years, and that it was likely to have overstated the crop. It is certain that even if, miraculously, the forecast had been correct in its estimate of weight of unmilled rice harvested, the amount of edible food would have been significantly overstated. The margin of error from this alone prevents any meaningful comparison of rice availability in 1941 and 1944, so invalidating Sen's theory.

There was an exceptionally serious and widespread fungus outbreak in Bengal in 1942, an epiphytotic. Something like this had previously been seen perhaps one year in ten or twenty, in only one country in the world at a time. Neither the chowkidars nor the agriculturists had seen anything like this before. They could only make a wild guess as to what effect it would have on yield. There is some reason to believe that in such cases people are likely to overestimate the crop (See Appendix 1 for more details). (Tauger (2009) found the scientific evidence on the fungus, and reached the conclusion that the fungus had a much more serious effect than had been recognized at the time, both on the aus crop and on the December one.

When the crop was harvested, then dried out for two or three months, in March and April 1943, rice was taken to be milled. It was found that there were fewer grains than expected, that they were small, and that many were so infested by fungus as to be inedible (Amery, 1988). This means that even if the government had known how

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<sup>34</sup> The areas worst hit were devastated by a storm surge, a tidal wave, but wind and rain damaged crops over much of the province. The Bengal Famine Manual (Bengal Government, 1941) is clear that a famine caused by a storm surge is the worst kind, with devastation that requiring instant, and major, relief programmes. A famine should be declared instantly. However there was never any declaration of a famine, because Government could not procure enough food for the relief laid down under the Famine Code.

much was harvested, they would not have known how much was eaten.

A plant pathologist in the research centres concluded,

*‘Nothing as devastating as the Bengal epiphytotic of 1942 has been recorded in plant pathological literature. The only other instance that bears comparison in loss sustained by a food crop and the human calamity that followed in its wake is the Irish potato famine of 1845.’*

(Padmanabhan S. , The Great Bengal Famine, 1973)

He presents a table of the ‘Yield of rice per hectare at the Rice Research Stations at Bankura and Chinsurah in the epiphytotic year (1942) compared with yield per hectare the stations in a normal year (1941)’ showing that for the critical late *Aman* crop, which provided the bulk of the food, yields in trials were down by 39.5% to 91.2% in different varieties, with nearly all varieties having losses of more than 70%. I present fuller information in Appendix 1: The Fungus.<sup>35</sup>

Mark Tauger, who found Padmanabhan’s work, later set out its conclusions succinctly (Tauger, Mark and reply by Amartya Sen, 2011). He also pointed out that Padmanabhan was an acknowledged leader in the pathology of such fungi, and that his conclusions were accepted by rice agronomists and pathologists in India.<sup>36</sup>

Since Tauger is rigorous and his evidence unassailable, Sen sneers – and the number of sneers and personal attacks here is a tribute to the power of Tauger’s criticisms. Sen also presents a large amount of false evidence (which I discuss elsewhere in this paper. He suggests that Tauger’s evidence that there was a fungus attack depends on results of fields in just two research stations rather than two regional research centres, and that, because they are four hundred miles apart the fungus may not have occurred in between! Sen then suppressed the experience of the people who took their grain to the mill, *suppressio veri*, then asserted, without evidence, that the scientists were wrong, and then asserted that the chowkidars were right. And so on. (Tauger, Mark and reply by Amartya Sen, 2011). It would take a 20,000-word paper to list and briefly answer all the falsehoods produced by Sen in this brief exchange.

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<sup>35</sup> I must thank Mark Tauger for finding this body of research and Padmanabhan’s papers and alerting me to their existence.

<sup>36</sup> ‘Both the International Rice Research Institute and the Indian Central Rice Research Institute (click on “Overviews,” “Background and Location”) attribute the Bengal famine to the plant disease that sharply reduced the 1942 harvest.’ (Tauger, Mark and reply by Amartya Sen, 2011)

## Did Mahalanobis correct the statistics?

One chapter of The Famine Inquiry Commission's report, that on rice availability, claimed that the famous statistician, Professor Mahalanobis, supported the calculations of availability, while the rest of the report repeats Mahalanobis' criticisms: he was, in fact one of the harshest critics of these figures. (1944; 1943) (1946). He thought that the failure to deal with the famine was because decision-makers had only dreadful crop forecasts to base their decisions on. He managed to raise the money to start the Indian statistics programme which produced accurate measures of crop production, a methodology that was adopted worldwide (1944) (1946). He cites a lot of statisticians and administrators who had observed the forecasting and had damned the figures long before the famine.<sup>37 38</sup>

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*'The Royal Commission on Agriculture (1928 p605) under the chairmanship of Lord Linlithgow himself [By 1943 Lord Linlithgow was the Viceroy, and took personal charge of the food portfolio] was of opinion that these 'are admittedly often mere guesses and are not infrequently demonstrably absurd guesses'. Dr A. E. Bowley and Mr. D. H. Robertson thought 'the annual statistics of area to be almost worthless'. [Report on a Scheme for an Economic Census of India, 1934, pp. 36-39.] The Bengal Paddy Enquiry Committee (1938) fully endorsed these views; and speaking of crop estimates expressed the opinion that these were least developed and least dependable of all agricultural statistics in Bengal. [Report, Vol. 1, p. 88.] The Bengal Land Revenue Commission (1940) observed that 'no dependable statistics existed in Bengal ... to show the yield of the various crops' [Report, Vol. I, p. 76[ quoted the complaint made in 1876 by MucDunnell who had commented on 'what has been held to be a great want to an administrator in Bengal, the want of agricultural statistics'. [Report on the Food-grain Supply of Bengal and Bihar. Introduction, p. iii] and*

*'What is to be really regretted is that in spite of strong and unequivocal condemnation of existing crop statistics by a series of most authoritative Committees and Commissions from 1876 to 1940, no steps whatsoever had been taken, even in the period of grave war emergency, to improve the collection of statistics relating to rice.'* (Mahalanobis P. , 1944)

<sup>38</sup> Dewey (1978) quotes statisticians and administrators as saying that Indian statistics were '*not merely guesses, but frequently demonstrably absurd guesses*' (Royal Commission on Agriculture in India, 1928, p. 605). Bowley and Robertson (1934, p. 35) thought them '*useless for any purpose*'. It was also stated that '*no dependable statistics existed in Bengal*' (Bengal Land Revenue Commission, 1940, p. 76) that there were '*no meaningful production statistics*' (Mansergh, 1973) and '*not only incorrect but absurd*' (F.H. Villiers to Sir Edward Grey, 1914, quoted by Dewey 284.) '*the resultant statistics were proclaimed "a farce, and the annual papers a fraud"*' (Dewey C. , 1978) p290, '*blatantly absurd results*' (Dewey C. , 1978) p298. Even if the data collection had been perfect, the lack of random sampling, and the averaging procedures used produced errors which would have invalidated the statistics for most purposes (Trevaskis, 1931; Panse & Sukhatme, 1954c; Dewey C. , 1978).



Only one section of the Famine Inquiry Commission reports claims that the statistics had been corrected, and Sen repeats this claim time after time. That is to say, his claim is that Professor Mahalanobis magicked 17 years of statistics which everybody considered to be meaningless or worse than useless to make them correct – that he had sat in an office and corrected 17 years of crop *forecasts* (no attempt had been made to measure the actual production), turning them into correct production figures, and he had done this long after the crops had been harvested, when there was no possibility of even looking at the growing crop.

Clearly, nobody who gives the matter two minutes' thought could believe this. However, people who are working in the System 1 'affect' mode do not give things two minutes' thought.

Whoever wrote this chapter for the Famine Inquiry Commission was not referring to the forecasts of yields made by the chowkidars: he was referring to the fact that Mahalanobis's work for his own survey methodology suggested that the method of aggregation from district to national level caused biases in the long run. He suggested that weighting by another data set, of unknown bias and accuracy, might be better. The Famine Inquiry Commission chapter showed that, over a seventeen-year period, there was an accumulated discrepancy between the results when using the two different weights. Since we are only interested in the forecast for 1940/41 and 1942/43, not in a seventeen-year period, the discrepancy is negligible. It is disturbing that such a claim should have been made.

Mahalanobis did set out some reasons. In 1943, well before the Famine Inquiry Commission was even appointed, he said.

*'For it is not possible to call for statistics and get them at a moment's notice by issuing an administrative fiat. Or rather I should say that although it is always possible in this country to get some kind or other of statistics by executive order, in most cases such statistics are likely to be entirely worthless.'* (1943, p. 5)

*'Then there is the temptation of intellectual dishonesty. Professor Mahalanobis gave examples throughout his paper: the temptation of the field worker to "fudge" his figures, the political pressure resulting in an inaccurate census, the hint of the virtual suppression of an unpalatable report. . . fixed opinions and unreasoned prejudices, and I know how easy it is to hunt for figures to prove a theory rather than to hunt in the figures for the truth. (Elphinstone, M. D. W., discussing Mahalanobis 1946 p374)*

*'I entirely agree . . . that the apathy of administrators and the peculiar difficulties in which statistical work has to be carried out in*

*India has to be experienced in order to be properly appreciated. The average administrator in India expects the scientific or statistical technician to supply evidence or proof in favour of what the administrator thinks to be right rather than to give independent advice on objective grounds. Intellectual dishonesty . . . would in such circumstances be an actual advantage in securing promotion in official posts' (Mahalanobis P. , 1946, p. 378)<sup>39</sup>*

It appears that the official who prepared the tables for the Famine Inquiry Commission acted in this way. Sen kept up the tradition, manipulating their tables in an unacceptable way, to produce results favouring his theory. The Famine Inquiry Commission itself did not include any statisticians, and the only economist on the Commission refused to accept these figures, producing a minority report with different estimates of availability.

The availability estimates are also meaningless

Sen relies on availability estimates, which were known to be meaningless for other reasons than dreadful crop forecasts. The claims about availability in the statistical chapters of the Famine Inquiry Commission Report and in Mahalanobis's (1943) paper are a rough calculation, adding up the food in stock at the beginning of the calendar year, the new crop, and imports, and subtracting the food stock at the end of the year. Both Mahalanobis (1943, 1944) and Desai (1953) are highly critical of the estimates.

1. The import-export statistics referred only to rice arriving at Calcutta or leaving Calcutta by rail or steamship. Inter-provincial trade by traditional 'country boats', the normal method of trade over the centuries, was ignored.<sup>40</sup> (So it would not be possible to quantify the very large quantity of rice that had been exported in 1942, to take advantage of the high prices elsewhere in India – especially when the government imposed price control without closing the borders (Pinnell, 1944)).
2. Mahalanobis said,  
*'No reliable statistics relating to . . . stock of rice was available.'*

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<sup>39</sup> This is by no means confined to India, or to poor countries. I have seen it in Britain.

<sup>40</sup> Pinnell (1944) considered this informal marketing system to be very important, and fought successfully to prevent the military from seizing and destroying all country boats in Bengal as a defence against the imminent invasion.

A little thought confirms this. In a country where perhaps 90% of production was by farmers who produced primarily for subsistence, village storage was particularly important, but impossible to measure. Local stocks were held by landlords, money lenders and farmers. Conceivably, pre-war estimates may have been obtained by writing to some of the bigger traders, but statistics that do not give this information in their footnotes are of little value. One might expect some better figures in wartime, but Pinnell (1944) says that a significant number of the firms that were supposed to have registered had not done so, and neither the Civil Supplies Department nor anyone else had the administrative manpower needed to locate them. Still less did they have the manpower needed check returns by physically counting the bags in stock (notoriously, neither bin cards nor accounts are reliable). In addition, there were stocks held by urban manufacturers and by chambers of commerce, etc. but these were not recorded until December 1943, when they were no longer important. Stocks held by government organizations and by the army, are not known.

3. Mahalanobis said *'the accuracy of this Census figure has been questioned. What is the annual increase in population? We do not know.'* (See also Mahalanobis (1946 p367). The Director of Census agreed. The three censuses before the famine each had their problems and were not comparable, so no population growth rates could be calculated. (Government of India, 1946, pp. 3, 24) The Bengal figures for 1941 were particularly bad: *'Mr Dutch in Bengal had the heaviest load of all . . . local excitements and intransigence ... in so troubled a post'* (Government of India, 1946) p3. [i.e. he managed to complete something that looked like a census report in a province on the verge of insurrection]. The Director believed that the 1931 census understated the population of India because of a political boycott, and the 1941 figure may have overstated the population because each religious community was trying maximize its population for future elections etc. This meant that the rate of population increase between 1931 and 1942 was certainly overstated by the Famine Inquiry Commission. This is how statistics should be presented: reporting the figures actually collected, and reporting the errors and biases observed, so that the statistics would not be misused. The post-war census is not comparable either, because of the massacres and ethnic cleansing of Partition.

Sen was perfectly aware that the figures for the different famines were not comparable – Das (1949) who Sen quotes repeatedly when it suits him, is emphatic on this. Yet Sen's estimate of famine deaths (Sen A. , 1980) was based entirely on the assumption that they were. Dyson and Maharatna (Dyson, 1991) (Dyson & Maharatna, 1991) produced a full and rigorous refutation of Sen's research on famine mortality.

4. Consumption of rice, or other foods, per head was not known.

*'What is the consumption of rice per head? We have no data but only vague estimates ranging from less than 4 maunds (= 329 lbs.) to 6 maunds (= 494 lbs.) of rice a per head per year—all, of course, based on personal impressions.'* (1943)

A difference of 50%! We know nothing about other foods, including grains, cassava, oil, roots, bananas and plantains. A few minutes thought will give some idea of the problems of quantifying the consumption of illiterate villagers, especially as many were growing partly or wholly for subsistence, eating food they had not bought. Differences in what Hindus and Muslims or people in different ecological zones eat complicate the issue.

There is no possible measurement of the amount of food carried over from year to year. It would have been stored by farmers, landlords, moneylenders, and traders carrying out many different functions. There were none of the early warning systems developed in later years, with officials going through the villages looking into grain stores to see how much is stored compared to the normal amount. How much rice was in stock at the beginning of the famine year? How much was in stock at the end? Nothing? Four months' supply? What allowance should be made for fungus damage? We can only guess, assuming, for example, a larger carryover after a good crop<sup>41</sup>. (and I confess to having guessed). We certainly cannot assume away contemporary views that the combination of two disastrous crops (1940/1 and 1942/3) and the use of the intervening 1941/2 crop to replace the Burma imports, to feed refugees and the army (up to August 1942) and to supply other provinces which were seriously short, meant that there was virtually no carryover (It is common that a food crisis is caused by a succession of bad crops rather than one disastrous crop). There was an official perception of a food crisis throughout the last quarter of 1942 (Pinnell, 1944). Nor can we ignore reports of some people (numbers unknown) being sick because they ate newly harvested crops months before they were ready. And it is not possible to challenge the minority report 'In face of these observations any material 'carry-over' at the end of 1942 was a mere myth.' (Famine Inquiry Commission, Minute by Mr M. Afzal Husain p188).

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<sup>41</sup> In market economics we use different measures of carryover for different decisions. It is defined in terms of cost and timeliness of access and transport availability. Up to 1939, world carryover was appropriate for many decisions, the war limited the number of countries that could or would supply, then the submarine warfare and shortage of ships, especially during the Battle of the Atlantic, so effectively only Indian carryover was relevant. When the Provinces were given control of imports and exports of grain, there was a sharp distinction between Indian carryover and Bengal carryover. The loss of Burma, then transport problems in Bengal, meant that, in practice, Bengal had to rely almost entirely on its own carryover.

5. The timing of imports is critical in a famine year. Post-war experience is that, all too often, they only arrive shortly before the next crop, or after the next crop is harvested, and so may have little or no impact, or indeed be harmful, reducing the price for the next crop. The nominal date of arrival may be misleading, the date of arrival in port, for instance. Did the ship have to wait for a berth, or did it get a berth at all? Unloading into a lighter took longer. Unloading meant dockers going down into the hold and filling bags with a shovel, then carrying them out, which could take a month. Was the port Bombay, in which case it could mean a month to get it to Bengal, or was it Calcutta which was still some distance from the starving population? It could take another month or two to get the grain from Calcutta to the starving, by river boat or ox cart. The last mile could be the most difficult. This means that the availability figures based on import statistics may overstate the food which consumers could eat during the famine.
6. The uncertainty about when imports would arrive, or whether there would be imports, was a major factor throughout the famine. There can be no statistics on perceived availability during the famine.<sup>42</sup>
7. The calendar year is convenient for statistics but may not be appropriate. Crop years, consumption years and calendar years are not the same, and indeed, in some cases, crop year and calendar year were used in the same series.

### *Combined effect*

This means that none of the elements of the availability calculations are valid, not the crop forecasts, not the imports or exports, not the storage, not the population, not the population growth rate.

I have worked in some 38 countries, rich and poor. None had the information needed to make the calculations of availability presented by the Famine Inquiry Commission or Sen. Some of these countries had equally poor statistics. Few had anything meaningful on stocks, or consumption, and one or two had some idea of the accuracy of the statistics that they did produce. Countries, as opposed to provinces like Bengal, had impressively printed import: export statistics but some of these were

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<sup>42</sup> Tauger (2009) shows that there were major crop failures in other parts of India, which certainly affected the availability in India as a whole, and the willingness of other provinces to export to Bengal.

badly misleading, where there were leaky boarders, or some types of criminality.

So why did both Sen and the administrator who made these calculations for the Famine Inquiry Commission claim repeatedly that Mahalanobis approved of the availability calculations, and why did they not, at least, set out his views as caveats?

#### Sen's claims of accuracy

*'On two occasions I have been asked, "Pray, Mr. Babbage, if you put into the machine wrong figures, will the right answers come out?" ... I am not able rightly to apprehend the kind of confusion of ideas that could provoke such a question.'*

Charles Babbage. *Passages from the Life of a Philosopher*. (1864, p. 67)

**When Sen is shown to have made very serious false statements, he does not try to defend himself, nor does he withdraw the falsehoods. He diverts attention by a sneer,**

*'Instead, Bowbrick's disputation is based on arguing that the official statistics are unreliable, and the calculations derived from them erroneous (with a "margin of error", he says, "of 3000%")' (1986 p127)*

This is a lie of course. I did not suggest that Sen's *calculations* had a margin of error of 3000%. I said that *his repeated claim of reliability and accuracy*, had a margin of error of 3000%. He had claimed in the strongest terms that his version of difference between the 1941 and 1943 availability figures were *reliable and accurate*, This claim requires that the crop forecasts for both of these years were virtually 100% accurate, when we know that they have on occasion been wrong by more than 50% and all contemporary statisticians thought they were meaningless. I was discussing only the comparison using the crop forecasts – the other elements of the 'availability' calculation were also suspect, so I would now argue that the margin of error was much higher, and that the unreliability was completely off the scale.

Economists are taught from undergraduate level that, if you feed data that are inaccurate into your model, there is a very high probability that the results will be more inaccurate still. This is shown clearly by the simple example of a firm with raw material costs of 60% of turnover, operating costs of 39% and a profit margin of 1%. A 10% error in the price of raw materials (i.e. 6% of turnover) is 600% of the profit margin. A 10% error in the operating costs (i.e. 3.9% of turnover) is 390% of the profit margin. A 10% error in both could mean an error of 60% of turnover if they cancel out and 1140% if

they do not. This is in many ways the typical economic model.

Morgenstern (1963) quotes an example from Milne (1949) to show that slight errors in input can cause enormous errors in output. He takes two equations: -

$$x - y = 1$$

$$x - 1.00001y = 0$$

which have the solution  $x = 100\,001$  and  $y = 100\,000$

The almost identical equations

$$x - y = 1$$

$$x - 0.999999y = 0$$

have the solution  $x = -99999$  and  $y = -100\,000$

While this is an extreme example, you cannot know whether your model will produce similar results unless you test it with variations of the data within the limits of its error. It is possible that the errors might cancel out, but it is equally likely that they would multiply. This does limit the type of economic and econometric analysis we can do, and it means that it would be most improper to claim accuracy and reliability when there is none.

That is to say, Sen lied, and when he was challenged, he lied again.

Not producing any 'alternative set of production figures'

Sen's sneer that,

*'He [Bowbrick] rejects the output figures that I had used, taken from the report of the official Famine Inquiry Commission, but he does not go so far as to produce any alternative set of production figures.'* (Sen 1986 pp 127-8)

confirms his total lack of integrity. If we do not know the correct figures, we must say so. I most certainly would not invent a set of production figures when there are no statistics that were considered meaningful. And, no he does not present 'output figures' or 'production figures' – the Famine Inquiry Commission figures were crop forecasts, with the forecasts of yields reported by the chowkidars being weighted by different guesses of the area planted in each division.

In later years Sen has changed his story, now denying that the figures he used

were 'taken from the report of the official Famine Inquiry Commission'.. In his criticisms of (Tauger, Mark and reply by Amartya Sen, 2011), (Mukerjee, Madhusree, reply by Amartya Sen, 2011) we see him denying that he took the '*output figures that [he] had used . . . from the report of the official Famine Inquiry Commission*' and claiming that he had at least looked at other sources. He states, '*we have seen the parading of a blatantly false statement to the effect that all my food production data for the Bengal famine were based on "crop forecasts."*' (May 12, 2011) But, as Sen's own statement above makes clear, they were indeed based on crop forecasts (Sen 1986 pp 127-8). And, of course, neither Tauger nor Mukerjee had claimed that Sen's figures were entirely, 100% based on these figures: on the contrary, there were further 'adjustments', fudging, fiddling, using other guesses, estimates etc. to the crop forecasts, making meaningless figures very much worse but the *base* is entirely the forecast. So Sen's statements are *suppressio veri, suggestio falsi*. The use of the words, '*parading of a blatantly false statement*' constitutes *abuse and bullying*, but his whole contribution to this exchange is full of similar abuse.

When one constructs an economic model of any market, one faces the problem that there are areas of the model for which there are no statistics, perhaps no hard evidence at all. Rigour demands that we try and identify where these gaps are, and to say, 'We do not know . . .' or even 'we cannot know'. We may then try and reduce the area of uncertainty by finding other sources of information. Fabricating statistics gives the wrong answer.

Assuming that something does not exist because we have no statistics can be worse, making the whole model a disaster. Any model which does not take into account foods other than rice and imported grains is dangerous. Bengal had no statistics on foods other than the main crops, foods like the less important grains, pulses, cassava and other root crops, fruit and vegetables, oil seed crops, coconuts, etc., milk and animal products. While each may be a small part of the total they can add up to a significant part of the total. Again, for some groups in some ecological zones they may be a more important source of calories than rice. We must acknowledge this and try to get around it.

And, of course, Sen did not produce any 'production figures': he produced meaningless crop forecasts.

Meaningful results from meaningless data?

Sen sneers,

*'Mr Bowbrick is consistent in always rejecting statistics on grounds of their unreliability ('Nearly all agricultural price*



*statistics, and particularly prices in such markets, are subject to enormous errors', p 109) in favour of no statistics at all.'*  
(Sen A. , 1986, pp. 131-2)

Yes, indeed. That has been the standard practice in all the sciences since Aristotle (350 BC, pp. I, 3), at least. Any economist or statistician would reject statistics based on a methodology that had been shown to give a reasonably correct figure one year and to have an error of more than 50% next year, and I quoted evidence that the Bengal crop forecasts were worse than this (1986). By contrast, Sen's reaction is both to suppress the fact that there were any errors at all in the statistics, and also to suppress any statistics that do not support him: *suppressio veri*. After the famine, the Bengal Government, belatedly, acted according to standard practice: they realized in September 1943 that the failure to deal with the famine was because the crop forecast statistics were so bad and so unreliable that nobody would act on them (Mahalanobis, 1943). Accordingly, the Bengal Government and the Indian Statistical Institute launched an emergency programme to develop a methodology for production figures based on statistical sampling, crop cutting and measuring the actual yield.<sup>43</sup> At the same time the government tightened up the operation of the old crop forecasts. In particular, only one 'adjustment' was permitted: the forecast could not be released until it had been adjusted to be compatible with the results of the crop cutting survey. This meant starting a whole new statistical series, not comparable with pre-famine figures.

I am pleased to say that my paper on price statistics (Bowbrick P. , 1988), has been credited with being the trigger that caused most of the world to scrap their price information systems completely, and replace them with systems that produced information that was accurate, meaningful, and useful, that was what was needed to influence real world decisions, and that delivered the information in a usable form to the right people, in the right place and at the right time. The effect on market efficiency has been enormous.

There are also special problems with food price statistics including rice prices, in extreme scarcity situations, problems which vary from country to country, from situation to situation. The figures in contemporary documents in Bengal are largely anecdotal. No details are given of the supposed meaning of the statistics or what the

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<sup>43</sup> This programme was constantly improving methodology for the next three or four decades, with changes being reported and discussed by statisticians and agricultural economists throughout the world. At the same time, it was developing methods of supervising staff – it is much easier to sit at home and invent data, than to go in the field and measure the crop. And it is much more profitable to invent field workers and collect their salaries. Every time they discovered a new problem, the previous crop forecasting system lost more of any credibility it might have had.

function of the market was – for example a local assembly market paying the ‘export parity’ would have changed into a market supplying local consumers at the ‘import parity’. It is often not clear whether the price quoted is the Calcutta corn exchange bulk price, or the price of a cupful of rice in a local market. It is usually not stated whether they are supposed to be retail prices or some sort of wholesale prices (and there are many types and levels of wholesale.) It is not stated which of the 20 or more broad rice grades recognized in Bengal they are supposed to refer to (and what proportion of sales were of that grade), and what sub-grades, whether there were 4% broken or 40%, whether the rice was sold by the ton, by the bag or by the cupful, and whether the rice was sold as paddy (even with a calculated rice equivalent) or milled in different ways, whether it was bagged or loose, and so on. There is no discussion of the collection procedures or reliability. On the contrary, O Grada, for example, quotes prices from Knight (1954) without mentioning Knight’s caveats (p35). Collecting prices in retail markets is far more difficult and expensive than collecting prices in wholesale markets, and is likely to be less accurate. (See, however, Griffiths on a cheap, accurate, system, which is only useful for very specific purposes)

There is no reason that anyone in a famine situation would tell officials the real price – rather the opposite. Actual prices cannot be observed when there is bargaining, which is the norm. Indeed, in Pakistan in some markets, I have observed the buyer and seller negotiate silently: they hold a cloth over their hands and indicate the price with their fingers. So nobody else knows what they have agreed.

It is notoriously the case that news reports and reports by officials tend to quote the very highest or the very lowest prices they have heard mentioned in times of extreme scarcity or glut (economic history textbooks over the years have warned about this). There is clear evidence in the Bengal price series that officials reported the official maximum prices rather than the real prices for some periods. I have observed in the Punjab, price reporting using the 1938 Punjab Markets Act, where the marketmaster decides in the ‘correct price’ for the day for each commodity as the market opens, writes it on a blackboard and reports it to the statistics office and expects everyone to trade at that price. In fact, buyers and sellers negotiate prices on their perception of supply and demand in the normal way. It is not known if the Bengal system was the same, but this is the sort of intervention that was the common in the 1930s generally. Market reporters frequently do not even visit the markets. There is every reason to doubt whether the figures cited are comparable, over time or between locations, let alone accurate.<sup>44</sup> Griffiths (2003) mentions some of the problems with rice prices in a

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<sup>44</sup> Historians are cautious about such anecdotal evidence: ‘If today we were to see “Tracy is a whore” or “Donna sucks you off for a fiver” daubed up at a bar or bus shelter, we would not automatically assume that either of them was actually a prostitute. Nor would we assume that “a fiver” was an accurate reflection of the prices charged for these sexual services in the area.’ (Mary Beard, 2008, p. 232).

crisis in a country similar to 1943 Bengal

Sen's claim that he had used all the available 'production statistics'

A provincial governor in Siberia told me in 1992 that his first action after Perestroika was to order the destruction of all Soviet-era statistics. 'You see', he said, 'We have two kinds of fool in our country. One thinks that any statistics that are printed are correct. The other says, "Yes, it is obvious that these statistics are wrong, but they are the only statistics we have, so we have to use them and pretend that they are right."'

Sen claims repeatedly that he had used all the available 'production statistics':

*'In discussing the causation of the Bengal Famine of 1943, I had used all the available production statistics, without exception, including the 'unadjusted' and 'adjusted' figures of output (and current supply) presented by the official Famine Inquiry Commission.'* (Sen A. , 1987, p. 10)

*'However, in the absence of inventing our data, we cannot do much better than using all the statistics that can be found, which is exactly what I did.'* (Sen A. , 1987, p. 10)

In fact, Sen uses only

a) the official crop **forecasts** that were *useless for any purpose*<sup>45</sup>, *'not merely guesses, but frequently demonstrably absurd guesses'*<sup>46</sup> *'a farce . . . a fraud'*<sup>47</sup> *'blatantly absurd results', 'disbelieved by the very government that produced them'*<sup>48 49</sup> *'no meaningful production statistics'*<sup>50</sup> *'not only incorrect but absurd'*<sup>51</sup> *produced by 'a system inherently vicious'*<sup>52</sup>

b) the same crop forecasts, after they had been quite improperly subjected to yet

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<sup>45</sup> Bowley and Robertson (1934, p. 35)

<sup>46</sup> (Royal Commission on Agriculture in India, 1928, p. 605)

<sup>47</sup> (Dewey C. , 1978) p290

<sup>48</sup> (Mahalanobis, Organisation of Statistics in the Post-War Period, 1943)

<sup>49</sup> (Dewey C. , 1978) p298.

<sup>50</sup> (Bengal Land Revenue Commission, 1940, p. 76)

<sup>51</sup> (F.H. Villiers to Sir Edward Grey, 1914, quoted by Dewey 284.)

<sup>52</sup> (Trevaskis, 1931, p. vol 1 p 200) quoted by Dewey, 1978

another round of 'adjustments' by yet another official (for the Famine Inquiry Commission), and

c) the same crop forecasts when quoted by Blyn (1966), after he had 'adjusted them'<sup>53</sup>. : indeed, Sen says that Blyn produced

*'a similar picture to that given by the government statistics and the Famine Inquiry Commission, and which indeed followed closely the methodology and substantive information used by the Commission.'*

That is to say all the figures used by Sen are based on the one series of crop forecasts.

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Sen does not use any of the other beliefs current at the time, even those presented as statistics:

1. He suppresses the assessment of these crop forecasts by the contemporary Indian statistical profession, even though criticisms were made by world famous statisticians, whose methodologies were adopted around the world subsequently. I had cited Mahalanobis and Desai on this.

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<sup>53</sup> Blyn's book, *Agricultural Trends in India 1891-1947: Output Availability and Production* (1966) is in fact a collection of all the official agricultural statistics for all of India for a 56-year period, produced by an American academic 19 to 75 years after they were produced. Nobody who gave the matter five minutes thought could believe that an American professor, nor anybody else, could have corrected the figures for Bengal, let alone for all India, for this period, somehow miraculously making them virtually 100% correct. Still less could he have shown that all the Indian and Bengali statisticians, who had seen the forecast methodology and its results were wrong in their assessment. I have shown above that the forecasts for 1940/1, 1942/3, and 1943/4 and for later years were produced using different methodologies, and are not comparable.

<sup>54</sup> Sen continued to make such claims for decades. Mukerjee, for example, said '*... Sen based his crop estimates on projections ...*' (Mukerjee, Madhusree, reply by Amartya Sen, 2011) which is undeniably true – they were based on crop forecasts. Sen denies this, saying '*she misdescribes my estimates as being based only "on projections."*' No, she did not say this, but they are, in fact, based only on projections: worse, the projections, the meaningless forecasts, were 'adjusted' – i.e. fabricated - by level after level of officials, for political or other reasons, before publication. They were then 'corrected' – i.e. fabricated – by an official, then fiddled by Sen. But, regardless of how often they were fiddled, and by whom, the statistics are 100% *based on* the forecasts – if they are removed, there is nothing left. That is to say Sen distorts and rubbishes her perfectly correct claim and then suggests that this means she was incompetent. What is more, it was known even before the famine that the figures were completely meaningless and worse than useless, and there was a world famous Indian statistical programme after the famine to remedy this, which Sen does not mention.

Sen repeats this extraordinary claim when discussing Tauger, (Tauger, Mark and reply by Amartya Sen, 2011) Tauger, M., Sen, A. He also makes false claims about Tauger's argument.

2. He suppresses the fact that the only economist on the Famine Inquiry Commission refused to accept their 'availability' estimate and published a dissenting minority report presenting estimates which disagreed with the ones used by Sen.
3. There is no discussion of the Director of Agriculture's informed belief that the forecast produced by his department overstated the crop by 25%. (Mahalanobis (1943, 1944). It is significant that in 1942/43 the Director of Agriculture did not 'adjust' the crop forecast to fit his personal belief, as had been the norm in the past (see the discussion of adjustments above), so this forecast is not comparable with that for 1940/41 – the two years Sen compared.

While nobody would argue that such departmental estimates are accurate and reliable, they were at least based on the professional assessment of agricultural officers and of agricultural researchers who had seen the crop when it was growing, unlike the official crop forecast. Such estimates are always used as a check when a food crisis is threatened. Indeed, I have worked in a lot of countries where these were the only production forecasts or production estimates for most sectors, including grain crops.

4. He mentions, but chooses to ignore, the beliefs of some traders at some points of time. Again, these should have prevented the Bengal Government from dismissing the 1942/43 crop forecasts as false alarms.
5. He suppresses the evidence of the beliefs of many traders over many months, which I had referred to (Bowbrick P. , 1986).
6. He chooses to reject the evidence on the effect of the fungus epiphytotic, on entirely specious grounds, which I discuss in an Appendix 1.
7. He does not mention other statistics, such as the official Settlement statistics<sup>55</sup>:

*'Official rice estimates have actually differed by more than 23% on an average from Settlement figures which are usually believed to be*

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<sup>55</sup> I am reluctant to use these or any other figures for which the methodology is not known, or has not been assessed. It is too easy to make mistakes. For instance, there are mentions in the contemporary literature of a crop-cutting survey of rice before the famine, which one might imagine was more accurate than the eye estimation of the forecast, but it turned out, on examination, that this was an attempt to define a notional 'normal' crop – what it would have been in the absence of wind damage, rain damage, flooding, insect damage and disease. Enumerators did not draw statistical samples, but were told to avoid all damaged areas of a field, and to decide for themselves on which areas of the field were 'most typical' of the undamaged crop. (Department of Agriculture, Bengal, 1922)

*reliable.’ (Mahalanobis 1945 p2)*

8. Elsewhere Sen mentions that other sources exist as Tauger points out

*‘Das’ work was not the only source that posed a challenge to these statistics. Sen cited a document by Bengal Governor Rutherford to criticize what he considered his unwarranted focus on shortages, but on the next page of that document Rutherford presented data on the 1942 harvest that was much lower than the one Sen used, but came directly from contemporary observers and deserved at least to be considered; Sen, p. 82, fn 47; N. Mansergh, ed., The Transfer of Power 1942-7 (London, 1973), 4: 361ff.’ (Tauger 2009 p175)*

9. Tauger (2009) quotes Das (1949), who Sen quoted repeatedly on other aspects of the famine

*‘Das focuses on food shortages and examines harvest data. He describes the method of estimation based on the crop area and the “average” yield per acre, but points out: “The area under rice cultivation and the annual yield from it in Bengal cannot be exactly estimated. Information available from different sources vary [sic] enormously.” As proof he cites four different estimates of the average rice area and yield during the 1930s, prepared by the Famine Inquiry Commission, the Indian Department of Commercial Intelligence and Statistics, the Land Revenue Commission, and those of an Indian scholar in a plan for rehabilitating Bengal after the famine. The areas ranged from 21.6 million acres to 27.4 million acres, and the average yield from 8.4 to 11.5 million tons. He then cites additional sources from 1945-1946 that show similar discrepancies in estimated areas and harvests. In Das’ view, “it is not unjustifiable to say that none of these sources can be fully relied upon.” (2009 p172)*

This means that Sen was perfectly well aware that these other sources of information existed, and he suppressed the fact.

If the Indian Government had given due weight to these other sources of information, they would not have dismissed the 1942/43 crop forecast of a famine as being just another false alarm. (In this discussion we are not concerned with the fact that some of these sources gave consistently higher guesses of total production over the years.)

Thus, Sen’s production statistics were entirely based on just one of the many sources that should have been taken into account, the crop forecast as presented in

the Report of the Famine Inquiry Commission. His other availability statistics come from the same source. It is noted that this source is the report which was not published because Sen *'replied with a strong recommendation that the Report was so flawed as not really to be worth putting out as a book'* (Maxwell, 1987)

Sen's claim that I had only one source of production estimates

Sen adopts the strategy of throwing back criticisms, accusing me of the very errors he has been shown to make – just as, in some cultures, someone caught fiddling the accounts is expected to accuse the auditor of fiddling the accounts. When I showed that his whole argument depended on one single set of dreadful statistics, and on his suppressing the large amount of other evidence, he continues to ignore the evidence and states that my whole argument depended on one miniscule bit of information:

*'Here we come to his second argument. Bowbrick notes, apparently quoting the Famine Inquiry Commission, that 'the trade was talking of the worst crop in 20 years' (p 114) ... And it is this piece of 'propaganda' by trade circles - quoted as such by the Famine Inquiry Commission - that provides Bowbrick's basis for rejecting both the official estimates of food production and the Famine Inquiry Commission's own adjusted figures, used in my empirical analysis. Mr Bowbrick's faith in the gullibility of the reader is evidently boundless.'* (Sen 1986 pp 127-8)

*'In his Rejoinder Mr Bowbrick asserts that he had 'presented other evidence which suggested that the production statistics were not just unreliable but wrong' (p 5). Indeed, Mr Bowbrick did reject the official statistics in favour of some alleged data verbally and vaguely referred to by "the trade". Rejecting the official statistics, Mr Bowbrick had referred to his understanding that "the trade had its own way of estimating supplies"' (1987 p11)*

*'It is indeed hard to put much faith in Mr Bowbrick's rejection of official statistics and of the Famine Inquiry Commission's figures on the basis of one reference to what "the trade was talking of, ..." In my reply to his original article I could not help wondering that 'Mr Bowbrick's faith in the gullibility of the reader is evidently boundless', '- and nothing in this new Rejoinder can prevent that speculation.'" (Sen A. , 1987, p. 11)*

Again, Sen's abuse gives us reason to believe this is a lie. In fact, there are several lies intertwined. The first is that the claim that my rejection of the crop forecasts was based purely on the views of the trade refers to a single sentence in my 18,000-word paper! *Suggestio falsi*. Second, he suppresses the fact that I mentioned

*'Bhatia, quoting from the unpublished evidence to the Famine Commission, states that public men and organizations had warned the government. Sen himself quotes pressure, from 'a businessman much involved in rice trading', to increase imports by one million tons as late as October 1943.'<sup>55</sup> This would of course have been against the businessman's interests if he had large speculative stocks.'* (Bowbrick P. , 1986, p. 114)<sup>56</sup>

*Suppressio veri*. Third, he suppresses the fact that I presented a wide range of evidence, including the opinions of the Indian statistical profession. *Suppressio veri*. Fourth, Sen obscures the fact that the 1942/43 crop forecast predicted a famine, and that at the time there was reason to believe that this was not just another false alarm. *Suppressio veri*. Fifth, he does not mention the view of the Director of Agriculture that the crop was 25% less than the forecast. *Suppressio veri*.

*'It was thus considered likely that the main (winter) rice crop in 1942-43 would fall short of the last season's crop by 22.8% according to [the crop forecast] or by 38.1% in the opinion of the Director of Agriculture himself.'* (Mahalanobis P. , 1944)

Sen's sources also present a lot of evidence to support the belief of the post-famine consensus, that this forecast was over-optimistic, so the famine was worse than forecast. The reported views of the trade were consistent with both of these beliefs. Sen lied, repeatedly.

## MISREPRESENTATION

### Speculation theory

It is instructive to look at one brief paragraph of mine, which destroyed all Sen's claims that speculation was a cause of the Bengal famine, and see how Sen responded

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<sup>56</sup> The Civil Supplies Branch also made use of the wartime postal censorship to find out what grain traders believed and what they were doing about it. (Pinnell, 1944)



– suppressing my criticisms and the refutation in his source, and then producing sneer after sneer, by misrepresenting what I had said, using *suppressio veri, suggestio falsi* and the ‘dead cat’.

I said (1986):

*‘Sen brings up the old bogeyman of speculation as one of the most important causes of the famine. He talks of ‘speculative withdrawal’, especially between December 1942 and March 1943, but also up to November. There was also “vigorous speculation” from March to November.’*

*‘. . . he quotes the Famine Commission in his support. This is a mistake because the Famine Commission described speculation at a different time, a type of speculation that could not have caused the famine.’*

*‘Sen is also at variance with the facts given in his sources on the subject of the timing of the speculation. There was little left on the market for speculators to buy between December and March, and after that there was little indeed available at a very high price - hardly the time for ‘vigorous speculation’*

In his response, Sen did not mention the criticism that he had misrepresented his source. Nor does he challenge my other criticisms. *Suppressio veri*.

I said,

*‘There is an enormous literature on speculation, hoarding and storage dating back at least to Adam Smith. One thing is agreed - that the uninformed layman’s criticisms of speculation are unfounded.’*

Sen replied,

*‘On the substantive point of speculation, Mr Bowbrick points out that ‘uninformed layman’s criticisms of speculation are unfounded’ (p 119), and he refers to Adam Smith. As it happens, Adam Smith and ‘uninformed laymen’ do not exhaust the world.’*

To produce this sneer,

(1) he suppresses the fact that I had made it clear that I was familiar with the enormous literature on speculation, hoarding and storage, and had only mentioned Adam Smith in passing – *suppressio veri*.

(2) he says that I claimed that Adam Smith and 'uninformed laymen' 'exhausted the world', when I had mentioned the enormous literature on the subject – *suggestio falsi*.

(3) he suggests that I cited Adam Smith as an example of modern theory, when I had cited it only to show that the myth had been powerful two and a half centuries ago, – *suggestio falsi*.

I said,

*'Yet Sen does not provide a model to show why the uninformed layman should be correct in this instance.'*

Sen makes no mention of this criticism in his responses. Nor does he produce any model, let alone one that would explain how one or two million tons of rice vanished into thin air. (I set out some of the factors that such a model would have to cover in Appendix 2). *Suppressio veri*.

I said,

*'Speculation would only have caused the famine if it reduced the total supply on the market. Neither the Famine Commission nor anyone else has suggested that speculators reduced total supply either by exporting or holding stocks until the next season (which would have lost individual traders a lot of money). On the contrary, the traders imported (legally or illegally) all the grain they could buy. Furthermore, house-to-house searches for grain in mid-1943 showed that there were no enormous stocks. The failure of speculators to respond to the government's market intervention also suggests that there were no large stocks.'*

Sen makes no mention of the Famine Inquiry Commission refutation in his many publications or in his responses to my papers. *Suppressio veri*.

The Famine Inquiry Commission examined the speculation hypothesis rigorously in accordance with scientific method and with an enormous amount of evidence – much of it of a nature only obtainable in wartime.<sup>57</sup>

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<sup>57</sup> Braund (1944) is a bit clearer that he was following formal scientific method by setting out the explanation of the famine that were current in 1943 as 'causal hypotheses' which he proceeded to try

The refutation is unchallengeable, and Sen does not attempt to challenge it.

I said,

*'There is an enormous literature on speculation, hoarding and storage'*

Sen replied,

*'If Mr Bowbrick wishes to find out how speculation can be destabilizing, he should perhaps look at some recent economic literature on the subject. . . I would recommend beginning with Oliver Hart, On the profitability of speculation', Quarterly Journal of Economics, Vol 91, 1977.'*

This is a smear, pure fabrication, suggesting, without any possible evidence, that I knew nothing at all about destabilizing speculation, which I had first come across as a student quarter of a century earlier. *Suggestio Falsi*.

It is also a 'dead cat'. It is designed to get the reader thinking about the sneer, rather than Sen's complete lack of theory or evidence. The paper is totally irrelevant. No paper could conceivably rescue any of Sen's theory, unless it took into account the realities of the 1943 Bengal famine. In Appendix 2, I set out some of the factors that would have to be covered in such a paper, factors that are not mentioned in low-level academic work.

And of course, nothing in Sen's work makes any use of this or any other speculation theory.

### [A cyclone-based explanation of the Bengal Famine](#)

Sen claims

*'Mr Bowbrick favours a cyclone-based explanation of the Bengal famine. His claim that this produced a great reduction of output, which 'the trade estimated to be the worst crop in 20 years', was dealt with earlier in this reply and shown to be false.'* (Sen, 1986 p131)

This is a lie. I did not have '*a cyclone-based explanation*'. Nor did the people predicting the worst crop in 20 years. Like everyone else writing on the famine, I was careful to set out the main factors which reduced the crop, not just the cyclone, as Sen states. The Director of Agriculture thought that the drought damage reduced the crop

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and refute. Which he, and the Famine Inquiry Commission did, admirably.

by one million tons. The fungus outbreak, which was not caused by the cyclone, was considered to have had a greater effect than the cyclone,

*'Nothing as devastating as the Bengal epiphytotic of 1942 has been recorded in plant pathological literature. The only other instance that bears comparison in loss sustained by a food crop and the human calamity that followed in its wake is the Irish potato famine of 1845.'*  
(Padmanabhan S. , The Great Bengal Famine, 1973)

Thus, Sen pretends that I ignored two thirds of the crop damage. It is a lie.

Another lie was Sen's claim *'the trade estimated to be the worst crop in 20 years', was dealt with earlier in this reply and shown to be false.'* (Sen, 1986 p131). He did not even attempt to challenge this prediction 'earlier in this paper' let alone show it to be false: he just asserted that it was wrong, without evidence, but with abuse, saying that the trade were presenting false information to make higher profits – a claim impossible to substantiate. In fact, it was not in the financial interest of traders to claim that there would be a famine when there would not be – on the contrary, they would have made a substantial profit by hiding the fact, buying cheaply immediately and selling at famine prices. And, yes, I have known rice traders who were more interested in preventing a famine than in making money.

### Supply at a point of time

I showed that Sen's analysis depended entirely on just two figures, his claims on the 'total food availability' for the calendar years 1941 and 1943. My view, as an agricultural marketing economist, was similar to that of the post-famine consensus, that both the perceptions of current availability and the perceptions of future availability were constantly changing from the beginning of 1942 to the end of 1943, and that these changes affected the progress of the famine, and government policies to deal with it. The actions of the Government of India and provincial governments, and the constant changes of regulations changed expectations and actual supplies. Again, I discussed this at considerable length. I emphasized the importance of constant monitoring of the supply situation.<sup>58</sup> Sen's response is to accuse me of making just the

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<sup>58</sup> One would expect any agricultural marketing economist to know that the wholesale prices of perishables like strawberries and lettuce frequently double or halve over a day's trading: the day starts with traders taking a view of supply and demand, and setting the price accordingly. If the perception is incorrect by even 10% and the price remains unchanged, there inevitably comes a time when supply is half or double the amount that people will buy at the price. In normal times grain traders can correct by price changes, import, export, and changes in stocks. In a famine, though, the aim is to end with near

mistake he made:

*'Bowbrick's discussion of famines in general and of the Bengal famine in particular is rendered rather chaotic by his persistent confusion between 1) availability decline over time, and 2) supply inadequacy and shortage at a point in time.'* (1986 p126)

There are no references to anywhere I might have done this, no quotation, no analysis – and it would take pages to set out what I am supposed to have done – there is just a bald, and repeated, assertion. But then nothing he has written shows any understanding of agricultural economics, market economics, economic theory, or what economists do.

#### Food distribution schemes

I had shown that some employers in Calcutta had started food distribution schemes rather than paying higher wages (so employees would not have been able to double their food consumption with their higher wages) but that these schemes covered only a quarter of the population (See Famine Inquiry Commission pp31, 32, 63). I said that

*'It is not true that [as Sen claimed], 'almost the entire population of Calcutta was covered'. The preferential schemes never covered more than a quarter of the population and they were often cut because of shortage of food. Preferential supply schemes, plus the controlled and approved markets, received 32% of the grain available in Calcutta in the first quarter, 43% in the second, 23% in the third and 18% in the fourth. (Bowbrick 1986 p117)*

Sen responded by claiming

*'This is based on three factual errors.*

*'First, rather than concentrating only on the food distribution scheme devised by the Bengal Chamber of Commerce, if account is also taken of the other 'foodstuff schemes' ... we get from the Famine Inquiry Commission the total figure of 1.09 million employees. With their dependents, these 1.09 million could have scarcely been no more*

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zero stocks as the new crop becomes available, and there are constant changes in beliefs about when or if imports will arrive. So constant monitoring is needed.

*than "a quarter of the population of greater Calcutta' (even if we take the high figure of six million as the total number of residents of Calcutta)."* (Sen 1986 p31)

Needless to say, he was wrong and I was right. If we take the Famine Inquiry Commission's figures for dependents (p31) and apply it to these figures, we do indeed get 25-30% of the population.

This cannot be dismissed as sloppy research by Sen. He was perfectly aware that I had taken dependents into account. Indeed, in my 1986 paper I said, *'In his reply to my Development Studies Association paper, Sen said that this sixfold discrepancy was due to the fact that I had ignored dependents. I did in fact allow for them - see for example, Famine Commission, op cit, Ref 2, pp 30, 31, 63 and my calculations presented in Table 2. Even if I had neglected them, a substantial discrepancy would remain.'*

Sen then accuses me of another 'factual error' of ignoring the 'controlled and approved markets', though I had explicitly included them in the supply figures, in the text and a table. *Suggestio falsi*.

In fact, as his sources made clear, these markets were most definitely not there to supply rich people wanting to eat twice as much as usual, but were rather 'rationing by queueing',

*'Anybody could purchase from a controlled shop if he was prepared to stand in a queue and wait, perhaps for hours. Great difficulty was experienced in supervising these shops and controlling the queues, which grew longer and longer,' (Famine Inquiry Commission, 1945, p. 59)*

Das (1949), who Sen quotes repeatedly when it suits him, gives a vivid description of the failures of these distribution systems. Again, I did not make a factual error: Sen misrepresented me and the facts. *Suggestio falsi*. Again, even if I had been wrong, it would not have produced a million people doubling their food consumption and paying four times as much for the food, as Sen's claims imply.

The third factual error Sen claims is

*'Third, the greater demand of Calcutta came not just from the price subsidies, but also from the expansion of purchasing power due to the war boom'*

Yes, indeed, this is a factual error, but it is Sen who made this factual error. His assertions are not supported by facts. He makes no effort to show that anybody in Calcutta ate even twice as much in 1943 as in 1942 or 1944. Again, he suppresses the fact, which I mentioned, that Calcutta's consumption actually fell during 1943. Again, he suppresses the fact that there was a desperate

struggle from December 1942 to keep Calcutta supplied, and that rations frequently had to be cut and that stocks were at dangerously low levels,

*‘From January to May 1943 the total stocks of rice held by the Chamber and the employers’ organizations connected with the Chamber exceeded two weeks’ supply only once and that was in the month of March. During the remainder of the year, the stocks were at a higher level but they never exceeded more than six weeks’ supply.’ (Famine Inquiry Commission, 1945a, p. 64)*

Pinnell (1944) and Braund (1944) say that supplies were often down to two- or three-days’ consumption and once went below one day’s consumption. If any large group had been eating the quantities Sen claims, there would have been a million deaths from starvation in Calcutta, when, in fact, the small number of deaths were nearly all destitutes fleeing the country areas.

### The merit of food imports

My refutation is about what Sen said – his bad theory, bad statistics and misstated evidence, so Sen deflects the discussion to what he claims are **my** beliefs, which are irrelevant. Another ‘dead cat’. He sneers:

*‘But I suppose, it does reflect Bowbrick’s own belief that if a famine is not caused by what he calls a ‘shortage’, then there is no need for food imports.’ (1986 p 126).*

*‘Bowbrick would not accept the merit of food imports, simply because the excess demand happened not to have resulted from a decline in total supply.’ (1986 p126)*

The idea that a famine might be caused by changes in demand is very old. It was always considered interesting because it raised the possibility that a famine could be dealt with by reversing these changes, when it was difficult, impossible or expensive to import. It was seen to be obvious in 1942-1943 that if (and only if) the cause of the famine was a change in demand brought about by speculation, hoarding, an urban boom, or inflation – the popular myths prevalent at the time, later adopted by Sen – the famine could be stopped in its tracks by rationing and control of stocks. This was seen to be practicable since most of the world was using rationing and control of stocks as part of food policy in 1943. The policy was extraordinarily attractive to policy

makers, as it saved them trouble, and it enabled the Government of India, and the provincial governments to say, 'Nothing to do with us: it is the Bengal Government's problem.'

The immediate attraction of Sen's claim that most famines were caused by changes in demand rather than changes in supply was that it enabled decision makers around the world to say, 'Not our problem.'

In fact, the main message of my paper was that such a policy was incredibly dangerous. I started by setting out the mainstream view that market intervention (i.e. price control, control of stocks, seizure of stocks, prohibition of exports, etc) plus imports would be normal (1986 p108). These policies might only give the government time to arrange imports, or to limit the number of deaths. In 1943 most countries adopted some such methods.

*'In addition to this, rationing is taken to be a possible complete solution for a first or a second degree shortage, although the practical problems are such that it would be unsafe to rely on it. There are problems arising from poor administration, corruption, political interference or difficulties with physical distribution.'* (1986 p108)

And of course this would only return one to the previous situation which is unlikely to be optimal: one may argue that Bengal, like most countries in most of history, had a Third Degree Shortage in normal times, with high infant mortality, low life expectancy and three quarters of the population suffering from malnutrition.

*The mainstream view is that, ideally, at least enough food should be imported to meet the shortfall in supply. However, since Sen does not recognize that there is a shortage, he does not accept that any food has to be imported at all* (1986 p 108)

That is to say, his argument was exactly what he says were my beliefs.

I produced a lot of evidence to show that the crop forecasts, etc. were so bad, that one could never have any confidence whatsoever in a claim that there was No Shortage, or a First-Degree Shortage. Refusing imports on the base of a guess in a famine situation would be criminal. It could kill millions, as it did in Bengal in 1943. Which is, again, stating '*the merit of food imports*'.

I argued strongly and at length, with evidence from Sen's sources, that the Bengal Famine was allowed to occur because decision makers like politicians and administrators, in Bengal, in surplus provinces, in the Government of India and in Britain, chose to believe that Bengal had enough food and that imports were not necessary (Sen's view). Evidently this myth is particularly captivating. Refusing to import on the basis of a myth can kill millions. Which is, again, stating 'the merit of



food imports’.

I pointed out that, while diagnosing a food shortage famine as a demand change famine could be catastrophic, diagnosing a demand change famine as a food shortage famine would not be. The imports would ensure that people would be fed. Which is, again, stating *‘the merit of food imports’*.

My conclusion was as far from Sen’s claim as possible:

*‘Making the opposite mistake and diagnosing a Sen-type famine as a food availability decline (FAD) famine does not matter: the action taken will rapidly bring the famine to an end.’ (1986 p107)*

*‘The only way to be sure of curing a famine, however caused, is to import more food. Any analysis is dangerous if, like Sen’s, it underestimates the degree of shortage - it will lead to inadequate relief measures and to a worsening of the famine.’ (1986 pp123-4)*

People who have actually read my papers have understood the arguments,

*‘Bowbrick (2008, 63) also argues that importing food in a situation of existing insecurity is the only way to minimise the food problem and to “save lives”.’ (Engler, Köster, & Siebert, 2014, p. 6/27)*

So Sen lies, yet again.

Modern readers may not have realized that when I said *‘It is very odd, therefore, that Sen’* made this claim (1987 p 8), I was using the British academicspeak of the time, which used the full British understatement. I am not sure of the modern equivalent, perhaps *‘a pack of lies: work out for yourself why he said it’*.

I would emphasize that imports are not the easy option that Sen thinks. There is a vast amount of evidence that it is often difficult or impossible to get imports at all for many reasons – because the country cannot afford to buy them, because donors may not produce the food or the money, because of war, because of transport constraints and because they often arrive after the famine is over, just in time to flood the market as the new crop is harvested. Bengal had many such problems in 1942-43.

So Sen’s assertions are not just a dead cat, aiming to move the discussion away from his many errors and fabrications, they are repeated lies.

### Misrepresenting Sen’s views?

Sen repeatedly claims that I misrepresented his views.

*‘Mr Bowbrick’s alleged refutation of my analysis of famines consists*

*largely of; confusing different concepts, misstating empirical evidence and misdescribing my views. He makes profuse use of all of these bemusing genres.'*

His abuse sets off one set of alarm bells, his failure to produce a reply meeting academic practice, references to support his claims, for instance, sets off another set. Again, this is the 'rant' which is just throwing back evidenced criticisms with abuse.

He does not, of course use the standard academic practice, setting out, with *extensive, referenced*, quotations – not just a couple of words – where I might have done this, and with extensive, referenced, quotations of the concepts he claims I confuse. Still less does he provide a hard economic analysis of the concepts and how he claims I confuse them: this would require a far greater knowledge of market economics, marketing economics and agricultural economics than he has shown any sign of having. I note, for example, his inability to understand the concept of defining a word and sticking to the same definition throughout an analysis.

I have set out at great length, in some detail, and with full references, some thirty or forty of most important of his misrepresentations of key facts (Bowbrick P. , 1986; 1987; 1988, 2008). All meticulously referenced. All repeatedly checked and cross-checked by friends and colleagues. I set out many more in this paper. Neither Sen nor anyone else has attempted to challenge me, though this would be a matter of straightforward scholarship, if and only if I was wrong. Instead he throws back the accusation, with unevidenced assertion, but no evidence whatsoever.

His claim that I misdescribed his 'views' is absurd. I was not, and am not, interested in his 'views' or 'beliefs': I was presenting a refutation of a single, scientifically-testable, economic model, his theory of the causation of the 1943 Bengal famine – a very different thing. Accordingly, I was very careful to say in my refutation that I was confining myself to what Sen said and to the sources he cited, as my priority was to show that he systematically misrepresented his evidence, and I cited the examples meticulously. I was careful not to speculate on what Sen 'really' thought, or on his views, or his concepts or to comment on them: they are irrelevant.<sup>59</sup> I am not interested in his 'entitlement approach', a non-scientific belief system. His statements on his views, on facts and on statistics change throughout his work. Many of his responses refer to what he said about other famines, where the facts were different. This means that if I say Sen said 'X', it is no surprise if anyone comes up with evidence that he said 'Y' in another publication, or, indeed, in the same paper. His practice of changing the meaning of a word throughout a paper, or even in a single sentence,

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<sup>59</sup> There is now a substantial literature presenting different interpretations of what he 'really thought', and what his terms meant at different stages of his argument: his presentation is garbled. But this is irrelevant to the facts of this particular famine.

means that neither he nor anybody else knows his views or concepts – I have mentioned his use of ‘shortage’ in many different senses, none of them supported by a definition.

He ends with pure abuse.

*‘I suppose the distortion and misrepresentation in all this are just standard parts of “Bowbrickia”’, (Sen A. , 1986, p. 5)*

*‘In my reply to his original article I could not help wondering that ‘Mr Bowbrick’s faith in the gullibility of the reader is evidently boundless”,’ - and nothing in this new Rejoinder can prevent that speculation.”’ (Sen A. , 1987, p. 11)*

Which, as my kindergarten teacher pointed out, is an admission that he has no defence.

## LACK OF CRITICISM BY READERS

It might be expected that anyone with even a basic economics degree, anyone trained in critical thinking about the real world, should have understood the weaknesses in Sen’s work and Sen’s responses when these were pointed out to them. They have been taught how difficult it is to find meaningful data and produce meaningful statistics. They have been taught how to build economic models about real world situations. My paper, and others, showed very clearly that Sen consistently misstated the facts and that his economics was abysmal, with the errors always biased to supporting his ideas. The least one could have expected was that readers examined his responses critically, and that they also compared my statements and Sen’s with Sen’s sources – this critical thinking is, after all, fundamental to a professional or academic training. It is easy to check his claims against my refutation, and against his sources.

Again, no economist gets a first degree without picking up something about scientific method, even if they do not do the required reading. If nothing else, they must surely be taught the epistemology of statistics. How can they not use it?

Instead, it appears that quite a few economists chose to believe any assertions that supported their pet theory or belief system. They ignored their economic training, and they adopted the decision modes they use for deciding which politician, or which football club to support.

## PART 3: THE RESPONSE OF THE RESEARCH PROGRAMME

**'When the facts change, I change my mind. What do you do, sir?'**

**John Maynard Keynes**

Scientific method in economics, as elsewhere, starts with the supposition that people are more likely to notice, and to accept, evidence that supports what they want to believe. They are less likely to observe, take notice of, or accept evidence that challenges their 'beliefs' (i.e. confirmation bias). I have put 'belief' in scare quotes because it can be interpreted in many ways. At one level is my belief that a given supermarket is making quarter of its profit from sales of fruit and vegetables: this is a testable hypothesis which can be supported or refuted by an examination of the accounts: I will refer to such beliefs as a testable model. Then there are generalizations intended to produce 'initial hypotheses' which someone may use to produce a testable model. For example, one may use one's experience of how one type of market often behaves as an 'initial hypothesis' of what is happening in a market one has not worked on before: one may quickly move to a usable model by adjusting the assumptions to the realities of the situation. Good economic theory performs a similar function. More general beliefs on the nature of free markets and the desirability of government intervention in markets, for instance, are sometimes referred to as 'belief systems' or 'religions', or 'theology' as they are untestable. They are likely to be influenced by a range of beliefs which may not be obviously linked to the matter in hand, including religious and political beliefs. They are more likely to selectively accept data and economic analysis that supports their beliefs.

Accordingly, scientific method requires that we constantly challenge our beliefs and our conclusions. One way is to treat our beliefs as hypotheses to be challenged, and that we challenge them ourselves and encourage others to challenge them. One refutation is worth a thousand papers: it can stop thousands of economists working with bad theory for instance.

I show below how Sen's supporters have dealt with my criticisms, and to some extent, other people's criticisms

## SUPPRESSION OF CRITICISMS

Suppression of criticisms can be achieved, first, by preventing publication of criticisms, and, second, by not mentioning any criticisms that are published.

Referees and editors can and do prevent people from publishing ideas or facts that they do not like. They do not have to state their reasons: it is enough to say, 'some interesting ideas, but not publishable', 'nothing new', 'weak arguments', 'not familiar with the literature', or 'does not understand the concepts'. I have been shown one referee's report saying that the paper should be rejected unless major criticisms of Sen were removed. We cannot know how many papers refuting Sen's work have been suppressed in this way.

These referees used their position to support fraudulent research, and to suppress a refutation. The referees are accessories. This brings to mind those who put millions of lives at risk by suppressing the scientific evidence on Covid 19 and those who did not act firmly, decisively, and immediately when they realized that Wakefield's research on MMR vaccine (1998) was untenable, again putting millions of lives at risk.

Most of the people who use Sen's work do not mention criticisms of it. A very effective way to preserve a belief system is simply not to mention any evidence, theory or criticism that challenges this belief. There are more flat- earthers in the world today than ever before. They publish to believers and to people looking for something to believe, not mentioning critics or inconvenient evidence. Many constantly quote things that support everyday experience: 'I mean it's obvious: you just have to look!' If they mention the unbelievers, they distort the criticisms and ridicule them: 'The Australians, at the bottom of the earth, walking on their hands!'

Similarly, climate-change deniers hide the existence of the science that 97% of the world's scientists believe. Holocaust deniers do much the same.

The anti-vaccination movements of the 19<sup>th</sup> and 20<sup>th</sup> centuries largely disappeared as one of the major killers, smallpox, was eradicated, as polio and yellow fever were largely wiped out, and as other diseases like TB (The white plague!), typhoid and paratyphoid largely disappeared, in the rich countries at least. Those of us working in areas that had been death traps in our fathers' and grandfathers' time, took our armfuls of vaccinations and survived. Then someone presented fraudulent evidence that, in one small experiment, people given the MMR vaccine were more likely to become autistic. (Wakefield, 1998) The paper was retracted, and the author was eventually struck off the medical register. Nevertheless, it helped spark off an anti-vaccination movement around the world. This works, first, by propagating this discredited paper to potential supporters; second, by suppressing the criticisms; third by failing to report counter evidence; and, fourth, by attacking the motives of people making criticisms. The movement manages to ignore the revival of big killers like

measles following the reduction of vaccinations. Anti-vaxx beliefs appeal to different groups: some people were already supporters of alternative medicine; some mistrusted big pharma; some thought vaccines were part of a conspiracy by certain governments to sterilize people of their race or religion; some found that their religions had forbidden vaccination thousands of years before it was invented.

Astrologers use 'confirmation bias' to support their belief. They, like economists, publicize any successes, but can give plenty of excuses when their predictions are wrong. Some people who ask anyone they meet 'Are you Aries or Libra?' and can find some of the 'common attributes' for their star sign in their behaviour – rather as some people find something in any famine that fits their beliefs.

A tiny proportion of the people who cite Sen, also cite his critics. Most suppress all criticisms. Rather as the flat-earthers or anti-vaxx people handle criticisms. Some of the reasons they suppress criticisms are:

- Because they are not aware of them. They may have got their beliefs from an undergraduate lecture, or the brief summary that was on the lecturer's reading list.
- Because they do not find them mentioned in the literature.
- Because they read sneers in the literature and think that the critics are refuted. Or have heard sneers by their lecturers.
- Because they do not want to believe anything that refutes what they had thought was good theory – it makes them appear stupid.
- Because they have produced theses, papers or books using Sen's theory and their reputation and career would suffer if it became known just how bad Sen's theory was.
- Because they are emotionally attached to the belief system it supports – Sen's 'entitlement approach' may be the first thing that they ever read on why people go hungry, which is hard to believe, but many students had a sheltered upbringing.
- Because they have a passionate belief in some other view.
- Because of their political or religious beliefs.
- Because of the pleasures of being contrary.
- Because their employer, perhaps a university department, finds them inconvenient.

### Types of suppression

It is convenient to distinguish between different types of suppression of criticism that will be discussed later.

- Referees or editors suppress criticism.

- People cite Sen but do not mention his critics or set out their criticisms.
- People cite Sen mentioning some critics, but not their criticisms.
- People cite Sen, mentioning some critics but with brief notes misrepresenting their criticisms.
- People cite Sen, misrepresenting his critics comprehensively, with serious misrepresentations of the facts and the theory.

## WHAT ARE THE PERCEIVED CRITICISMS?

Those people who have cited my papers have very different impressions of the status and implications of my criticisms. There are, in fact, eight distinct implications of my formal refutations of one piece of empirical research, Sen's theory of the 1943 Bengal famine.

1. I explicitly, and repeatedly, said that I was not appraising Sen's entitlement approach (Bowbrick P. , 1986, pp. 105-6, 123). The credibility of anyone who criticizes me for 'not understanding Sen's entitlement approach' is, therefore, suspect.
2. I explicitly, and repeatedly, said that I was not denying that demand shifts sometimes cause famines, and that there would always be supply and demand shifts in the course of a famine. The credibility of anyone who asserts that I believe that all famines are caused by falls in food supply is suspect.
3. I showed that the combination of fraudulent evidence, fabricated or abysmal statistics and abysmal economic analysis or, more often, no economic analysis, just assertion, meant that Sen's theory of the 1943 Bengal famine was wrong in all respects.
4. This implies that Sen's generalizations on famines have no empirical basis, not even what little could be obtained from comparing four famines. This is so, even ignoring the criticisms that others have made of his analysis of the other famines, and his comparisons. They are refuted.
5. This implies that it would be irrational and unethical for anyone to read, let alone believe, anything else Sen had written, including his 'entitlement approach'. Writers on scientific method note that non-testable belief systems vanish because they become unfashionable, or politically unacceptable. One might expect a belief system to become unfashionable if it was shown that it was created by such a person.
6. The weaknesses of Sen's work would be obvious to any specialist in the area, and any economist should understand them immediately they are

pointed out. The implication is that economists who refuse to acknowledge these weaknesses have no credibility. Any such belief system might become unfashionable if it were known that it was supported by and propagated by such people.

7. My refutation also performed the functions of a normal economic analysis of a market: it helped identify why the market had not performed as it was expected to, and what might be done in future to make it work better. When we are analysing markets, we are also expected to alert our colleagues to new or unexpectedly serious problems. Like my colleagues, I read such analyses for a range of reasons, to see how different market structures work, to find new ways of operating a market or factors I may have missed, to find new theory or ways of analysing markets, to find difficulties encountered in the analysis and in their statistics, and so on. In this case, for example, I pointed out the dangers of basing policy on myths; the need for policymakers to keep monitoring the changes in supply and in perceived supply throughout a food crisis, regardless of one's belief in how the famine was caused; and the need to bear in mind 'garbage in, garbage out' – none of these would be considered controversial by professionals, but they evidently need constant re-emphasis. I have read thousands of such economic analyses, and none of them were, or were intended to be, tests of, or criticisms of 'the entitlement approach': they were serious economics.
8. Even if one of my refutations could be shown to be wrong – and nobody has attempted to show that any of them were wrong – my other refutations and those of other researchers would remain.

Many of the people citing me think that empirical economic research cannot actually or potentially refute 'the entitlement approach': the approach is formally untestable. They explicitly choose to ignore all empirical criticisms of Sen on these grounds (they do not state that they also ignore all Sen's empirical work, as is the logical corollary). They have an implicit belief that Sen's 'famine generalizations' are part of 'the entitlement approach' and so are untestable. They silently suppress all discussion of the refutations on Sen's 'famine generalizations'. They suppress the implications of the refutations on the credibility of any belief system created by such a person. I discuss some examples below: Osmani's is a particularly good example.

Other followers of Sen clearly have a wider view, though they do not spell it out. They appear to believe some or all of the following propositions. Three parts of Sen's work are separate, but closely linked, even overlapping: his economic theory of individual famines, his generalizations from four such studies, and 'the entitlement



approach'. The first two are testable, and so 'scientific', but the third is not. Some would appear to think that 'the entitlement approach' is testable, but do not say how, or give any indication that they think it has been tested. They do, therefore, at least mention empirical work. Again, however, they silently suppress all discussion of the refutations on Sen's 'famine generalizations'. They suppress the implications of the refutations on the credibility of any other empirical work done by such a person or of any belief system they created. I discuss some examples below.

### Mentioning critics without their reasons

Many people cite critics but make no attempt to explain to the reader what the criticisms are. It would be easy to explain to readers what my criticisms are: the abstract of my 1986 paper is brief and clear, and the abstract of my 1987 rejoinder is blunt and is impossible to misunderstand. When I wrote my papers, the citation ethic was that authors owed it to their readers to alert their readers to relevant literature.

*'The empirical details of Sen's account have, however, been subject to criticism.'* (Bowbrick 1986). (Williams, 1999)

*'Hence even if Sen's own recommendations for economic policy against famine were wrong (as argued for instance by Bowbrick, 1986; cf. Sen, 1986) . . .'* (Agne, 2010)

*'Famine theorizing after Sen has contested many aspects of Sen's writings. In fact, the recognition and admiration his work has received across the wider social sciences is only rivalled by the criticism, opposition and, at times, fury it has encountered as well (see, for example, Rangasami 1985; Bowbrick 1986; de Waal 1989, 1997; Keen 1994a, 1994b). It would be far beyond the scope of this article to survey and critically engage with the detailed controversies.'*

(Plümper & Neumayer, 2009)

*The factual details of Sen's analysis have been contested on empirical grounds (see Bowbrick 1986), but this quarrel does not figure in the ethical analysis that follows in this paper. (Thompson, 2015, p. 22)*

An extraordinary statement in a paper on ethics published in a journal of ethics!

*'This theoretical framework has been widely discussed and debated. For a critique of this approach, see Rangasami 1985; de Waal 1989;*

*Bowbrick 1986; Nolan 1993; Desai 1987 and Bhatia 1993'. (Mishra, 2000, p. 81)*

*'Bowbrick (1986) goes further than Benicourt and emphatically denounces Sen's well-known theory of famines as both "factually flawed" and theoretically unsound.' (Duhs, 2008, p. 183)*

## The FAD approach

I have been accused of adopting an 'FAD approach', though I repeatedly and clearly said that I did not, and that, like all economists I have met or read. In fact, I was acutely aware of the demand side (1986 pp105, 106, 124: 1987p 8)<sup>60</sup>. 500 words in two

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<sup>60</sup> *'It is not presented as a supply-side analysis in contrast to Sen's demand-side analysis. On the contrary, it was the weaknesses and contradictions in his demand analysis that showed that his supply figures could not be accurate.'*1986 p105

*'Famines may be caused by: a fall in food supply, a rise in the demand for food (e.g. because of an influx of refugees), a redistribution of what food is available (e.g. with hyperinflation or unemployment meaning that some people cannot afford to buy food), or shortages within a season, caused by transport problems, etc. It is difficult for a casual observer to isolate the cause of a particular famine, because a famine caused by crop failure, for example, will necessarily be accompanied by speculation, a redistribution of income, reduced purchasing power by some groups and by regional shortages.*

*'Adam Smith thought that, in practice, all famines were caused by a sudden fall in food supply.' Likewise, modern economists have thought that such famines were the norm, but that they can be caused, on occasion, by other factors.'* 1986 p 106

*'His belief that any economist working on famines could ignore demand is odd. Mainstream economists have been obsessive about the demand side of famines for centuries and have certainly not forgotten it in the last 50 years. Since 1939, the UK has been carrying out an annual National Food Survey which was started precisely to find out how the poor were eating during the war, and which continues to give information on the nutrition of different socioeconomic groups. Commercial market research is no less interested in the ability of different socioeconomic groups to buy products. The existence of working-class cost of living indices in India in 1943 (which Sen quotes) shows that the matter was on economists' minds then. It is impossible that anyone in a country with a shortage would not be aware of the problem. I am now working in a country with a food problem and every day colleagues, friends or casual acquaintances give me detailed budgets to show that they cannot afford to buy rice. The message is hammered home by the near riot in the queue for relief rice outside my office'. 1987 p8*

very tightly written papers.

In a long career, working with professionals in 38 countries, and keeping abreast of agricultural economics literature from around the world, I have never encountered any sign of anyone having an FAD approach, in any of Sen's definitions of it. Three-quarters of the 38 countries I have worked in were undergoing significant or major changes in demand, and the ability of various groups to obtain food, or were still adapting to recent changes. Analysing these changes and responding to them is a fundamental part of food and agricultural policy (see also my comments in the section on The Merits of Food Imports p 60). It must also be clear that nearly every country in the world in 1943 was well aware of the role of food distribution patterns in a food crisis.

I believe that the causes of a given famine are a matter of fact, not dogma: what are the facts in one particular case. To fabricate one's evidence to fit the dogma one holds is unacceptable.<sup>61</sup>

*'Thus, these facts are regarded as central to the FAD approach.'*  
(Bowbrick, 1986, 1987). (Daoud, 2018)

I will show below some fuller and more serious misrepresentations on this.

## I WAS NOT REFUTING ENTITLEMENT

I made it quite clear that I was not attempting to refute the 'entitlement

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*'The appropriate method of examining a famine has nothing to do with the opposing dogmas of the FAD approach (if such an approach ever existed) and Sen's entitlement theory. The approach normally used in examining price policy and marketing is rigorous and has an enormous explanatory power. A complex model is built up to take into account all institutional factors and other factors relevant to the market. Such a model has the advantage that factual inaccuracies are immediately revealed as inconsistencies. It also has the advantage of taking into account the agrarian problems, the price policies and the marketing systems that are all too often the underlying cause of the famine, and that strongly influence the course of the famine.'* (Bowbrick P. , 1986, p. 124)

<sup>61</sup> Rubin (2016, p. 21), would prefer to consider this as an argument between commentators who all believe in supply and demand factors, but who disagree on their relative importance in general, or which causes famine more often. This is not the argument I have observed, which has more in common with a religious sect denouncing heresy. Rubin's suggestion that it would be acceptable to have the argument he describes is extraordinary. When it comes to the crunch, all that matters is the truth about the one situation being examined.

approach’ – you cannot test, prove, support or refute non-scientific or belief systems: they are abandoned if they become unfashionable. I was refuting Sen’s theory of the cause of the 1943 Bengal famine.

*‘The language of normal economic theory will be used, rather than that of Sen's entitlement theory. There are several reasons for this. First, Sen himself used this language when dealing with the Bengal famine, with his occasional mentions of entitlement declines, etc, being external to his analysis. Second, we are concerned with what actually happened, rather than with the labels put on the effects. Third, the use of the value-loaded vocabulary of entitlement would confuse people who are not familiar with it, or who do not agree with it. Finally, discussions have made it clear that different people interpret his entitlement theory in quite different ways.’<sup>62</sup> (Bowbrick P. , The Causes of Famine - A refutation of Professor Sen's theory, 1986, pp. 105-6)*

*‘It has not been the aim of this paper to appraise Sen's entitlement theory. It should be noted, however, that even in the hands of its originator it is apparently incapable of detecting the many contradictions in the model presented or the many factual errors. Modern marketing theory, on the other hand, exposes the errors and contradictions immediately.’ (Bowbrick P. , 1986, p. 123)*

*‘The appropriate method of examining a famine has nothing to do with the opposing dogmas of the FAD approach (if such an approach ever existed) and Sen's entitlement theory.’ 1986 p124*

It is surprising, therefore, that, as I show below, some people have chosen to state that

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<sup>62</sup> In fact, there is now a large and growing literature arguing about what Sen actually meant, since he used his terms in so many different ways, without defining them, and he made a lot of contradictory statements in his many writings and indeed in single publications. I pointed out the lack of consensus on what Sen’s entitlement ‘theory’ really was, with Sen himself being contradictory on it – which made it impossible to attempt to refute, as any criticisms could be dismissed by saying, ‘he does not understand what Sen really meant’, or quoting what Sen said elsewhere.

*‘It is common for Sen and his defenders to dismiss critics of the entitlement approach as “misreading”, “misinterpreting”, or even “misrepresenting” Sen’s intentions. But this begs the obvious question: How could so many academics have misunderstood what Sen was trying to say in Poverty and Famines—which is, after all, a brief essay written with great elegance and clarity? I suggest that the confusion is largely of Sen’s own making.’ (Devereux, 2001) p246*

my work was an attack on entitlement theory, rather than an attack on bad economics, bad statistics and false evidence. Many of Sen's supporters respond to any criticisms of his work with the statement that no weaknesses in his empirical work can refute or discredit his 'entitlement' theory in any way. (e.g. (Thompson, 2015, p. 22) (Cutler, 1988, p. 42) (Osmani, 1993) ), supporting the view that it is non-scientific, a belief system. This supports the view that 'entitlement' is a belief system, not a testable economic theory, and so is not 'scientific'.

*'Yet even if Sen is proved entirely wrong in terms of his analysis of food availability, which is unlikely unless some startling new source of information is revealed, his entitlement theory will still be valid.'* (Cutler, 1988, p. 42)

Cutler's belief that no conceivable empirical evidence can refute Sen's belief system places it on a par with the Famine Fairy belief system. His very different suggestion that we should ignore all the vast evidence that Sen's statistics are wrong '*unless some startling new source of information is revealed*' is extraordinary, the suppression of a large amount of hard, testable, evidence. It is totally unacceptable to take one scrap of 'meaningless' evidence as outweighing the large amount of evidence of different kinds that is available. It is also *suppressio veri* to omit the criticisms of Sen's theory, and statistics and his fabrication of evidence.

It would be irrational to discuss 'entitlement'

For other reasons it would have been irrational for me to discuss Sen's writings on 'entitlement' or anything else, or to discuss the work of his followers. We have every reason to expect that his writings on his 'entitlement approach', like his writings on the Bengal famine, would be a mishmash of fraudulent evidence, fraudulent theory, and fraudulent statistics. And, at the politest, we may take it that the writings of anybody who took Sen's work at face value, even when its errors are pointed out, are to be avoided.

This **discredits** all Sen's work, including 'the entitlement approach.

'Entitlement' is redundant

Another reason not to discuss 'entitlement' is that it was, and is, redundant: nothing would be lost, and a lot gained, if it were quietly dropped. There was already a vast amount of hard theory on the demand side, tried and tested in the field, covering the whole range – not just the snippets he borrowed. I am obsessive about economic

theory: I need good theory because I work on very complex markets, where there are gaps in the data, and the quality of data is poor. For example, the theoretical toolkit I used as a professional agricultural economist covered, on the demand side alone,

- The accumulated wisdom on the subject collected over the centuries up to the 1960s which has been formalized as agricultural economics.
- The new theory produced in agricultural economics since then.
- Agricultural marketing, which was, of course, the first body of work on marketing.
- Non-agricultural marketing, which was starting to become relevant.
- Agricultural marketing economics.
- Standard economics.
- Consumption Theory – which I have published on.
- Experience. Of course, my theory had to conform with the real world, so I had spent a lot of time talking to traders, and to farmers and working in shops. This was relevant to famine marketing. For example, as might be expected from experience elsewhere, Bengal had more than 20 distinct types of rice, each of which had its own price (Stevenson, 2005), with, of course, its own demand and cross elasticities (Bowbrick P. , 1992). There are many implications, including making famine price statistics misleading.
- The experience of my colleagues and the institutions we work for.
- Rural sociology, academic and professional: I expect the Ministry of Agriculture I am working with to be acutely aware of this. Which tribes refuse to plant anti-famine crops and why? Who decides whether to slaughter cattle, the villagers or the Induna appointed by the Paramount Chief? Who allocates village land for production? Who decides what to grow, the headman, the paterfamilias, or each wife on her own account?
- Urban sociology: again, each Ministry of Agriculture is aware of this – feeding the population is their job. For example, are employed people obliged to feed the unemployed in their own extended family, or their own tribe? What is the impact on nutrition?
- Relations between town and country. Again, this is important in the normal work of the Ministry in some countries. In some, urban workers visit their home villages from time to time to collect some of the rice which is their right as family members. In others, urban workers are expected to take a bag of fertilizer with them when they visit their home villages.

These strands of theory are far richer than Sen's journalistic rehash of the myths current in 1943, and cover everything he mentioned and a lot more. Anyone familiar with them would immediately note many holes in Sen's approach, and, yes, I have seen my colleagues finding holes within minutes.

Like others, I found that much 'entitlement approach' was based on popular beliefs of forty to a hundred years before and on empirical work and theory from the 1870s<sup>63</sup> – and that was the best bits. The consumption theory he presented was childishly simplistic and missed most of what we had learned over the previous century. I found it garbled and confused, as others have. I found much of it was obviously wrong, Sen's inability to distinguish between demand and consumption, for instance. But there is no point in pointing out the failures of a belief system. The 'entitlement approach' is no more testable or refutable than Parkinson's Law, the Peter Principle, or the astrology columns in the daily paper. Like them it only says that something may sometimes happen, so people can cling to their beliefs if it does not happen on some other occasions. Like them it provides supporters with plenty of opportunities to find some situations which appear to fit the 'theory', even if some massaging of the data and the economic theory are needed to do so. Like them the 'theory' is so fuzzy and contradictory that people can always find a set of statements that point to one conclusion, and another set that points to different conclusions. 'Entitlement theory' is formally equivalent to the thousands of business books that take well understood phenomena, and present them in a popular form, using their own nomenclature to claim originality. This does mean cutting off the reader from the vast amount of theory and research on these subjects which does not use his nomenclature – Devereux, for example, talks of a food shortage as a "direct entitlement" failure' which, when used as a search term, will pick up nearly none of the relevant literature.

And all this means that the theories cannot be tested let alone refuted: they are not scientific. And this means that they cannot, in any sense, be proved. Nothing can give us any more reason to believe that they are correct.

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<sup>63</sup> The lack of originality of Sen's work has been noted:

*'Mitra (1982, p.488) makes this suggestion in the most trenchant manner: "Amartya Sen, I am afraid, has not said anything beyond what our great-grandmothers were already aware of". In a more scholarly vein, Rangasami (1985) has made the same point by claiming that the late nineteenth century literature on Indian famines was based on the spirit, if not the language, of the entitlement approach. (Osmani, 1993, pp. 40-41)*

Sen's discussion of the impact of the famine on different occupational groups was the part of his work that attracted most attention. This was a rehash of one table from (Mahalanobis, Mukkerjee, & Ghosh, A sample survey of after effects of Bengal famine of 1943. , 1946). This in turn was a replication of the work of Hunter (1873) who gave detailed evidence and analysis some effects of famine such as the occupations of the people most likely to die, and the reasons for this. Sir Bartle Frere (1874), again discussed this. Their work appears to be an attempt to quantify widely accepted phenomena. While Hunter and Bartle Frere presented scientific economics, they and a lot of others also mentioned myths and non-scientific beliefs.

### I did not try to refute 'entitlement'

Despite this, a lot of the attacks on my work are based on what the writers choose to believe I said about entitlement.

Clement (Clément, 2009) says that I don't understand '*the entitlement approach*'. But I do not discuss it.

Fine suggests that I have been involved in a debate on the 'entitlement approach', though I was clear that I was not. He then categorizes my contribution and others (Nolan P. , 1993; Nolan & Sender, 1992) as

*'Second, there are those who have sought to dispute Sen's empirical account of the causes of or absence of famines.'* (Fine, 1997, pp. 621-2)

And then ignores our refutations: *suppressio veri*. So Fine assumes away my refutation, and that of others, while apparently assuming that empirical work is irrelevant to a non-scientific belief system. It also appears that he considers it irrelevant to generalizations about famines, but this is not clear.

### Osmani's misrepresentation

Osmani wrote some 1500 words on my approach as part of a paper on 'the entitlement approach'. He did not send me a copy of his paper, so I was not able to publish a reply when it was published – another way of suppressing criticisms.

Osmani (1993, p. 23) explicitly ignores all empirical work by anybody but Sen, and all empirical criticisms of Sen's work. He states that they are irrelevant, that they cannot challenge or refute 'the entitlement approach', which in his hands includes generalizations on famines (which I, and others, would consider to be refutable). Oddly, he does consider it alright to discuss 'normal economic studies of markets' if done by Sen, but not if done by anyone else.

He says, citing me,

*'What concerns us here is the claim of several of these critics that by refuting Sen's empirical analysis, they have discredited the entitlement approach.'* (Osmani, 1993)P23

He fails to distinguish **refuting** and **discrediting**. I explicitly and repeatedly said that I



was not trying to refute 'the entitlement approach'. Since it is not possible to **refute** a non-testable belief system, a fairy tale, or a religion empirically, one obvious way forward is to **discredit** it – that is the word he uses – by, for instance, showing that the person propagating it is incompetent or immoral, perhaps someone who has fabricated their evidence in empirical work, or used bad or fraudulent statistics and theory. If someone has done this, it would be irrational to have any confidence whatsoever in any of their writings. Osmani, however, suppresses all evidence of Sen's failures in empirical work. He suppresses the fact that Sen has been shown to have fabricated nearly all his evidence, his statistics and his analysis, leaving us with the presumption that everything else Sen wrote is equally unreliable. Thus, he suppresses the fact that the 'entitlement approach' has been thoroughly discredited in this way.

I also mentioned the fact that the 'entitlement approach', in the hands of its creator was not able to pick up all these weaknesses, which is evidence, if more evidence is needed, that the 'entitlement approach' is not a testable, scientific, theory, but a 'belief system'. This states its limitations under economic method. Again, Osmani suppresses this, which is unacceptable.

He switches to another line of attack:

*'A typical example is Bowbrick, who in an extended debate with Sen, has claimed to have refuted Sen's diagnosis of the Great Bengal famine ... He argues that "Sen's theory of famine will lead to the wrong diagnosis and the wrong remedies for famine and will therefore worsen the situation." ... 'Although his empirical arguments relate to this particular famine, he goes on to draw a general analytical conclusion, which is our main concern here. He argues that "Sen's theory of famine will lead to the wrong diagnosis and the wrong remedies for famine and will therefore worsen the situation." (Bowbrick 1986, p. 105)' (Osmani, 1993, p. 23)*

Well certainly there was an extended debate, the details of which Osmani suppresses – *suppressio veri*. Certainly, I refuted Sen's diagnosis of the 1943 Bengal famine: Osmani does not challenge my refutation any more than anyone else does. Certainly, I showed that Sen's diagnosis was a rehash of the one adopted by decision makers in 1943, and it worsened the situation – the view of the Famine Inquiry Commission and the post-famine consensus.

And then, as is normal in economics, I gave a warning to my colleagues: anybody applying Sen's mishmash of myth, abysmal theory, meaningless statistics, and fraudulent evidence, would inevitably worsen the situation. But that is a warning on bad economics and has nothing to do with 'the entitlement approach'. Yes, indeed, Sen's theory of the cause of the Bengal famine is so appalling, that anybody using it as

a guide when analysing other famines will probably reach the wrong diagnosis and the wrong remedies for the famine. And why else would anybody write a theoretical model of any famine except to present a guide to help others examining future famines? This conclusion has nothing to do with 'entitlement'. Nor have the conclusions of the thousands of other economic analyses of various markets I have read.

Or is Osmani claiming that the defining characteristics of 'the entitlement approach' are bad economic theory, fraudulent evidence, meaningless statistics, and bad statistical analysis? I would require a lot of evidence for this. Even if it were forthcoming, it would not **refute** the approach though it would **discredit** it.

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Since Osmani has assumed away all my empirical work, as well as everyone else's, there is nothing left for him to discuss, so he invents an attack: he says that what is left of my work, when my empirical work is assumed away, fails as a refutation of the 'entitlement' approach. But if my empirical work is assumed away, there is nothing left! His long discussion of 'my attack on entitlement' is pure fabrication.

Having suppressed all that I did, in fact, say, Osmani claims that I entered into a detailed discussion of Sen's 'entitlement approach'. This is pure fabrication. In fact, I did not discuss it: it would be irrational and immoral to give any credence to a belief system whose creator has produced such appalling and dishonest empirical work.

*'The first part of the claim, asserting the inevitability of misdiagnosis, is based on two premises. The first premise holds that "one cannot discuss famines without constantly taking into account aggregate food supply" (Bowbrick 1986, p.106), the implication being that reduction in food supply necessarily plays a part in all famines.'*

(Osmani, 1993, p. 24)

There is no such premise. There is no such implication. It is fabrication. It is unacceptable to draw such implications from part of a sentence and to suppress the argument in its entirety. What I said, repeatedly<sup>64</sup>, was that it is, at best, gross

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<sup>64</sup> *'Misdiagnosis can worsen the situation because the degree of shortage, i.e. the food availability per head per day, does not remain constant from one harvest to another. For example, if a government issues too high a ration, a worse shortage will be created, and mass starvation is a possibility. The misdiagnosis means that there will be no imports when imports are the only answer.'*1986 p 107

*'Inequalities in distribution*

*Those who believe in the FAD approach which Sen is attacking place great*

dereliction of duty to deal with a famine without constantly taking into account aggregate food supply – the *possibility* that there was insufficient food, in a situation where availability is constantly changing – as well as all the other factors normally taken into account by agricultural economists, including distribution, demand, purchasing power, changes in income, etc, etc, etc. Any economists I have worked with are always talking about both supply and demand, and we build up complicated realistic models to deal with this. We know that the price of a commodity can double or halve in a single day, in local markets or fruit and vegetable markets for instance. This is less likely to happen in rice markets when imports and stocks can be used to level prices (i.e. in non-famine situations.) When the market opens the market takes a view on supply and demand and sets the price. As the view is often wrong by 10% or more, if sellers maintain the price, there is inevitably a situation at some stage during the market period where the available supply is three times as much as people want to buy or one third as much. Similarly, if government puts a fixed amount on the market each day, at one time during the market period there will certainly be three times as much as people want to buy or one third. They may run out of food three months before the next crop is due. And, of course, perceptions of availability, supply and demand change through the course of a famine. So it would be criminal negligence not to keep checking. If, as Osmani implies, ‘the entitlement approach’ says that governments and traders should not do this, it is evil.

Osmani’s claim that I believed ‘*that reduction in food supply necessarily plays a part in all famines*’ is pure fabrication. I explicitly said the opposite under the heading ‘Causes of Famine’ (1986). Indeed, Sen explicitly says I said the opposite. In 1943, it

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*emphasis on the change in purchasing power during a famine, which means that the poor cannot buy food and that the people who had previously been moderately well off are impoverished. They emphasize the need for relief works, soup kitchens, special agricultural loans, loans for artisans and weavers, etc.<sup>53</sup> They considered that most farmers in 1942 had a reasonable yield with high prices and so were better off. However, rural indebtedness meant that the crop often went to a moneylender or landlord who made all the profits. The indebted farmers had to buy back their food, on credit, at an inflated price. Consumers spent most of their money on food and so could not afford other goods or services - those who supplied these goods and services died. Sen denies that the FAD approach recognizes these phenomena, though his examples are drawn from sources which adopted the FAD approach. He then makes the unwarranted assumption that because these switches in distribution accompanied the famine, they caused it’ (1986 p 118)*

*‘I must disagree in the strongest possible terms. Any responsible government should constantly reconsider its initial diagnosis and its assessment of supplies in case the degree of food shortage is worsening. Making up one’s mind at the beginning of the season and sticking to one’s diagnosis in the face of the evidence is a recipe for disaster.’ (1986 p122)*

was only too obvious that, for instance, an invading army could increase demand. And most countries have deaths from hunger not related to aggregate food supply. The post famine consensus was that the policy Sen now advocates influenced policy and allowed the Bengal famine to occur. *Suggestio falsi*.

Again, he says. *'The second premise consists in Bowbrick's interpretation of Sen's theory as a specific hypothesis that stands in opposition to the FAD hypothesis.'* (Osmani, 1993, p. 24). I said exactly the opposite: *'The appropriate method of examining a famine has nothing to do with the opposing dogmas of the FAD approach (if such an approach ever existed) and Sen's entitlement theory.'* (Bowbrick P. , 1986, p. 124).<sup>65</sup> I most certainly did not suggest that 'the entitlement approach' was a hypothesis: it is a belief system: it has no testable hypotheses – indeed Osmani explicitly rejects the idea that any empirical results can cast doubt on it. That is why my refutation confined itself to the specific hypotheses set out by Sen in his theory of

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<sup>65</sup> *'It is not presented as a supply-side analysis in contrast to Sen's demand-side analysis. On the contrary, it was the weaknesses and contradictions in his demand analysis that showed that his supply figures could not be accurate'. (1986 p105)*

*'Famines may be caused by: a fall in food supply, a rise in the demand for food (e.g. because of an influx of refugees), a redistribution of what food is available (e.g. with hyperinflation or unemployment meaning that some people cannot afford to buy food), or shortages within a season, caused by transport problems, etc. It is difficult for a casual observer to isolate the cause of a particular famine, because a famine caused by crop failure, for example, will necessarily be accompanied by speculation, a redistribution of income, reduced purchasing power by some groups and by regional shortages.'*

*'Adam Smith thought that, in practice, all famines were caused by a sudden fall in food supply.' Likewise, modern economists have thought that such famines were the norm, but that they can be caused, on occasion, by other factors.'* (1986 p 106)

*'His belief that any economist working on famines could ignore demand is odd. Mainstream economists have been obsessive about the demand side of famines for centuries and have certainly not forgotten it in the last 50 years. Since 1939, the UK has been carrying out an annual National Food Survey which was started precisely to find out how the poor were eating during the war, and which continues to give information on the nutrition of different socioeconomic groups. Commercial market research is no less interested in the ability of different socioeconomic groups to buy products. The existence of working-class cost of living indices in India in 1943 (which Sen quotes) shows that the matter was on economists' minds then. It is impossible that anyone in a country with a shortage would not be aware of the problem. I am now working in a country with a food problem and every day colleagues, friends or casual acquaintances give me detailed budgets to show that they cannot afford to buy rice. The message is hammered home by the near riot in the queue for relief rice outside my office. (1987 p8)'*

the Bengal famine.

Osmani then claims

*'He [Bowbrick] suggests that the entitlement theory proposes a 'redistribution hypothesis', (Osmani, 1993, p. 24).*

I made no such suggestion. I never used the phrase. I did not discuss 'the entitlement theory', only Sen's theory of the 1943 Bengal famine. Nor did I suggest that 'the entitlement theory' presents hypotheses. It is a belief system: it does not present any testable hypotheses. The many hypotheses I did examine and refute were specific to Sen's theory of the 1943 Bengal famine. *Suggestio falsi*.

Nor did I generalize on 'the entitlement approach'. *'Following such reasoning, Bowbrick comes to the conclusion that the entitlement approach will tend to misdiagnose famines in most situations'* (Osmani, 1993, p. 24). No, I drew no such conclusions: *suggestio falsi*. Again, what I did say about the entitlement approach, was to point out that, in the hands of its originator, it failed to identify multiple examples of fraudulent evidence, statistics and theory, as well as bad theory.

*'His argument would have had some merit if his interpretation of the entitlement approach were valid'* (Osmani, 1993, p. 25). Again, I presented no interpretation of the entitlement approach; I examined Sen's theory of the 1943 Bengal famine. *Suggestio falsi*. Oddly, Osmani understands that my 'degrees of shortage' are definitions, but draws his own idiosyncratic conclusions from them, attributing them to me (Osmani, 1993, pp. 23-24).

It is concluded, therefore, that Osmani assumed away all empirical criticisms of Sen's work: *suppressio veri*. He also assumed away the implications of these criticisms, that nothing Sen wrote can be trusted: *suppressio veri*. He falsely claimed that I had discussed 'the entitlement approach' at length, and repeatedly fabricates statements that I did not make – *Suggestio falsi*. I am afraid that this confirms my belief that one must be extremely careful with anything written by people who read Sen, and criticisms of Sen, without taking the time to understand them.

#### De Waal's misrepresentation

We may look at how De Waal dealt with the allegations of fabricated evidence, erroneous or fraudulent statistics and theory in his many writings on famine. His only citation of me in a long career is:

*'Those who hoped that Sen had slain Malthus's zombie were mistaken. Sen's most relentless critic, Peter Bowbrick, tirelessly and tiresomely attempted to rebut the 'failure of exchange entitlements'*

*approach to famine, trying to show that food shortages, or at least food availability declines and disruptions, caused famines. In his book Poverty and Famines, Sen showed empirically in three cases that famine had occurred without a decline in national-level food availability (Bengal 1943, Ethiopia 1973 and Bangladesh 1974) and that in one (Sahel 1973) there had been a food crisis as such.’ (De Waal, A, 2018, p. 48)*

How many false statements can you get into two sentences?

1. The abuse rings alarm bells, as does his use of value-laden language when describing other people who he disagrees with, or the results of their work. This is bullying everyone studying famine.
2. De Waal suppresses the fact that I have presented a rigorous, formal, economic refutation of every part of Sen’s theory of the Bengal famine. Nobody – and certainly not Sen – has attempted to challenge these refutations. De Waal cannot claim that he is ignorant of these facts: he says that he finds it ‘tiresome’ that I keep producing more evidence, statistics and theory which are not compatible with his personal prejudice. *Suppressio veri.*
3. No, I did not attempt to ‘rebut the “failure of exchange entitlements” approach’ [sic]: This approach is non-testable and so ‘non-scientific’, and I explicitly excluded it from my discussion.

*‘It has not been the aim of this paper to appraise Sen’s entitlement theory’ (Bowbrick P. , 1986, p. 123)*

*‘The appropriate method of examining a famine has nothing to do with the opposing dogmas of the FAD approach (if such an approach ever existed) and Sen’s entitlement theory.’ (Bowbrick P. , 1986, p. 124)*

4. I presented rigorous economic refutations, based on the facts and hard theory – state-of-the-art agricultural economics theory and mainstream economics theory – to examine Sen’s theory. Nobody has challenged my refutations. This happened to come up with results that did not support De Waal’s prejudices. I am shocked to see any economist, academic or professional, using ‘Malthus’s zombie’ as being what the Thought Police have decided is a heresy against the glorious leader’s pronouncements. Calling me Malthusian, Keynesian, Marxist, Neo-classical, Supralapsarianistic or Antidisestablishmentarian cannot in any way affect the facts and the theory of the 1943 Bengal famine.
5. I did say

*‘The mainstream view, going back to Malthus at least, is that*

*purchases of grain for distribution through the public system (Sen's recommendation) can be disastrous. Because there is a shortage, government merely bids up the price to astronomical levels.'* (1986 p109)

to show how long-established the theory is – though I suspect that this was first observed thousands of years ago. If De Waal dismisses this universally accepted bit of theory as '*Malthus's zombie*', he dismisses all theory using supply and demand – all modern economics in fact.

6. I was examining only Sen's theory of the 1943 Bengal famine and not attempting broad generalizations about the world. I certainly did not try '*to show that "food shortages, or at least food availability declines and disruptions caused famines."*' They can of course, but we have a vast knowledge of many possible causes, gathered over the millennia. I said, and Sen acknowledges that I said, that there have been occasions when famines were caused primarily by changes in supply (no economist would try to examine the progress of a famine without taking supply and demand into account.)
7. De Waal states firmly as late as 2018, '*Sen showed empirically in three cases [including Bengal] that famine had occurred without a decline in national-level food availability*'. He chooses to ignore or suppress the fact that the statistics on which Sen's statements were based were considered meaningless by the Indian statistical profession before the famine, and by the world's statisticians afterwards, that critics have shown that empirical evidence, both from Sen's sources and from contemporary sources that he did not cite disagreed with Sen's sources, and that a range of economists, experts in their own areas of economics had challenged his economic analysis. De Waal states that he has read the evidence I produced at least. He does not challenge it but chooses to suppress it. And this in a book on famine! *Suppressio veri, suggestio falsi*.

It is alarming that De Waal has written extensively about famine.

#### Devereux's misrepresentation

Stephen Devereux, an academic who has been writing on Sen's 'entitlement approach' since it first came out, wrote a paper 'Sen's Entitlement Approach: Critiques and Counter Critiques'. This turns out to be an exercise in suppressing critiques and misrepresenting them: both *suppressio veri* and *suggestio falsi*.

There is no possible defence to my many refutations of Sen's theory of the 1943 Bengal famine. Nobody has challenged them. Devereux chooses to suppress them, and to suppress their implications – that, for example, the views of anyone who, like Sen,

produces such abysmal statistics and theory, and who systematically fabricates their evidence, has zero credibility:

*'I shall not dwell on the **empirical** criticisms arising from Sen's applications of the entitlement approach to specific famines; instead I shall examine its credentials as a conceptual framework and **analytical** tool.'* (Devereux, 2001) p24

He then misrepresents the fundamental objective of my papers. *Suggestio falsi*. He states, falsely, that I claimed to have refuted 'the entitlement approach' (Devereux, 2001, pp. 245, 260), when my only mention of it was the statement that I was doing no such thing. (Bowbrick P. , 1986, p. 123. 124)<sup>66</sup>

*'The entitlement approach has been subjected to critical scrutiny many times before, ranging from a favourable "assessment" by Osmani (1995), to less favourable "reassessment" by de Waal (1990), "critique" by Nolan (1993), even "refutation" by Bowbrick (1986) and dismissal as a theoretical "failure" by Rangasami (1985) and Fine (1997).'* (Devereux, 2001, p. 245)<sup>67</sup>

*'Critics who challenge Sen on empirical grounds often confuse the two [Sen's entitlement approach, and his empirical work], believing that by demonstrating that specific famines were triggered by food availability decline they have "refuted" the entitlement "theory" of famine (see especially Bowbrick, 1986). In fact, the best they can hope to do is refute the "exchange entitlement decline" hypothesis for individual famines, since the entitlement approach incorporates the possibility of food availability decline as "direct entitlement" failure. (Devereux, 2001, p. 260)*

These statements are both *suggestio falsi* and *suppressio veri*. I was very careful to say

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<sup>66</sup> *'It has not been the aim of this paper to appraise Sen's entitlement theory'* (Bowbrick P. , 1986, p. 123)

*'The appropriate method of examining a famine has nothing to do with the opposing dogmas of the FAD approach (if such an approach ever existed) and Sen's entitlement theory.'* (Bowbrick P. , 1986, p. 124)

<sup>67</sup> I commented on Osmani and De Waal earlier in this paper, showing that there is reason to mistrust anything they say on Sen.



that I was refuting Sen's theory of the cause of the Bengal Famine of 1943 and that alone. My refutation is valid under all the meanings of refutation I have come across (and, as I have published more refutations than any academic I know of, I have studied epistemology and methodology carefully).

Devereux seems to accept, when it suits him, that it is not possible to use empirical evidence to refute or support 'the entitlement approach' any more than the 'Famine Fairy' approach or the astrological column in the daily paper. It is a non-scientific belief system. So I did not attempt to refute it.

Devereux has misrepresented what I did and what I claimed to do. *Suggestio falsi*. The effect was to hide my very serious criticisms of Sen, of his systematic fabrication of data, and his very bad economic analysis, and his fraudulent results. *Suppressio veri*. It was, and is, my belief that it would be irrational for anybody who has read my criticisms to read or cite Sen. It was, and is, my belief that anybody who reads or cites Sen, having read my analysis, has zero credibility.

Devereux says:

*'Refutation of entitlement "theory" by reinterpretation of data: In Poverty and Famines, Sen recalculated data from four famines to demonstrate: (a) adequate food availability and/or negligible decline from pre-famine food availability; (b) exchange entitlement collapse for specific population groups as a proximate cause of famine. Some critics have challenged Sen's use of food production, trade and price statistics, to claim that Sen underestimated the extent to which food availability decline was in fact an important element in these famines.'*  
(Devereux, 2001, p. 247)

and he cites me as an example.

I do not just 'challenge' Sen: this would imply a minor disagreement between academics, which he suggests is not worth discussing. I refute it formally and rigorously in many different ways (Bowbrick P. , 1986; Bowbrick P. , 1988, 2008). I do it so well that nobody, not Sen, not Devereux, not anybody else, has attempted to challenge or refute my refutations. Devereux suppresses this. What is more, in his footnote he mentions only three people who have challenged Sen on the factual basis and theoretical analysis of his theories of the causation of these famines. But there are dozens, at least, whose work he chooses to suppress.

[Sarracino's misrepresentation](#)

Sarracino dismisses my work on the following grounds:

*‘Bowbrick argues that famines cannot be discussed without taking into account aggregate food supply and contends that, by focusing exclusively on distribution, Sen ignores aggregate shortage. The problem with Bowbrick's critique is that he considers a limited space of causes and effects of famines attributable to Malthus's view. Moreover, also the second Bowbrick's sentence is inexact, because Sen focuses mainly on distributional problems but doesn't ignore in his framework the possibility of absolute shortages of food’ (FAD hypothesis). (Sarracino, 2010, p. 23)*

Just that! No mention of the multiple errors of fact, theory and statistics! *Suppressio veri*. I did indeed consider ‘*a limited space of causes and effects of famine*’ including changes in demand. That was because I was refuting Sen’s theory of the Bengal Famine, so I had to confine myself to what was in this theory. I made it clear that, to be fair to Sen, I confined myself to the sources he used, and that, had I been presenting my own explanation, I would have used a much wider range of sources and theory, and there would be no overlap with Sen’s theory. So what Sarracino evidently perceives to be criticism – that what I discussed is ‘*attributable to Malthus's view*’ – whatever that may mean – is a criticism of Sen, not me. I did indeed point out that Sen’s sources had showed that politicians and administrators concentrating on demand shifts and assuming away availability shifts had had a disastrous effect in Bengal in 1943. And, no, I did not contend ‘*that, by focusing exclusively on distribution, Sen ignores aggregate shortage*’. On the contrary, his focus on aggregate shortage is often interpreted as central to his argument, his claim that there was more food available in 1943 than in 1941. I showed that Sen had grossly misrepresented the evidence and used and misused meaningless statistics in order to claim that there was little or no aggregate shortage. Since the point of writing a theory of this or any other famine is to provide guidance to anybody who may be dealing with a famine in future, I did suggest that anyone dealing with a given famine should constantly reassess availability, as well as demand – both of which can be expected to change repeatedly before and during the famine. (See the section on Osmani’s misrepresentation above for more on this.) Everything Sarracino says about my work is false.

#### [Ravallion’s misrepresentations](#)

Ravallion wrote a review article, ‘Famines and Economics’, for the Journal of Economic Literature (1997). Readers of this particular journal will have expected the author to set out all points of view, including those he disagrees with. In fact, Ravallion

suppressed the very fact that I had argued that Sen's his theory was fraudulent, though he was perfectly well aware of it (Ravallion M. , 1998). He also suppresses the fact that **nobody** has challenged this refutation, not Sen, not anybody else. The readers may reasonably conclude that they have been deliberately deceived. *Suppressio veri*.

Ravallion sent an email to people who might wish to employ me – a top executive in the World Bank and others – saying that he did not accept my criticisms and that Sen

*'gave a detailed and credible reply to his original paper, in the same issue of Food Policy (May 1986).' (Ravallion M. , 1998)*

If he had read my papers, especially my 1987 reply, if he had even read the abstract, he could not fail to have been aware that Sen's reply does not even mention 95% of my refutations, those listed in the conclusion to my 1986 paper, so it cannot be considered detailed or credible. The abstract to my rejoinder (1987) is unequivocal:

*'This is a rejoinder to Professor Sen's reply. It is argued that Professor Sen has not attempted to answer most of the criticisms or to defend his misstatements. Where he has, he has introduced new misstatements. There are also some new errors.'*

In fact, Sen's reply is about as far from a '*detailed and credible reply*' as can be imagined.' So we have *suppressio veri* and *suggestio falsi*.<sup>68</sup>

#### *Ravallion on speculation*

Ravallion (1990), like Sen, suppresses the fact that the Famine Inquiry Commission, and contemporary sources such as (Braund, 1944) (Pinnell, 1944)<sup>69</sup> all, very properly, set out the null hypothesis that the famine was caused by speculation, and then tested it rigorously (as they tested the other hypotheses that Sen markets as his own). The analysis was rigorous, in accordance with speculation theory that would

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<sup>68</sup> There was also an e-mail campaign by two research students, whose names I withhold, as they were very young, sending personal abuse about me to anyone they could think of in the profession. They worked in a different area of economics and they had never published anything, but they were very angry. On enquiry, they disclosed the fact that 'dear Professor Sen's' wife had died a year or two earlier, and said that it was therefore very wrong of me to criticize his work. As Stalin said, 'One death is a tragedy; a million deaths is a statistic.'

<sup>69</sup> I am grateful to Denis Segal who found these sources and brought them to my attention. I am grateful too to the authors' literary executors who gave me clearance to get them made available in electronic form by the British Library. Denis Segal also described to me the horrific problems he had encountered when delivering food aid during the monsoon, sometimes landing it on an island and having to transport it through mud to a village a mile away, and all in a landscape dominated by wandering bands of armed dacoits who had chased the police away.

still be considered valid and used a range of evidence that would seldom be available in a famine. It was shown beyond any doubt that the famine was not caused by speculation. Neither Sen, nor anyone else has challenged me when I cited the Famine Inquiry Commission and presented my own analysis – Sen, like Ravallion, was careful not to draw the readers' attention to it, as it cannot be challenged.

Ravallion (1990) does cite the following:

*'There is an enormous literature on speculation, hoarding and storage, dating back at least to Adam Smith. One thing at least is agreed – the uninformed layman's criticisms of speculation are unfounded. ... Hoarding, like speculation, is a bogeyman invoked by politicians and administrators.'* (Bowbrick P. , 1986)

Like Sen, Ravallion suppresses the sentences,

*'Yet Sen does not provide a model to show why the uninformed layman should be correct in this instance.'* (Bowbrick, 1986 pp118-9)

*'It is concluded that Sen's bald statement that the famine was caused by speculation is contrary to accepted theory, and he has given no reason why in this case the accepted theory is wrong. He is also contradicted by the facts in the sources he cites. The point is firmly refuted.'* Bowbrick, 1986 p 120

Ravallion also suppresses the fact that Sen made no attempt to provide such an economic model in his two responses. *Suppressio veri*. He then says that it was not me but Sen who asked for a model rather than an assertion when claiming that the orthodox economic theory and facts were wrong – *Suggestio falsi*:

*'As Sen (1986) has noted (in response to Bowbrick), anyone familiar with the theoretical conditions needed to support such a faith in markets should be reluctant to share that faith in markets without some good evidence.'* [sic] (Ravallion M. , 1987, p. 32)

The sentence is meaningless, so I tried to find what Sen had said. I have been unable to find any such statement or even these key words in Sen's 1986 or 1987 papers. It is disturbing that a casual reader, perhaps a non-economist, or someone who has not read my work, may have read Ravallion and believed that he and Sen were making some abstruse theoretical point that cast doubt on what I said. It is even more disturbing that the same meaningless sentence appears in two more appearances

(Ravallion M. , 1997) and as a chapter in Dreze (Dreze, 1995). In fact, I had said that the onus was on Sen to produce a model, since he disagreed with accepted theory and all the evidence in his sources.

Ravallion also suppresses the fact that, in the same paragraph (Bowbrick, 1986 p119), I said, with the academic politeness of the time – the pretence that a competent economist had inexplicably made a mistake – , that Sen was acting fraudulently by asserting that the Famine Inquiry Commission supported his assertion, when in fact it had provided a detailed and rigorous refutation of it.

Oddly, Ravallion does cite some papers on speculation when discussing my work, papers which do not refer to the 1943 Bengal famine or deal with the realities of this famine (some of which I set out Appendix 2), and which are, therefore irrelevant. He does not attempt give any explanation of how these papers explain how one or two million tons of rice vanished into thin air, as Sen would have us think. Nor do they relate to any arguments made by Sen. This is unacceptable.

#### *On increasing aggregate food supply*

Ravallion makes claims about my beliefs:

*‘It has been argued that an increase in aggregate food availability is not called for to stop a famine which was not itself caused by a decline in aggregate availability (Bowbrick 1986). And the belief seems to have occasionally been put into practice.’ (Ravallion M. , 1997, p. 1228)*

Evidently Ravallion did not read my paper. The first claim appears to derive, instead, from Sen’s statements (1986 p126) which were discussed above in the section *The merit of food imports* and shown to be false in every detail. The main message of my 1986 paper, made at some length, was that such a policy was extraordinarily dangerous. Again, it appears that Ravallion has not read what I said, but relied on Sen’s statements (1986 p 126), even though he was perfectly well aware that I had accused Sen of acting fraudulently (Ravallion M. , 1998). Presenting Sen’s assertions as though they were his own independent research is also plagiarism, and Hoffer shows at some length that the careful rewriting of Sen words is an aggravating factor indicating that the plagiarism was deliberate (Hoffer, 2004, pp. 175, 180, 203-7). This plagiarism was of a particularly serious form, calculated to persuade the reader that two commentators had independently reached the same false interpretation of what I said. Academic ethics would require that Ravallion says something like, ‘Sen (1986 -126) asserts, without presenting any supporting evidence or mentioning the evidence to the contrary, ‘...’ *Suggestio falsi, suppressio veri.*

Other commentators have stated that I said something diametrically opposed to this:

*'Bowbrick (2008, 63) also argues that importing food in a situation of existing insecurity is the only way to minimise the food problem and to "save lives".'* (Engler, Köster, & Siebert, 2014, p. 6/27)

In fact, the belief has been put into practice, leading to disaster, not occasionally as Ravallion suggests, but frequently, over the centuries, particularly dramatically in the case of the Bengal Famine. (Indeed as I was writing my rejoinder, I was dealing with urban myths which gave complex, unevidenced, and extraordinarily unlikely, but enchantingly quirky reasons not to import when even when we would be in a full blown famine in three or four months without imports (Griffiths, 2003)).

Elsewhere Ravallion says,

*'Others have argued that Sen undervalues the importance of food availability, and that in so doing he risks misinforming famine policy and worsening the situation (see for example Peter Bowbrick 1986; and Devereux 1988).'* (Ravallion M. , 1997).

I did not say anything so flattering. I said that Sen had consistently and repeatedly acted fraudulently in his theory of the 1943 Bengal famine. Anything he says is, therefore, suspect.

#### Rubin's misrepresentation

Rubin is another academic who has published extensively on famine. Again, he misrepresents the criticisms.

*'Several scholars have argued that the entitlement approach diverts attention away from where it is needed most: food production ([including] ...Bowbrick 2008 ...).'* (Rubin O. , 2009)

No, I have not written on the use of entitlement approach in food policy. I have not seen any attempt to mix the two, thank God. Changing food production in the course of a famine is seldom possible.

*'In so doing, Sen's approach has even been accused of causing famines instead of curing them (Bowbrick 1986: 105).'* (Rubin O. , 2009)

I have indeed shown that the approach that Sen markets as his own was applied

during the 1943 Bengal famine, and was the main reason the famine was allowed to happen. This is in accord with the post-famine consensus. And there have been other examples in history. Nobody has challenged me on this. So why the sneer?

There is another way in which true believers may cause famines

*‘These scholars believe that food production shortages (both locally and globally) are the most important factors in causing famines and recommend warning systems based on the monitoring of food production and availability (Bowbrick 2008; . . .). As such, their arguments clearly belong to the FAD tradition.’ (Rubin O. , 2009)*

Is there really anyone, including those who believe in Sen, who doubts that an early warning system can, in some cases, tip the balance and make it possible to prevent a famine? Is there really anyone who has not noticed that some countries are particularly vulnerable to famine caused by weather, for instance? Rubin seems to be asserting that only a heretic, a FAD believer, would support early warning systems for supply: a true Sen follower would abolish them. He does not describe an early warning system for demand famines. This is yet another example of my concern:

*Bowbrick argues (2008: 15) that “the entitlement theorists are so eager to distance themselves from the FAD-approach that they tend to avoid looking at availability at all.” (Rubin O. , 2009)*

It is entirely unacceptable that Rubin should use the words, *‘As such, their arguments clearly belong to the FAD tradition’*, to condemn arguments as heretical because the facts of a single episode in a single market do not support a political, religious, or belief system such as Sen’s entitlement approach. In this case the people denounced as heretics say that in one single famine, the one they are writing on, the main precipitating cause was a fall in production, not a change in demand. I have never come across anyone who had ‘an FAD approach.’ I doubt if any of these critics if any of us would deny that some famines have been caused by a change in demand – it has been part of economics for millennia.

Rubin is aware that

*‘Some scholars have questioned the validity of Sen’s empirical examples. [some including] Bowbrick (2008), ... have re-examined the 1943 Bengal famine and they all call into question Sen’s claim of a FAD-less famine ...’ (Rubin O. , 2009)*

He suppresses the obvious implication that Sen is a bad economist, who has, at best,

made multiple errors, but refuses to retract what has been refuted.

It is a gross misrepresentation of my work and the critiques I am most familiar with to say,

*‘This type of critique questions whether Sen’s entitlement approach can adequately capture the effect of a FAD and whether the policy conclusions derived from the entitlement approach will be the most beneficial policies for avoiding famine.’*

No, we were showing that Sen’s analysis of specific famines was wrong. I believe that it would be irrational and immoral to produce the critique he describes.

While Rubin chooses to ignore this evidence of fraud, he does cite me once, on another topic,

*‘Bowbrick (1986) argued that the approach leads to the wrong conclusion and that it might cause famine rather than prevent it. More than 20 years later, Bowbrick (2008, p. 18) still argued that the approach could lead to millions of deaths.’*

Well yes. ‘When the facts change, I change my mind. What do you do, sir?’ (John Maynard Keynes). When the facts and the theory do not change and are not challenged, I do not change my mind. Why should I, sir?

#### [O Grada’s misrepresentations](#)

It is also instructive to look at the work of O Grada, who has written a number of papers on the Bengal Famine and Sen over the last thirty years. He presents reviews of the literature which suppress all mention of refutations, fraud, or bad economics (O Grada C. , 2007; 2008). His statement,

*‘While Sen’s interpretation of the Bengal famine has not escaped criticism, it undoubtedly remains the most influential’ ‘See, for example, Bowbrick, ‘Causes of famines’; Goswami, ‘Bengal famine’; Islam, ‘Great Bengal Famine’; Weigold, ‘Famine management’; Law-Smith, ‘Response and responsibility’; Alamgir, Famine in south Asia, pp. 82–3; Tauger, ‘Entitlement’. (O Grada C. , 2008, p. 21)*

I find his suppression of criticisms of Sen in his review of the literature (O’Grada, 2007) and his suppression of evidence deeply troubling. *Suppressio veri, suggestio falsi*.

He cites me only twice, once as supporting his belief that there was a very limited



shortfall in the 1942 crop (O Grada C. , 2007, p. 27). This is incorrect: I have no confidence whatsoever in his calculations. I quoted the Indian statisticians' evidence that the crop forecasts he uses could easily have been wrong by 50%, invalidating everything O Grada has written on the famine, but he suppresses everything I and the Indian statistical profession has said on them. *Suppressio veri, suggestio falsi*.

He presents crop forecasts without caveats.

His use of abysmal production, export and population data in fancy mathematical analysis is equally worrying. The analyses multiply the errors of the source data.

His use of population statistics as though they were accurate and comparable, in spite of the caveats in his main source (Government of India, 1946), is worrying.

O Grada's price statistics are appalling, usually based on newspaper stories, or equally unreliable sources. There is no discussion of the collection procedures or reliability. On the contrary, O Grada quotes prices from Knight (1954) without mentioning Knight's caveats (p35).

There is every reason to doubt whether the figures cited are comparable, over time or between locations, let alone accurate.

# PART 4: CONCLUSIONS

## BAD ECONOMICS DRIVES GOOD OUT OF CIRCULATION

Gresham's law – one of the oldest economic laws – states that bad money drives good out of circulation. If debased coinage has the same money value as pure silver, people will melt down the pure silver for other uses. In the same way, bad economics drives good economics out of existence.

I have shown that, in one research programme at least, fraud and abysmal economic analysis is published and cited as long as it supports the dogma of the referee or the journal. Anyone can be a prolific academic publisher and get top jobs just by submitting a lot of bad papers supporting one viewpoint. Those who publish most become editors, referees and professors and use their position to suppress papers that do not support their viewpoint

People who do first class research, meticulously checking their facts, and analysing it with state-of-the-art theory – all of which takes time and skill – will be unable to compete, and are likely to switch to areas where their skills are appreciated.

It is easy to churn out pure theory by the yard, if one does not mind having unrealistic or contradictory assumptions, if one makes no attempt to have assumptions or conclusions that relate to the real world, if one makes it so opaque that the referee will not try to understand it. It is easy to get garbage statistics, feed them into a computer and produce econometric analyses giving results quoted as being accurate to eight decimal places, but which bear no relation to reality.

Some people invent their own terminology for well-known marketing phenomena – a favourite tactic of those who write business books for sale at airport terminals. Using your own terminology means you can use words in quite different meanings from one page to another, and get away with it. As with the 'entitlement approach'.

But those of us who try to do important, rigorous, research, checking our facts and using state of the art theory, cannot churn out papers at this speed. Bad research drives good out of circulation.

Those who have researched the refereeing process over the years concluded that it is worse than random in selecting good papers. Two referees are likely to agree to reject dreadful papers, and to accept mediocre ones. But they are also likely to agree to reject the outstanding ones, that are cited thousands of times when they are

eventually published. Indeed, if one raises the question of refereeing in any academic common room people will produce dozens of examples of dreadful refereeing.

## THE BANALITY OF EVIL

Around the time of the Bengal famine, Adolf Eichmann was rounding up 400,000 Jews and sending them to the death camps. He did not hate the Jews; indeed, he was a Zionist, wanting them to go to Israel, but he had a job to do and did it to the best of his ability. As he went to the gallows his chief emotion was his long-held resentment that the Reich had not recognized how well he had done a difficult job, and had not given him the promotion he deserved 'The banality of evil' was Hannah Arendt's comment (Eichmann in Jerusalem: A Report on the Banality of Evil , 1964).

Eichmann's crime was a minor matter compared with producing fraudulent economics of famine. The fraud means that famines are caused, are not prevented or are dealt with badly, resulting in death and destitution on a massive scale.

In most cases the non-academics who cause famines appear to have similarly banal motivations. I have met people who were creating food crises in African countries, not because they hated Africans, but because that is what they were ordered to do by the World Bank and IMF. I have worked in a country where the civil service had a long history of concealing food crises and actual famine, because the President did not want to admit that such a thing could happen under his rule.<sup>70</sup> I have also worked in a lot of countries where food policy was causing a constant low level food crisis, most obviously resulting in very high toddler mortality – killing a lot of people over the years. Most outside observers, and many of the junior civil servants, saw that there was problem, but the usual problems of political intransigence, inter-departmental politics, personality clashes, bloody-mindedness and pressures from international organizations pushing a particular political model, resulted in no action being taken. And there were the businesspeople who thought that the famine was certainly going to happen, because of Act of God and the incompetence of the government, so they might as well make as much money out of it as they could. I have met someone who stopped the imports of emergency food aid, in order to collect some bribes for himself. I suspect he satisfied his conscience with the reflection, 'Everybody is trying to make money for themselves, everybody is corrupt – that is how

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<sup>70</sup> This was in Malawi. However, B. R. Sen reports that when he was in charge of Indian food administration in 1946, the Chief Whip of the Congress Party did something similar.

*'At the very mention that we should ask for a grant from the USA, he became angry and said that on no account should we go begging even if thousands of people were to die! The proposal got no further. (B.R. Sen, 1982, pp. 55-56)*

the world works. I am just better at it.' All apparently normal people who had no animosity to those they killed, most of them charming, but few of them even wondering if they were doing something wrong, and those who wondered having devised protective strategies to stop themselves from drawing the obvious conclusion. The banality of evil.

But why should academics have decided to produce fraudulent theory which might kill tens of millions of people, and to suppress the work of those who have shown it to be fraudulent? They do not have the excuse of Eichmann and those operating Auschwitz that they were only obeying orders. It is easy to see why undergraduates might be taken by Sen's ideas. One can see that a student from an economically secure background who may be stunned by the revelation that some people starve because they cannot buy enough to eat. One can see that Sen's journalistic rehash of the myths that appear in famine situations and his claim that he was attacking orthodoxy would be inspirational to an undergraduate. After all, the myths keep re-emerging because they are so attractive. The Famine Inquiry Commission had presented a damning report on the administration, which fitted the post-war anti-imperialist sentiment in Britain. Of course, most students have similar enthusiasms about some ideas, but our degrees teach us to beware of our enthusiasms and how to analyse them, so the enthusiasms are usually dropped or put aside – to be believed but not used – after a year or two.

But I do not believe that any academic, whether lecturer, or post-graduate researcher or writer on famine does not have the intelligence or training needed to understand the criticisms. Can it be that the chance of getting an easy publication by supporting the views of certain editors and referees, and so getting employment and promotion is the overriding motivation?

## APPENDIX 1: THE FUNGUS

The 1942 rice crop was attacked by a fungus (*Helminthosporium oryzae* Breda de Haan, now called *Bipolaris oryzae*) which caused serious losses in the districts of Birbhum, Bankura, Midnapur, Hoogly, Howrah and 24 Paraganas (Padmanabhan, Raichoudhury, & Ganguly, 1948). Padmanabhan, who studied this disease over a lifetime<sup>71</sup> said,

*Nothing as devastating as the Bengal epiphytotic of 1942 has been recorded in plant pathological literature. The only other instance that bears comparison in loss sustained by a food crop and the human calamity that followed in its wake is the Irish potato famine of 1845.*

(Padmanabhan S. , The Great Bengal Famine, 1973)

He presents a table of the ‘Yield of rice per hectare at the Rice Research Stations at Bankura and Chinsurah in the epiphytotic year (1942) compared with yield per hectare in the stations in a normal year (1941)’ showing that, for the relatively unimportant early *Aus* crop, trial plot yields were down by 6.8% to 58.2% depending on variety, and, for the critical late *Aman* crop, which provided the bulk of the food, yields were down by 39.5% to 91.2%, with nearly all varieties having losses of more than 70%. It is unfortunate that nobody collected similar data from the network of research stations, trial plots etc that covered Bengal at the time – these would have provided a valuable check on the validity and accuracy of the crop forecasts<sup>72</sup>. It appears to be

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<sup>71</sup> Tauger, who found this research, assessed Padmanabhan’s qualifications, ‘Padmanabhan’s key paper thanked a range of plant pathologists from different organizations who had experienced the outbreak in different contexts. “The Great Bengal Famine” (Annual Review of Phytopathology, Vol. 11 (1973), cited twenty-five articles in scientific journals (not “musings”) by Padmanabhan, N.K. Chakrabarti, S.B. Chattopadhyay, T. Hemmi, T. Nojima, and several other Indian and Japanese scientists. In addition to this and many other articles, Padmanabhan also published *Rice Research in India*, co-edited with P.L. Jaiswal (New Delhi: Indian Council for Agricultural Research [ICAR], 1985), *Breeding for Disease Resistance in Rice*, coauthored with S. Gangopadhyay (New Delhi: Oxford University Press and IHB, 1987), *Rice Production Technology* (Bombay, 1980), *Fungal Diseases of Rice in India: A Critical Review* (New Delhi: ICAR, 1974), and other publications on related topics. Both the International Rice Research Institute and the Indian Central Rice Research Institute (click on “Overviews,” “Background and Location”) attribute the Bengal famine to the plant disease that sharply reduced the 1942 harvest. (Tauger, Mark and reply by Amartya Sen, 2011)

<sup>72</sup> Bengal had a network of research stations including

- 2 Research Institutes at Dacca and Calcutta,
- 3 main experimental farms at Dacca, Chinsurah and Rajshahi
- 7 special or subsidiary research stations and research farms, at Berhampore, Rangpur, Krishnagar, Chinsurah, Bankura, Rangamati, Kalimpong
- 22 research/demonstration farms and agricultural stations run by the agricultural department

impossible to access this data now. (Dipak Basu personal communication.) It would appear, from his acknowledgements and the number of co-workers he had that this research was in line with what agronomists and plant pathologists had observed at the time.

He argued that the epiphytotic was caused by unusual climatic conditions, a crop weakened by early season drought being hit by an attack of the fungus when there was a late season rainfall. This meant a perfect climate for fungal growth when the rice was approaching harvest and was particularly susceptible to fungal attack.<sup>73</sup>

The fungus introduced a major bias into crop forecasts. Crop reporters, administrators, farmers and agricultural officials, would have failed to allow for the reduction in the 1942 crop, when the crop was hit by a drought, then a cyclone, then a devastating fungus attack the cumulative effect of this would have been substantial.

One bias is that 'eye estimation makes for a general toning down of fluctuations'.<sup>74</sup> (Panse V. G., 1954 ; Panse V. , 1954b). It seems that in India, as everywhere else in the world, estimators are overinfluenced by the good parts of the field, and that the effect of crop failures is underestimated. Statisticians around the world also point out that when looking at a standing crop people are likely to underestimate the damage caused by fungus, disease, and insects.<sup>75</sup> Similarly, survey staff in a sample survey may think it is 'unfair' to measure the puddle, path or swathe of lodged rice that the sampling system has identified as their plot, when the rest of the field is growing well, so they measure a different plot instead.

They are particularly likely to underestimate the losses if the problem is not one they are familiar with. The reporters would have recognized the brown spots as being a common disease which appeared early in the season with minor effects on the yield, perhaps 10%. Nobody in Bengal realized that this common disease would suddenly turn into a major fungus epiphytotic (i.e. outbreak) a change very rarely seen internationally. There had only been one such epiphytotic in India, in the Godavari

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at Burdwan, Berhampore, Bankura, Suri, Krishnagar, Jessore, Gosaba, Faridpur, Rangamati, Comilla, Varisal, Kishoreganj, Mymensingh, Pabna Dhanbari, Bogra, Jamalpur, Rangpur, Maida, Dinajpur, Mainaguri, Charbadna. (Fisher, 1937) p 403

Each of these would have had research plots and demonstration plots including plots on private farms outside the research station itself.

<sup>73</sup> Surprisingly, to an economist, the pathologists do not mention the cyclone as a contributory factor.

<sup>74</sup> See however (Desai R. , Standard of living in India and Pakistan, 1931-2 to 1940-41,, 1953) who also takes into account the impact of bureaucratic 'corrections'.

<sup>75</sup> E.g. *'With regard to objectivity, forecasts based on human judgement, such as farmers' appraisals, tend to be conservative in that they are too high in poor years and too low in good years. That is, the appraisals reflect the average of past yields to too great an extent'* (Huddleston, 1978) p50. This suggests that the forecasts in 1942 would have been optimistic, seriously underestimating the effect of a strange disease.

Delta in 1918-19, and a handful elsewhere in the world (Padmanabhan S. , The Great Bengal Famine, 1973). Padmanabhan thought the epiphytotic was caused by the combination of an early season drought and abnormal weather conditions in the ripening season, when rice was most susceptible to fungus infection.

### Losses or damage?

Eye estimates of a growing crop concentrated on the outside appearance of the grain in a growing crop, and even crop-cutting would show only the volume of paddy produced. Virtually no farmer would have recorded weight at harvest. They would have recorded the volume, the number of bags taken from the field or the number of grain stores filled. The grain would only have been weighed when it was marketed. It is possible, just, that traders made decisions on weight per bag. (At this time the weight per bushel, then a measure of capacity rather than weight, was an important quality measure in US wheat marketing.) This introduces a further level of uncertainty about what the eventual yield of milled grain would be. Only after the crop had been dried out, threshed and milled would the full implications be noticed. There were reports in 1943 that not only was the crop much reduced, but that the milling percentage was reduced, with empty seeds, and some small grains within the seeds. Some grains so mouldy as to be obviously inedible. The palatability was poor. (Amery) These effects would not have been apparent until the paddy was dried, then milled.

Symptoms such as reduced number of tillers per hill, a reduction in root and shoot length, and a possible fall in panicle length may not have been noticed by crop reporters, and even if they had been, nobody knew what the effect was likely to be. While the number of grains per earhead might be halved as a result of a serious infection, this was obscured by the much increased weight of chaff per earhead, e.g. 4.6 grams of rice and 9.6 grams chaff per earhead for a healthy plant, and 2.1 grams of grain and 42 grams of chaff for a very badly infected plant with choking symptoms (Vedhyasekaran, Ramadoss, & Srinivasalu, 1973, p. 490).

The later studies show a perhaps half the number of grains per earhead, and only a slightly lower milling return, and that the apparently undamaged grains might have had 25% - 35% less starch and 75%-91% less sugar than uninfected grains (Vedhyasekaran, Ramadoss, & Srinivasalu, 1973). There was substantially more protein in the damaged rice, but it is not known how the changes in amino acid composition would have affected digestibility and nutrition. This suggests something between 20% and 28% fewer calories per hundred grams from rice produced on severely or very severely affected plants, a very serious bias when the nutritional quality of the rice is critical. The storage losses of infected grains have not been assessed

There have been some attempts to quantify the effects of serious infestations in

later years (Padmanabhan, Raichoudhury, & Ganguly, 1948; Vedhyasekaran, Ramadoss, & Srinivasalu, 1973), but of course it was not possible to replicate the climatic stresses so these infestations were not on the scale of the epiphytotic year of 1943, and may have been different in kind. The disease has been brought under control by breeding for resistance and by sprays, which, again, means that it is no longer possible to quantify the effects.

The Famine Inquiry Commission (p33) mentions that they had been informed by several people and by the Government of Bengal that the losses due to the fungus were more than those due to the cyclone, and this statement has been mentioned elsewhere.



## APPENDIX 2: SPECULATION

A myth popular at the time of the famine, and repeated by Sen, was that speculators removed a substantial amount of grain from the Bengal market – perhaps one or two million tons – reducing availability, and that, as a result, people starved. This is not normal seasonal or inter-seasonal speculation where firms make money by stabilizing supply. Sen's main source, the Famine Inquiry Commission, produces overwhelming evidence that this did not happen, and the post-famine consensus agrees. Orthodox economists would be very surprised if it had happened. Mainstream economics places the onus of proof on any economist, administrator or politician who claimed that speculation caused a given famine. Hard theory and evidence are needed when the claim is so unlikely, and the number of deaths caused by acting on this belief when it is wrong is so high.

I had read speculation theory for quarter of a century before I wrote my criticism of Sen, and I had applied it. I find it difficult to believe that any agricultural economist would be ignorant of its existence and its broad approach. In this Appendix I apply the standard theory to the reality of the Bengal market, and show that no rational person would have attempted to do the speculation which would have caused a famine

The questions that arise are

1. Is it possible to create a credible economic model of speculation causing the Bangladesh famine? Answer: No.
2. Is the evidence available compatible with speculation causing famine? Answer: No.
3. Would it have been rational for the Bengal Government to concentrate their anti-famine policy on getting famine speculation stocks back on the market? Answer: No.
4. Did they in fact concentrate their anti-famine policy on getting famine speculation stocks back on the market? Answer: Yes.

I start by trying to create a model of how speculation might have caused a famine. I ask if rational people would have embarked on this, given the financial risks, the market risks and the risks to their own safety. I identify crucial assumptions. I consider the implications of the assumption that all the players in the market were evil, willing to kill a lot of people to make money, and the implications of the assumption that they were normal humans. I then look at the facts. I make use of the accepted theory of speculation.

## Normal speculation

We may first distinguish between normal commercial speculation and speculation that might cause famine. The mainstream economic models for commercial speculation might start with a single trader who must decide how much to buy, at what price, and at what date, and then how much to sell, and when to sell, in order to maximize profits. Factors that might be relevant would include perceptions of what supplies were 'available' within the market at the time of purchase, and perceptions of the probability of these changing, through import and export for instance. Beliefs in market supply available each month until the new crop comes on the market are relevant. The perception of future supply affects decisions: if the new crop is expected to be big, it may be rational to sell all stocks before the new crop hits the market, retaining the minimum working stocks. If the new crop is expected to be low, it may be rational to reduce sales sharply as soon as the trader gets the news, in order to carry over the maximum into the next season. Much of the profit comes from choice of selling date. The information available on prices, stocks and shipping movements is typically bad. Many of the decisions are based on rumours. The availability of credit is important: traders often have to sell a few months before they want to in a rising market because their credit has run out.

## How speculation might cause a famine

The speculation that might cause a famine is very different. The theory starts with very simple models, assuming, perhaps, that a firm, or cartel, has 75% of the market in normal times. It removes 25% of total supply from the market by destroying it, by exporting it, or by storing it until 1944 or 1945. The result is that prices rise sharply because there is an inelastic demand for food. The cartel intends that the increase in price they get is at least enough to cover its costs and it hopes that it will be much greater. All the sellers in the market, not just cartel members, benefit from the higher price – the non-members are 'free riders' (an important concept in speculation theory). If, however, the firm or cartel had only 10% of the market it would still have to withdraw the same 25% of the total supply from the market, to achieve the same rise in price. However, this increase in prices would then not cover the increased costs of buying in stocks. (Students are expected to produce models of the various possibilities mentioned here.) That is to say, the smaller the proportion of the market, the less probable it is that this speculation could be profitable.

There were in fact millions of firms operating in the Bengal rice market ('firms' being used here in the economic sense, including large farmers, landlords,

moneylenders etc. as well as the grain traders: anyone selling outside the village in fact. It is by no means certain that a firm or cartel could have had as much as 2% of the market, which makes it extraordinarily unlikely that anyone would have attempted to withdraw large quantities from the market. The market consisted of an urban sector, of up to 15% of the total market, and a rural sector of perhaps 85%. Traders in the rural sector included millions of farmers, as well as landlords, moneylenders, and local traders. Most of their normal business was transfers inside this sector. In some years they did export to urban areas, mainly Calcutta, and export to other provinces, in which case they may or may not have used the services of some of the urban traders in Calcutta. During the famine there unusually large amounts were shipped from the rural traders to Calcutta. The urban traders were procuring for the cities, and handling some of the imports and exports. The urban traders were, of course much more visible to administrators, city dwellers and academics, who appear to have believed that they had a larger market share than they did. I have come across nothing in the Bengal literature which sets down which firms might have been involved in speculation to create a famine, nor what market share they might have had.

Contemporary reports were that farmers, landlords, moneylenders and local traders saw that there was a high post-harvest price and believed that there was a serious shortage and that prices would rise throughout the season. Those who could do so sold enough at harvest to cover their immediate financial requirements, taking advantage of the high price. They then stored their remaining stocks and waited for prices to rise. They were, therefore, carrying out the normal within-season speculation. Farmers in many countries routinely speculate like this, but cash flow is a constraint: the high post-harvest prices in Bengal removed this constraint. One result was that the amount of grain hitting the market immediately after harvest was much less than the marketable surplus; it was reduced by both the fall in production and by the increased stocks held at village level. And this implies that it would have been extraordinarily difficult for any market manipulators to buy enough grain to create a famine.

### Imports and exports

The theory's initial model has an explicit assumption of a closed economy, with no exports from or imports to Bengal. With open borders, one would expect that when prices jumped in Bengal, trainloads of rice would start arriving in Calcutta within weeks, bringing down the prices and bankrupting the speculators. The economy of India, and its governing ideology, was based on free markets. Traders planning speculation in August 1942, say, would have expected imports to be easy.

When planning the speculation in, say August 1942, the cartel would have taken a view on how much in total was going to be exported and imported in each month until the end of the famine, and how much was likely to be imported as the famine

developed, and when the imports would arrive. If there were large imports the moment it became clear that there is a famine, they would lose a lot of money. We know that members of the trade were forecasting a famine from the cyclone on and demanding that Government arranged enormous imports. That is to say Government would have plans prepared, even if it did not believe the forecasts.

As it turned out, there was a constant change of national policies during the famine period, suspending free trade, introducing free trade, imposing regional free trade, then changing back again. Provincial policy, in neighbouring provinces, in surplus provinces and in Bengal itself, kept changing. The market area went from 'all India' to 'Bengal' to 'Bengal and neighbouring provinces' to 'Bengal' to 'Bengal and perhaps some provinces' with Australia coming into the reckoning eventually. This meant that rational Bengali traders kept changing their beliefs about total availability, about whether there would be imports and when the imports would arrive, which had a major effect on prices and the course of the famine.

### Organizing and managing the cartel

It is unlikely that any one firm would have the market share needed to make it profitable to create a famine, so it would have to create or join a cartel. Speculation theory considers problems in organizing and managing the cartel at some length. The cartel relies on agreements between members, which may not even be written down. They were certainly not enforceable in law, as they were dealing with an immoral and unlawful enterprise.

All members incur costs, both their normal trading costs and the purchase of extra rice stocks to be exported, hidden or destroyed, as well as assembly, storage and security – protecting the stores from looting by criminals or the general public.

The problem of Free Riders arises again. If a firm joins the cartel, it gets the higher market price, but it incurs the significant cost of buying rice to remove it from the market. If a firm refuses to join the cartel, it does not have to help buying the rice that is to be removed from the market, but it still gets the higher market price, so makes a much higher profit. It is then termed a 'Free Rider'. Indeed, if the Free Rider believes that the cartel is going ahead, it would be rational for it to buy more than usual and store it to sell at the higher price, further reducing the profit of the cartel. <sup>76</sup>

It may be argued that it is going to be a lot easier to set up a cartel when there

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<sup>76</sup> This is a development of the theory of the tragedy of the commons, where, for example, farmers who have the right to graze on common land all know that they would all benefit from applying fertilizer to the land, and that they would be better off still if they were the only farmer not to agree to pay for the fertilizer. While this phrase first appeared in print in 1833, the concept certainly was mainstream centuries older, having driven the enclosures, and was probably well known for millennia. It is used routinely in analysing African traditional agricultural systems for instance.

are only a handful of traders, and virtually impossible when there are hundreds. Practical problems, such as the hatred between different communities in Bengal at the time, suggest that it would be difficult to control a substantial part of the market.

Controlling the cartel is the next problem. For the plot to work, all members of the cartel must act in concert, in buying, in withholding from the market and in selling. There is no legal contract to enforce, because they are acting criminally, under wartime legislation. Each individual member would profit by, say, selling their *total* stocks including those held to create the famine, not just the agreed amount, when the famine prices reach their peak. If one of a three-member cartel did this, there would be a sharp fall in prices, which would mean that the others lost money from the venture. If one member of a ten-member or fifteen-member cartel did this, they would certainly make much more money. If all the other members stuck to the agreement, they might have a reduced profit, but still a substantial profit. However, if they thought it likely that one of the remaining members was going to break the bargain and sell off everything, each would decide that it was in their interest to sell everything immediately, before anybody else did, and the market would collapse – with 25% of normal sales dumped on the market over one or two months! Anybody who hesitated would be bankrupted. There are many models of similar situations coming from various sources, including game theory, the theory and practice of agricultural cooperatives, speculation theory, monopoly theory, and marketing economics.

Each member of the cartel knows that the other members are criminals, willing to kill hundreds of thousands of people to make money. Why would they trust them? On the other hand, who would dare to double-cross people like this?

#### Information and uncertainty.

Any cartel in Bengal in 1942-43 was not operating under perfect information.

The cartel could reach a consensus on the probable crop. This would be more reliable, but not necessarily more accurate, than the official crop forecast. The consensus would change as the crop reached maturity. The consensus might differ from the consensus of competitors, and it will certainly not turn out to be 100% accurate.

There was no information on quantity being stored. There would be rumours buzzing round in the trade. The cartel would know, though, that their own actions, buying aggressively, would be the source of many of these rumours – what is now called a feedback loop.

There was no possibility of calculating demand elasticity with the information available. This and the lack of production and import information meant that cartel members could only guess what effect taking a million tons off the market would have.

Would this just have reduced supply to normal? Would it result in a major famine? In one case the members would have been bankrupted. In the other, risks discussed below would have arisen.

### Non-Financial Risks

Risk is a cost. While the literature does discuss some of the financial risks, the non-financial risks are more important.

Creating a famine would have been highly dangerous for the firm or cartel. Both the general public and the authorities would have been hostile. Wartime Bengal was on the front line, with invasion imminent. In August 1942 the Japanese had destroyed all Ceylon's defences and was about to make an unopposed occupation, as the first step in an invasion of India.<sup>77</sup> The Japanese bombed Calcutta later in 1942. After what had happened in China and Burma, the whole population was afraid. Few people believed that the Allies would win the war. Much of Bengal was simmering on the verge of rebellion, and conflict between Hindus and Muslims was about to burst into massacre and ethnic cleansing, so the authorities could not tolerate anything that might upset the population. Accordingly, anybody who was found to have given false information on stocks, let alone to have conspired to have created a famine, would have been prosecuted. At the time, conspiring to create a famine might have resulted in 10 to 20 years' rigorous imprisonment. Their total stocks would have been seized, with very little compensation, if any.

There are other risks, more serious for the cartel members, but also faced by traders who did not join the conspiracy. In a famine, there is usually a hostility to traders, and claims that they are deliberately creating the famine. The owner and staff may be lynched. Their families may be lynched. There may be riots aimed at the owner's co-religionists or members of their race. In Sierra Leone I found that this meant that traders were not willing to trade even on behalf of Government in a food crisis, however profitable it may be: it was just too dangerous (Griffiths, 2003).

There may be food riots, with the mob breaking into grain stores and stealing the contents. In some of the rural areas of Bengal, bands of decoits ran the district, with police having to abandon their posts. In 1943 the government searched for the rumoured massive grain stores. They did not find them, just seasonal storage at lower than usual level, but grain was still seized, for propaganda purposes. The army, engaged in distributing food to the starving, sometimes broke open local merchants' stores and told the local population to help themselves.

The non-financial risks, often ignored in the literature, would themselves make it

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<sup>77</sup> This was called off the night before the invasion, with the aircraft carrier diverted to Midway to mop up the battered remains of the US Navy. Due to one of those small accidents that change history, it did not quite happen like that.

extraordinarily unlikely that a famine would have been deliberately created by speculators.

### What happened to the stocks?

It would have been impossible to hide what was happening. The stocks removed would have been exported, stored or destroyed.

Exports were very public (the food authorities and Special Branch were monitoring the docks and the railways), and it would have been noticed by everybody in the trade that the cartel was buying at a very high price, paying the costs of assembly and transport, and selling at a much lower price – and this at a time when all other traders were scouring neighbouring provinces for rice, and smuggling it back to Bengal.

Removing two million tons from the market and storing it through 1943, then putting it on the market in 1944 was not an option and did not happen. An extra two million tons would have caused the 1944 prices to collapse to a very low price indeed. This would have been noticed, and questions would have been asked, with the possibility that the survivors lynched the traders, or the government prosecuted them. The implication is that the stored rice was left to rot over the next few years, which would have been obvious to the neighbours from the smell.

There was little possibility of traders hiding the enormous stockpiles needed to create a famine. A country at that stage of development does not have large, empty, stores in which they can be hidden. Transport was a constraint and impossible to hide (much would have been by oxcart). Thousands of people would have been employed building up the stockpiles. Millions of people, consumers, would have been aware of what was happening.

Those traders who warned of a famine in 1942, though it was against their personal financial interest to do so, would certainly have known what was happening and would have informed the authorities. Most traders would know what was happening, certainly any that had been approached to join the cartel. Nearly all of them, I suspect, would wish to prevent the famine. The cartel would need to borrow enormous sums of money to buy the extra stocks they were withdrawing from the market. Banks would not have lent to fund an extremely high risk, highly illegal, and publicly and politically unpopular scam. In 1943, the banks were, of course, questioned by the food authorities and Special Branch, who were trying to find the stocks. The banks did not report any unusual loans. Wartime mail censorship and informers would have identified the cartel stockholders.

Inevitably there would have been leakage of information by the staff of cartels, from the owners down to security guards. And the more firms in the cartel, the more

leakage there would have been. In 1943 the food authorities offered rewards for information about hidden stocks.

In addition to the knowledge that famine speculation would have been irrational, we *know* that it did not take place. There is no evidence of massive, loss-making exports from Bengal, rather that after October 1942 Bengali traders scoured the surrounding provinces to buy any rice available, paying high prices, and that this rice was smuggled into Bengal. An enormous amount of effort was put into finding the two or three million tons removed to cause the famine – not easy to hide. The Civil Supplies Department used the full resources of wartime legislation to search for such stocks. Wartime mail censorship, examination of the bank accounts of traders to see if they were borrowing enough for massive purchases, Special Branch investigations, paid informers in the docks, and rewards for information found nothing. The wartime legislation, Defence of the Realm Act, etc provided powerful means of dealing with people who deemed a threat. Raids on traders showed only that traders had less in store than at the same date in previous years, implying that they were involved in normal speculation. Some of these traders had their stock seized for propaganda purposes. There was never any sign of how the mythical millions of tons of famine-speculation rice were disposed of – if it they had been put on the market in the next few years, the markets would have crashed. If they had been destroyed, thousands of people, relatives of the dead, would have known.

Since there was no possibility that the plot could have been concealed, the financial and non-financial risks of creating a famine were prohibitive.

### Protection Money

It follows that there could have been no possibility that the cartel would have gone ahead without paying protection money.

At local level the police and local politicians, and the local mafia would have to be paid off to prevent lynchings, riots and looting.

The police and food department would have to be paid off when they searched wholesalers' premises – even innocent traders had their stocks confiscated.

The soldiers, who were distributing food to the starving, were very angry at what they saw – they gave their own rations and any military stores they could steal to the starving.<sup>78</sup> The high command, up to the Chiefs of Imperial General Staff, and Field Marshall Wavell, when he became Viceroy, believed that the famine was not only a

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<sup>78</sup> Brown (1944) observed Indian and British soldiers voluntarily going on short rations to feed the starving, and suggests they may have stolen some military supplies. Stevenson (2005) says, less convincingly, that they stole so much of the military stocks that the army would have had great difficulty in resisting an invasion or in attacking Arakan.



humanitarian disaster but a military catastrophe which could change the outcome of the war. It was inconceivable that they could be bribed. While one or two might have taken bribes, it would have been impossible to turn everyone, including Indian officers and men from different provinces, and British, Australian, African, American and other officers up to the Viceroy, a former field marshal in charge of the 14<sup>th</sup> Army.

For such a major scam the cartel would have had to bribe politicians and administrators up to cabinet level. It would have had to bribe the leading parties – there was a change in the administration in April.

It would also be necessary to bribe Indian Civil Service administrators and the Viceroy himself.

The various changes in food policy including suspension of the free market and refusal to export to Bengal were necessary to sustain the famine. Are we to take it that this meant bribing the Food Conference delegates, the Viceroy, Churchill, the representatives of the exporting states, the first minister of Punjab, the Bengal Food Department etc.?

#### Policy Prediction

The famine speculation requires a closed border: if it is easy to import, the import parity price is the highest price that can be obtained. Imports by government can be sold cheaply or given away. The low prices that follow may bankrupt the speculators. From the outbreak of war, the Indian and provincial governments played an increasing role in the rice trade, setting maximum prices and, by May 1942, controlling, then taking over all inter-provincial trade rather than letting traders do this on their behalf. It was clear that policy was changing frequently and unpredictably. For example, the policy of free trade within India switched to Indian government control of all inter-provincial trade, then to free trade within a region, then back again. The provincial government might set a maximum price within a district, and forbid private trade from that district, then change the regulation a month or two later. Speculators could have no idea how much would be imported or when – a business risk which few would take.

Once the trade had begged the Bengal Government to import, from October 1942 on, there was always a probability that very large imports, probably half a million tons within six months, would begin sooner than later. Any speculator would aim to cover costs before this.

What was observed was very different. In December 1942 the Bengal Government made an announcement to a national Food Conference, which was widely interpreted or reported as saying that Bengal had 'a modest surplus', an announcement which had catastrophic consequences. The Famine Inquiry Commission p 46 claims that they actually said was that in view of the emergencies elsewhere in

India, Bengal would manage without imports for five months, but would need substantial imports then.

It was agreed that surplus provinces would supply rice to Calcutta, but very little of the promised supply was actually sent, partly because of a widespread belief that Bengal had enough food and that speculation, hoarding, inflation, etc were what were creating high prices and hunger. The Viceroy personally believed this and continued to do so until he retired in August 1943<sup>79</sup>, so it was the de facto Indian Government policy. Surplus provinces were reluctant to supply Bengal until August. The Bengal Government based their policy on this belief. They refused to borrow money to import rice, though the money could easily have been raised (Famine Inquiry Commission, 1945a). When they did agree to import, The Bengal Minister of Food, Surawady, gave his friend and political ally M.A.H. Isphani, who had a large grain trading business, the monopoly for imports: this was highly profitable, as long as there continued to be a shortage, so there was no incentive for massive imports.

The Imperial War Cabinet and the controllers of allied shipping declined to allocate shipping to Indian food imports, on the grounds that the Battle of the Atlantic was at its peak, and Britain needed every ship it could get for survival, though, with hindsight, we can say that the battle was won in March. After this, the belief was that every ship was needed to supply D Day and the attack on Germany – had this failed the war would have been lost. Such decisions are easier to make if one chooses to believe that India and Bengal had enough food, and the rumour was certainly prevalent in Whitehall. Eventually a minute to the Imperial War Cabinet from the Chiefs of the Imperial General Staff saying that the famine was a military disaster as well as a humanitarian disaster tipped the balance, but the food arrived late.

The failure to import was entirely unpredictable and nobody would have embarked on a famine-causing speculation in the hope that this would happen.

It also had its effect on normal speculation. Traders would plan their sales on the expectation that large imports would start by, say, April. Every day's delay after this date that the amount of food per head per day for the rest of the year fell. So prices continued to rise. This would explain the otherwise inexplicable pattern of rice prices through the famine year.

A different form of price instability emerged during the famine. The Bengal Government, from time to time tried to buy large quantities of 'surplus' rice on the market. As this did not exist, the only effect was to push up prices. The Government then tried other, equally unsuccessful measures. These random price movements were impossible for speculators to forecast. No speculator would have taken them into account in 1942.

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<sup>79</sup> On 19 October 1943, when the famine was serious, the new Viceroy, Wavell, noted in his journal "On the food situation Linlithgow [The outgoing Viceroy] says chief factor morale." Moon, 1973 p34

It is concluded that there is no possibility of a rational speculator trying to create a famine. The facts are not compatible with enormous amounts of rice being removed from the market.

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<sup>1</sup> The 1941 Census estimated total urban population of Bengal to be just 3.7 million out of a total population of 60.3 million (Government of India, 1946), but this figure was believed by the Famine Inquiry Commission and the Department of Civil Supplies (Pinnell (1944) and Braund (1944)) to have grossly underestimated the urban population. Food requirements were worked out for the population of Greater Calcutta, over six million. There was immigration and emigration between the census date and the famine, including, for example, perhaps 300,000 refugees arriving from Burma, military personnel arriving after the census date and refugees leaving Calcutta and Bengal especially when invasion was imminent (before the battle of Midway) and after the Japanese bombing of Calcutta.