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Famine falsehoods and publication ethics: rejoinder to Daoud and the Journal of International Development

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Famine Falsehoods and Publication Ethics: Rejoinder to Daoud and the Journal of International Development.

INTRODUCTION

I published an article on falsehoods and myths in famine research (Bowbrick P. , Falsehoods and Myths in Famine Research – The Bengal Famine and Daoud, 2022a) using Daoud (2018) for examples. A year later the editor of the Journal of International Development, invited me to write a Rejoinder to a Response submitted by Daoud, and this Rejoinder passed the refereeing process. The editor, Åshild Kolås, a non-economist, then insisted that I cut out all mentions of falsehoods, in spite of the fact that the subject of the paper I was defending was falsehoods. She also cut out other statements and evidence. She said that if I did not accept changes, the Journal would make changes and publish them without my consent. I believe that if I had let this happen I would have been acting fraudulently, knowingly and deliberately publishing falsehoods, which would have caused a large number of deaths. I would have been an accessory. Accordingly, I withdrew my Rejoinder from the journal. The Journal published his response anyway (Daoud, 2023). This paper is a rejoinder to Daoud, with an analysis of publishing ethics issues involved, using the criteria of COPE (The Committee for Publication Ethics) – the ethics body of the academic publishing industry.

1. FALSEHOODS AND MYTHS IN FAMINE RESEARCH

I use the words ‘false’ and ‘falsehood’ as attributes of a statement, including being ‘contrary to fact or truth’, ‘being untrue’, being ‘contrary to what is true, erroneous’. ‘Falsehood’ is not an attribute of the person making the statement. The word does not imply misconduct: there are innocent ways in which falsehoods can creep into one’s work. I distinguish between falsehoods misrepresenting the facts and failure to mention facts which do not support or refute an argument. A single falsehood may invalidate a section of a paper, the paper as a whole, or a research programme.

The literature on famine is contaminated by fraud and error, falsehoods and myths. Only when these have been identified and removed can we have a rational discussion of famine. Bad economics can kill millions, so absolute rigour is required. I have taken one extremely influential research programme, that on the Bengal famine of 1943, as an example, and I examine a recent example, Daoud’s paper, ‘Synthesizing the Malthusian and Senian approaches on scarcity: a

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realist account' (2018) which presented an empirical model of the Bengal famine of 1943, partly because it had a large number and range of falsehoods, and partly because it drew heavily on falsehoods in other papers in the research programme. My objective was to identify falsehoods, not to cast blame on anyone, or, indeed, to identify who first introduced the falsehoods. I have been careful to concentrate on the evidence and to exclude anything that could be construed as a personal attack or an insult.

My paper 'Falsehoods and Myths in Famine Research – The Bengal Famine and Daoud' (2022a) and this Rejoinder are about the use of falsehoods and myths in the economics of famine. Daoud's (2018) paper presented an empirical economic model or case study of the Bengal famine of 1943. I showed at length that many of his factual statements in this model were falsehoods, either misrepresenting the facts or not mentioning facts which did not support or refuted his argument. Therefore, his analysis was wrong, and his model refuted.

Critical comments like this are of fundamental importance to the experimental and social sciences. They expose bad economics, so we can replace it with better. Without them, economics would be static. We would be drowned in bad economics. Critical comments also discourage people from research misconduct, producing careless, sloppy or just fraudulent research. RetractionWatch.com lists more than 30,000 academic papers that have been retracted for falsehoods, usually a handful falsehoods rather than the very large number in Daoud's paper.

In the past, academics observed that if there was serious criticism of their research, they could demand the 'right to reply' ensuring that, at worst, they got two citations and one publication. They could write on any subject they liked, quite unrelated to the criticism, and deflect the readers' attentions from their failings. They could also include personal attacks on their critics. And, since their work had been shown to be wrong, they had little to lose from producing new falsehoods. A few years ago, it became apparent that this was seriously damaging the integrity of research. Some journals took action themselves: The American Economic Review, for example, now says 'There is no automatic right to Reply; the author of a Reply must provide substantive and material discussion of the issues in question'. Daoud produces none. COPE, the Committee on Publication Ethics set up by the academic publishing industry, published guidelines on what was ethically unacceptable, suggesting that journals should not publish replies or responses that broke these guidelines (COPE Council (Committee on Publication Ethics), 2021). These guidelines must be taken as the minimum standards: through a long career I have found that most of my colleagues had stricter standards – we were not influenced by the financial interests of publishers, but we were acutely aware of the harm caused by ethical failures.

COPE (2021) requires that replies and responses

- Must be on the subject.
- Must be supported by evidence.
- Must focus on the substance of the article it is responding to.
- Must have evidence or data to support the claims made.

A single failure in any of these is considered such a serious ethical offence that an ethical journal is expected to refuse to publish the paper. It is a matter of concern that the Journal of International Development published Daoud's Response, especially as its publisher, Wiley, played

a key role in drafting this advice with COPE and the journal editor had asked Wiley for advice in handling the Response and Rejoinder.

Daoud has now written a Response, (Daoud, A Theory of Famines - a response, 2023). If Daoud had stuck to academic culture and COPE guidelines, this Response and my Rejoinder would be a normal academic exchange using hard evidence and rigorous analysis, perhaps with him challenging my identification of falsehoods, and with me defending it, perhaps with both of us accepting that the other had made good points and that we were wrong on some or all points. Unfortunately, the response broke most of the COPE guidelines, so this discourse is impossible. I have had to adopt one I would have preferred to avoid. I have had to bring 'falsehoods in famine research' back to central stage, but ignoring his previous falsehoods and discussing his new falsehoods, and I have had to discuss publication ethics.

2. DAOUD'S FAILURE TO RESPOND TO THE CRITICISMS

In his reply, 'A Theory of Famines – a Response' (2023), Daoud has not even attempted to challenge the evidence that his paper was full of falsehoods and myths. This is a tacit admission that he has no defence.

If he had any defence, he would have taken each instance I claimed was a falsehood and attempted to show, with solid evidence and rigorous analysis, that I was wrong. He has not done so. It is now more than two and a half years since I sent Daoud my criticisms, so he has had ample time to check all my sources and analysis.

In a very few cases, he asserts, without evidence or argument, contrary to evidence in his sources, and contrary to the evidence I had produced, that I was wrong. I discuss these assertions in a later section. An assertion without evidence and analysis is not new evidence or argument. He introduces new falsehoods. He repeats old falsehoods, such as statistics which I had shown that the Indian statistical profession had dismissed in the strongest possible words three quarters of a century ago, without mentioning their criticisms, let alone attempting to counter them. He produces new falsehoods. Clearly, he has found no possible way to challenge my criticisms.

3. DAOUD'S 'HASAS APPROACH'

Daoud's response is mainly on an irrelevant subject that Daoud admits I do not mention – his personal 'HASAS approach' which he now calls his 'SAS approach' or his 'theory of famines'. This is a diversion, a distraction: it draws the reader's attention away from the fact that I have shown that his paper is littered with falsehoods.

COPE is clear that any response must be on the subject: if it is not, it should be rejected. It is worrying that the Journal of International Development published this response.

In a later section I give reasons why it would be irrational to use his theory.

4. DAOUD'S EMPIRICAL MODEL

My paper was on falsehoods and myths in famine research as its title says. I said,

'the researchers in the field have continued to use the myths, the falsehoods and the theory three quarters of a century after they were refuted. This paper examines a recent example. I show that his statements about this famine and his use of the literature are falsehoods. . .'

Many of these falsehoods are not new: the false information and bad economics appear in the literature, especially the literature of the last 45 years by Sen and his followers, and it is not surprising that Daoud should have been tempted to use them. It is surprising, though, that he should have ignored the criticisms and refutations of them in the other literature he cites.

Daoud (2018) presented his own empirical model:

'an actual case as an empirical example to analyse the above questions' ... To make the analysis more concrete, I will focus on the Bengal famine of 1943' (Daoud, 2018, p. 454) 'I used the Bengal Famine of 1943 as an empirical case study, showing how my theory can be applied.' (Daoud 2023). 'the empirical case—the 1943 Bengal famine—that I used to exemplify my theory.' (Daoud, 2023, p. 1)

My paper (2022a) showed that this empirical model was wrong: it had falsehoods and myths throughout. I refuted it many times over. It is fundamental to economic method that an empirical economic model (combining fact and theory) that is consistent with falsehoods is wrong, and one that is consistent with the large number of falsehoods I identified has zero credibility. COPE, the Committee on Publication Ethics, states,

'editors should consider retracting a publication if:

- They have clear evidence that the findings are unreliable, either as a result of major error (e.g., miscalculation or experimental error), or as a result of fabrication (e.g., of data) or falsification (e.g., image manipulation)'*

Most of the falsehoods and myths I identified in Daoud's paper are to be found in his model. If the facts of a model do not fit the theoretical economics of the model, either the evidence or the model is wrong. This implies that the theory he used, or the way in which it was used, was so bad that it did not force the author to recognize that the facts were wrong. Daoud now appears to have recognized that his 'empirical model' is indefensible and decided to concentrate on his 'SAS theoretical approach'. He makes no attempt to challenge my facts with evidence and analysis. but almost immediately states, *'the conceptual validity of my theory does not even hinge on that particular case'* (Daoud, 2023, p. 1). It is not clear how this squares with his statements *'the empirical case—the 1943 Bengal famine—that I used to exemplify my theory. ... the empirical evidence I have used to develop my theory.'* (Daoud, 2023, p. 1) *'In the Daoud (2017) article, the event of interest is the 1943 Bengal famine.'* (Daoud, 2023, p. 2).

5. 'CONTRADICTIONARY STATEMENTS'

Daoud does return to his model of the Bengal Famine in his very short section 'Contradictory Statements' at the end of his Response, producing an extraordinarily inaccurate summary of my refutation. He produces one or two sentences for each point he makes, enough to produce more falsehoods, but not enough to challenge any of my refutations: that would take evidence and analysis, as the American Economic Review and the Committee on Publication Ethics state.

Daoud makes no mention of the falsehoods I identified – he suppresses my criticisms. When he repeats a falsehood that I identified in his Response, he is therefore producing two further layers of falsehood, in addition to the *those* of his 2018 paper. Suppression of inconvenient evidence is a very powerful form of falsehood – for instance, if enough facts are suppressed the Flat Earth theory is self-evident and unassailable.

I identify other falsehoods he produced below. If some of the issues and falsehoods he produces appear to be trivial, it is because they are: it is a form of diversion and distraction. The more time readers spend on trivial issues, the less time they spend on important falsehoods. COPE considers that triviality of issues is grounds for rejection or retraction. (COPE Council (Committee on Publication Ethics), 2021)

4.1 Data uncertainty

Daoud claims '*I thoroughly discuss how data uncertainty restricts my analysis (see e.g., Section 3.1 in Daoud, 2017).*' In fact, he had done nothing of the sort. Much of my paper was showing that in his paper he had suppressed the large amount of evidence in his sources which showed that the uncertainty was vastly greater than he says in this section. I mention below some fifteen causes of this data uncertainty which he has suppressed.

Daoud claims to compare different food supply forecasts to show the range of uncertainty. He states,

'Table 1 summarizes the food supply data presented in four different studies. These studies were chosen because they provide either original data or innovative re-interpretations of data.' (p. 461).

He does not mention that I had discussed at length the falsehoods in this statement (Bowbrick P. , 2022a, Section 6 especially pp9-10). He makes no attempt to challenge my evidence. This is a new level of falsehood, repeating falsehoods without mentioning the criticisms.

I showed that he was quoting evidence that supported his personal beliefs, and failing to mention information that contradicted them. Most obviously he compares four estimates using neither original data nor a range of data sets, but all using the same crop forecast, based on the observations of the chaukidars, semi-literate village watchmen. He fails to mention all the other evidence, including crop forecasts which suggested much lower rice production, perhaps as low as half the chaukidar forecast. The different estimates of 'availability' he cites were produced by weighting the chaukidar crop forecast with guesses on other supply factors – I discussed (2022

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p12) the lack of hard evidence for these 'estimates of availability'. Again, the falsehoods of his paper are not mentioned in his reply.

One of these four estimates was produced by Goswami (1990). While Daoud cites Goswami repeatedly, he does not mention his conclusions, which give a very different story:

'Therefore, according to our estimates, aggregate per capita food grain availability in 1943 was significantly less than in 1941. To summarize:

- *Using the 1 lb. norm and the calendar year basis, per capita food availability was 6.1 per cent less in 1943 compared to 1941.*
- *Using the 1 lb. norm and the September-August basis per capita food availability was 7.1 per cent less in 1943 compared to 1941.*
- *Using the 1.05 lbs norm and the calendar year basis, per capita food availability was 12.1 per cent lower in 1943 compared to 1941.*
- *Using the 1.05 lbs norm and the September-August basis per capita food availability was 13.8 per cent lower in 1943 compared to 1941.'*
(Goswami, 1990, p. 457)

That is to say, Goswami concluded that Sen's estimate overstated aggregate per capita food grain availability by between 18% and 29%, which is too much to ignore: it refutes Sen and Daoud. The shortfall identified by Goswami might have been enough to tip Bengal from a food crisis to a famine even if one makes the false assumption that the chaukidar survey was meaningful. It is surprising that Daoud did not mention this as he cited Goswami, and Goswami is cited in the research programme purely because of this result. Again, important evidence which refutes Daoud is omitted.

This discrepancy, enormous though it is, is much smaller than the true discrepancy. Goswami was using the chowkidar rice crop forecast which was 'meaningless' according to the Indian statistics profession and which was thought to overstate the rice crop by more than a third by the Department of Agriculture, for instance – while wholesalers thought that it overstated the crop by much more. I discussed the evidence at some length (2022a). The two overstatements, taken together, indicate a crop failure big enough to cause a famine, possibly with a significantly higher death rate than some researchers have estimated.

My paper showed that the only evidence that gives any support at all to Daoud's claim of adequate supply is the meaningless chaukidar crop forecast (Bowbrick P. , 2022a, p. 10). The views of the Indian statistical profession – which included the world leaders in small-farmer production statistics – were that these statistics were,

*'useless for any purpose'¹, 'not merely guesses, but frequently demonstrably absurd guesses'²
'a farce . . . a fraud'³ 'blatantly absurd results'⁴, 'disbelieved by the very government that*

¹ Bowley and Robertson (1934, p. 35) quoted by Dewey, 1978.

² (Royal Commission on Agriculture in India, 1928, p. 605) quoted by Dewey, 1978.

³ (Dewey, 1978, p. 290)

⁴ (Dewey, 1978, p. 298)

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*produced them*⁵ *'no meaningful production statistics'*⁶ *'not only incorrect but absurd'*⁷
*'produced by a system inherently vicious'*⁸

The Department of Agriculture, which produced the chaukidar forecasts, believed that the 1942/3 rice crop forecast overstated production by one third. The rice traders said that the overstatement was even greater. I can find no evidence of anyone defending the chaukidar forecasts then or later, so it is alarming that they are routinely used in discussing the famine.

There was an enormous fungus attack on the rice crop in 1942 caused by disease brown spot of rice, *Helminthosporium oryzae*. See in particular Padmanabhan (The Great Bengal Famine, 1973), Das (1949), Desai [1953], Tauger (2006; 2009), and Bowbrick (2020; 2022a) for reviews of the literature. As this was unprecedented, people could only guess at what effect it was going to have on yields, though there was general agreement that it caused more damage than the 1942 cyclone. It was two or three months after harvest that the impact started to be realized. Yields in infected areas were dramatically reduced (Padmanabhan S. , 1973), so famine was inevitable in the worst hit areas. When the grain was milled, some months after harvest, it was found that there were fewer grains per ear, and smaller grains, the milling output was lower, and the nutrition was lower (Padmanabhan, Raichoudhury, & Ganguly, 1948; Padmanabhan & Ganguly, 1953). Traders then found that a lot of rice was so badly damaged by the fungus as to be inedible (Amery, 1988). This was obviously impossible to quantify at such a late stage - we cannot know whether the edible rice crop was reduced by quarter of a million tons or three quarters of a million. We know that even if the chaukidar survey had been perfect in all other respects, it would have overstated the rice crop. Other forecasts also failed to deal with this. This means that the chaukidar crop forecasts for 1941 and 1943 are not comparable, but Daoud still compares them.

However, Daoud uses only these chaukidar forecasts in his paper and his 'Response': they are the only evidence that give any support to his beliefs on supply. So he uses only statistics that he knows to be wrong, and he fails to mention all the evidence that they are wrong. He also suppresses contradictory evidence. Rigour, and the COPE guidelines require that he mentions the contradictory evidence, however inconvenient.

Daoud compares the chaukidar forecasts for 1941 and 1943 to support his claim that there was more food available in 1943, which is fundamental to his whole argument. This claim requires that the chaukidar forecasts for both 1941 and 1943 are 100% accurate, though the evidence is that they were known to be meaningless at best even before the famine.

He fails to mention the fact that Desai [1953], for example, compares the official estimates of such crop forecasts for jute with the results of scientific surveys carried out by Mahalanobis, which used crop cutting to get the actual yield, rather than eye estimates. Desai shows that the discrepancies are large, with survey estimates being between 47% and 153% of the amount

⁵ (Mahalanobis, 1943)

⁶ (Bengal Land Revenue Commission, 1940, p. 76)

⁷ (F.H. Villiers to Sir Edward Grey, 1914, quoted by Dewey 1978 p284.)

⁸ (Trevaskis, 1931, p. vol 1 p 200) quoted by Dewey, 1978.

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delivered to the factory. This means that the margin of error for Daoud's claim that there was 11% more food available in 1943 than 1941 is off the scale.

He fails to mention the evidence that in 7 of the previous 30 years, including 1941, the chaukidar forecasts produced frequent false alarms – warnings of crop failures when the crop was adequate. Mahalanobis (1943). This shows that the forecasts were unreliable. Mahalanobis suggested that this was why the authorities ignored warnings of a short crop in 1942/3. It also means that it is impossible to compare the 1941 and 1943 crop as is fundamental to Daoud's argument, and Sen's. (Mahalanobis, 1943, p. 4; Bowbrick P. , 2022a, pp. 10,11). The forecasts for these two years are not comparable. This might mean that the real availability in 1941 was 10% or 20% higher than the figure that Daoud uses – this alone invalidates his argument in its entirety. This refutes his claim 'I thoroughly discuss how data uncertainty restricts my analysis'.

He suppresses the fact that the 'availability' calculations – to get from crop yields to supposed grain availability per head – were meaningless, based largely on invented figures and arbitrary assumptions (Mahalanobis, 1943; Bowbrick P. , 2022a, p. 12).

It is very odd that no discussion of this evidence appears in the Response. Rigour, and COPE guidelines, require that authors should cite the evidence that disagrees with them and explain why it should not be believed. It is not acceptable for them to just keep on repeating what they believe: 'what I say three times is true' is not an acceptable argument.

The true crop yields could have been perhaps one and a half or two million tons less than the chaukidar forecast which Daoud uses, and there could be an even greater discrepancy in 'food availability' estimates. His claims on reliability are based entirely on guesses, and his claims that there was a range of accuracy of a few percentage points is clearly false. His claims on reliability must be rejected in their entirety. Throughout this section of his paper, he produces falsehood after falsehood.

This means that any conclusions drawn by Daoud from his analysis are false.

4.2 Bowbrick's 'contradictions'

Daoud states,

'Conversely, Bowbrick is making a series of contradictory statements. On the one hand, he writes, "The raw data were known to be meaningless." (Bowbrick, 2022, p. 9) (see in particular his Section 6.3 titled The availability estimates are also meaningless). On the other hand, he is absolutely certain about his claims that "there could not have been plenty of food" in Bengal. How does he know that "there was no plenty of food," given that he also knows that the data is meaningless?'

There are many falsehoods here. Yes, the statisticians thought the raw data for the chowkidar rice crop forecast was meaningless, but I and they most certainly did not say that all the other data is

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meaningless, as Daoud claims here. I identified other meaningless data, such as the price statistics, and the 'availability' calculations and I pointed out problems with other data. I did not use the meaningless data.

I did not suggest that the forecasts of the traders and of the Department of Agriculture were meaningless. On the contrary, they were based on observations of the growing crop by experts. They were not, however, based on statistical surveys, which means that I treated them with caution.

The 'availability estimates' - for food (not just rice) production – would be meaningless even if the rice crop forecasts had been perfectly correct, because they use guesses or arbitrary assumptions on other factors of supply to manipulate the raw data into 'availability' figures. They also leave out many foods. I am surprised that someone who states '*I thoroughly discuss how data uncertainty restricts my analysis*' (Daoud, 2023, p. 5) should have difficulty with this.

I was careful to report a range of crop forecasts, and to point out problems with them, most obviously the failure to account for damage caused by the fungus, but Daoud suppressed the problems. I certainly do not claim, as Daoud does, that any of the figures were 100% accurate or anything like it. On the contrary, all my statements on deficits, for instance, emphasised the large range of possibilities. Since the errors with the forecasts are so high, one is tempted to quantify the deficit needed to explain the starvation observed, but the data here are even worse. On the issue of 'no plenty of food' (sic) I have shown the reasoning over the last 36 years starting with a paper that Daoud has read and cited (Bowbrick P. , 1986). He has not challenged this reasoning, nor has anybody else.

'Then, he [Bowbrick] adds that any crop forecasts are meaningless, including that derived from crop production, crop forecasts, and the estimated impact of the fungus outbreak in 1943 (see his Section 6).'' (Daoud, 2023, p. 6).

His indignation at this is surprising: if the data we would wish to have are unreliable we must not be indignant that someone said so; we must change our analysis. Again, I said something very different,

'Contrary to Daoud's repeated statements, there were no statistics on food production, rice production or food availability. There were rice crop forecasts, which are inevitably much less reliable than measurements of production, and none of the crop forecasts had any statistical credibility.' (Bowbrick P. , 2022a, p. 8)

I did say '*none of the crop forecasts had any **statistical** credibility*' because the credible raw data of the agricultural officers and the traders were not collected from a statistical sample and because the aggregation procedure will have caused errors (as does the aggregation procedure used on chaukidar data), but this does not mean that they are useless, far from it: it means that the forecasts have a broader margin of error than we would wish. None of the forecasts could possibly have accounted for post-harvest losses due to the fungus. Very little of what we know about the world has statistical credibility, and indeed statistics may have a very low sampling

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error even when the observations were fabricated. There were no crop forecasts 'derived from crop production': that would be an oxymoron. The first survey of rice *production – what was harvested* – was done for the 1943/44 crop, after the famine. The Indian Statistical Institute's production statistics became the norm after this because they were credible and statistically reliable.

Daoud asks, 'How does he know that "there was no plenty of food," given that he also knows that the data is meaningless?' This is a falsehood: I showed that some data was meaningless, and avoided using it, as rigour demands. I then worked on data which had not been shown to be meaningless. I set out a large amount of evidence, fully referenced and argued, showing 15 different bodies of evidence pointing to a serious shortage of food, which he does not mention, though they are mentioned in his sources. It is an achievement to produce 15 falsehoods in such a short sentence.

In real-world economics, such as the economics of the Bengal famine, we always face the problems of gaps in our information, of false information, and of weaknesses in statistics – even the best statistics are usually collected for other purposes, with definitions inappropriate for our problem, probably as a by-product of administration. So, in any analysis of the real world, we never rely on a single source. We work with all available information, fitting it into a complex, interlocking, model so each bit of information can be tested against all others. We aim to refute our model and replace it with a better one. In this case:

1. There is no evidence at all that there was adequate food. The chaukidar forecast is known to be worse than useless.
2. Professional agriculturists in the Department of Agriculture, whose job involves assessing crops, stated that the chaukidar forecast had overestimated production by a third in 1943, but not in 1941. If they had been able to allow for the low yields and high post-harvest losses from the fungus, they would have calculated a greater overestimate. This means that it is impossible to compare the rice crops of the two years.
3. Mahalanobis (1943) says that in 7 of the 30 years leading up to the famine, including 1941, the chaukidar survey falsely predicted a crop failure. This shows the unreliability of the survey. This also means that it is impossible to compare the 1941 and 1943 crop as is fundamental to Daoud's argument, and Sen's.
4. Traders made a much lower forecast of the 1942/1943 rice crop than the Department of Agriculture. Their income depends to a large extent on their skill in forecasting, in buying standing crops or lending on them. It is also important in deciding whether to sell as much as possible before the next crop in the hope of getting a higher price, or to keep as much in store as possible if the next crop is short and prices will rise. If they had been able to allow for the low yields and high post-harvest losses from the fungus their forecasts would have been even lower.

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5. Calculations of the food availability, as opposed to rice production, required a lot of guesses (Mahalanobis, 1943). Goswami, for example claimed that the resulting estimates overstated availability by between 18% and 29%. (Goswami, 1990, p. 457)
6. Statistically controlled agricultural research plots showed catastrophic crop failure and post harvest losses from the fungus.
7. The Famine Inquiry Commission (1945) reports the general belief that the fungus caused more food losses than the cyclone (Famine Inquiry Commission, 1945), though they do not appear to have had access to research station reports. See also Amery (1988).
8. The Government of Bengal did not find large hoards in house-to-house searches.
9. The Government of Bengal found lower than usual stocks when searching traders' warehouses.
10. The Government of Bengal could not buy locally produced rice even by paying more than the going market price.
11. The claim that the famine was caused by some workers eating more than usual, so there was not enough to feed the population, is physically impossible. Nobody could eat that much.
12. Contemporary evidence was that 'it would probably be an underestimate to say that two thirds of the total population were affected by it' (Department of Anthropology, Calcutta University, quoted by Rajan (1944)). A very large survey of the survivors (Mahalanobis, Mukkerjee, & Ghosh, 1946) shows large impacts on rural populations. This means that it is necessary to explain an enormous difference between the claimed 'adequate' or 'sufficient' food supplies and the amount eaten by most of the population.
13. The claim that speculation caused the famine is impossible. See (Bowbrick P. , 2022a; 1986) and, particularly, Bowbrick (2022c).
14. The Bengal Government acted on the belief that there was adequate food. The result was a famine.
15. Nobody has produced economic scenarios to support the myths that grew out of bazaar rumours – that inflation, hoarding, speculation etc caused the famine.

See Bowbrick (2022a) for fuller details and references.

All available information points in one direction, and the information from different sources cross-checks. This is most unusual: we are usually trying to work with contradictory information. Daoud makes no attempt to refute this.

4.3. Plenty of food in Bengal?

Daoud claims that I misrepresent him by saying *'Daoud states repeatedly that there was plenty of food in Bengal'* (2022a, pp. 4, 6). This is yet another falsehood. Most of his falsehoods in his 2028 paper suppressed evidence on how bad the crop was, and overstated availability. I quoted his claim that there was at least 11% more grain available in 1943 than in 1941 when there was no famine. I quoted his statements on availability verbatim: these appear to support my understanding (2022a, pp. 3-4). The lowest availability that Daoud is prepared to consider is 'what Bowbrick (1986) calls a 'first degree shortage', meaning that 'there is sufficient food to provide a barely adequate diet for everyone, provided that there is rationing' (p. 106)' though this is excluded by his calculations above. Daoud's claim that I am wrong is based entirely on him introducing terminology that does not occur in his 2018 paper – 'macro abundance' and 'macro sufficiency' – terminology which he does not define. Changing terminology does not constitute an argument.

I examined his claim in great detail (2022a) and produced a wide range of evidence to show that it is false. In his Response, he repeats his claim, not mentioning the criticisms of his statistics, and producing no evidence, new or old.

4.4 Extra consumption

Daoud states, that *'he [Bowbrick] argues that there could not have been any extra consumption (e.g., due to military needs or hoarding) in Bengal,'* but he gives no reference and I have found no such argument in what I wrote. On the contrary, I did point out that in his discussion of Malthusian causes he omitted these:

'It is surprising that Daoud does not mention that the Famine Inquiry Commission discussed population as a factor, including the sudden population increase from the influx of Burmese refugees and the army. (Bowbrick P., 2022a, p. 22)'

Again, Daoud produces a falsehood.

My paper was entirely on falsehoods and myths in Daoud's 2018 paper, and was not presenting my own explanation (as Daoud mentions repeatedly in his Response) so I had leave out some points I would consider essential for my own explanation.

4.5 Hoarding

Hoarding is not 'consumption' as Daoud claims. If the size of the hoarded stock at the end of the famine is less than that at the beginning, the difference may be treated as consumption. I set out evidence that increased hoarding in the years leading up to the famine increased the availability in (Bowbrick P., 1986) which he cites. Formally, hoarding may be treated as a form of speculation (Bowbrick P., 2022a; 2022c). Daoud (2018, p. 459) cites the Famine Inquiry Commission as saying

that Bengal went into the famine with unusually high hoarding stocks, as I said, and to the extent that they were consumed during the famine – that is what they were there for – hoarding increased food availability during the famine. I quoted the Famine Inquiry Commission as saying that house-to-house searches found very little hoarded food. There were not a lot of people who had the money or the storage space to build up significant hoards. Again, Daoud produces a falsehood.

4.6 Price Statistics

I showed (2022a, p. 15) that price statistics were unreliable or obviously false, which I have been arguing for half a century and which is hardly contentious nowadays. Daoud's use of them produces meaningless results, which he casts doubt on his key beliefs, such as 'FED'. He admits reading my points

'Sixth, Bowbrick moves on to stipulate that price statistics were meaningless by writing that "Nobody knew how much was sold at any price" (Bowbrick, 2022, p. 14), or "Price statistics are notoriously inaccurate." (Bowbrick, 2022, p. 14). (see his Section 7).' (Daoud, 2023)

He chooses not to challenge me, nor mention the implications. Nor does he challenge my criticisms.

4.7 Inflation

Daoud's statement *'he [Bowbrick] claims that there was no inflation'* (2023, p. 7) is false. In view of the multiple falsehoods in his paper, one would have expected the Journal to check this. A quick search-and-find in my paper would have shown that I mentioned inflation (2022a, p. 20) and I have covered it in more detail elsewhere in a publication he cites (Bowbrick P. , 1986).

He goes on to say, *'or that inflation did not cause the famine.'* I did not say that inflation did not cause the famine: in 1986 I set out what observations a credible model would have to cover and explain if the inflation explanation were ever to be anything more than a myth or bazaar rumour. This was enough to frighten off analysts: neither Daoud nor anyone else has produced a model of how it could have caused the famine. In my paper I set out other factors that a credible model would have to cover, taking into account more recent experience, particularly of hyperinflation (2022a p20).

He goes on to say, *'or that inflation did not cause the famine – despite that Bowbrick has just refuted the measurement error of price statistics (his Section 9)'*. He appears to be arguing that it is false that inflation caused the famine if and only if there are accurate price statistics (or perhaps only if there are no accurate prices?). Neither bears any relation to what I said or implied. Daoud's argument is a diversion, a distraction: any reader who tries to make sense of it will certainly not be thinking of the subject of my paper, 'Falsehood and Myths in Famine Research'.

Again, Daoud's stating that I have not mentioned something, even if it were true, is not an attack on any of my criticisms'. I explicitly stated that I was not giving my analysis of the famine and not covering everything I thought important. I mentioned only falsehoods in Daoud's paper, and only some of them.

4.8 Malthusian Causes

Daoud states,

'Nonetheless, Bowbrick erroneously asserts that "Daoud claims a Malthusian cause of this famine, that Bengal's population grew so fast that the province could no longer feed itself. . ." (Bowbrick, 2022, p. 22)'. (Daoud, 2023, p. 4)

I had shown that his evidence did not support his claim, and he now denies that he ever made such a claim. Again, one would have expected the Journal to check. In fact, I started the discussion by saying *'Daoud (2018) claims that a Malthusian population increase was an important contributing factor,'* (2022a, p. 21), which is what Daoud states:

'Indeed, increased food requirements in the Bengal system seem to have been an important contributing factor. There was a relatively rapid pan-Indian population increase, along with a population increase in Bengal (Pp). The Bengal population increased from 37 million in 1881 to 61.5 million in 1941 (Greenough, 1982, p. 62; Islam, 2007). The Bengal province, which is one of the world's most fertile areas, 'is no longer an exporter of foodstuffs or even self-sufficient' (Greenough, 1982, p. 8), and indeed, 'a classical Malthusian situation of population outstripping productive resources developed' (Greenough, 1982, p. 62). Even during normal periods, food security in Bengal was generally low (Famine Inquiry Commission, 1945, pp. 6–7). A pan-Indian estimation of average annual per capita food output from 1893 to 1946 shows a decline of 32%. This is primarily attributable to a pan-Indian population increase of 38% (Bhatia, 1967, p. 315); however, there was an almost 61.4% increase in Bengal alone (see Table 2). A major population increase occurred from 1931 to 1941, which further pressured the Bengal food system. This strengthens the validity of treating FRI as an important background condition of the famine.' (Daoud, 2018, p. 463)⁹

This confirms my statement, *"Daoud claims a Malthusian cause of this famine, that Bengal's population grew so fast that the province could no longer feed itself."* (Bowbrick, 2022, p. 22)'. His claim is a falsehood.

We expect a Malthusian population increase to result in increasing vulnerability to other shocks to the system including crop failures and changes in demand, so population increase is a contributing factor, which is what Daoud claimed, and what I said he claimed.

It is surprising that Daoud should have forgotten what he wrote.

⁹ Note that he omits the presence of the Army and 350,000 refugees in Bengal during the famine.

4.9 British Colonialism

Daoud states,

'Seventh, he [Bowbrick] states that British colonialism did not suppress Indian interests and that there was no political bias against Indian rural farmers (his Section 8)' (Daoud, 2023, p. 6).

This is a falsehood: I said nothing of the sort: I do not hold any brief for the colonial policies. I have not encountered the mix of issues Daoud mentions, nor do I see them in his original paper. The sole purpose of my Section 8 was to expose falsehoods in Daoud's work.

My Section 8 showed, inter alia, that the key actors in the famine, the Governments of the United Kingdom, India, and other countries in the Empire, the Bengal Government, the governments of the surplus provinces, traders, and their staffs had very different objectives and acted differently (Bowbrick P. , 2022a, pp. 17-19). The Government of India, for example, could not impose its will on the provinces let alone on the private sector.

This invalidates Daoud's political beliefs on 'British colonialism': his belief that every action or lack of action by any politician or official, British or Indian, Bengali or Punjabi, Christian, Muslim, Animist or Hindu, at any level of administration in the Governments of the United Kingdom, India, Bengal and the surplus Provinces, was part of a single, immoral, colonial plot.

I pointed out the rigorous approach needed:

'It would be standard practice to start by examining what actually happened, then to examine which decisions contributed to the disaster and then to ask who made the decisions and what their stated reasons were. Only then could one start to analyse what political factors influenced the decisions and whether they were local political factors or colonial.' (2022a, pp. 18,19)

5.0 On Sen

Daoud says that my *'contradictions have already been addressed by Sen (1986, 1987) but keep on resurfacing.'* (Daoud, 2023, p. 5). This is a falsehood, both misrepresenting the facts and suppressing other facts. My paper set out a brief summary of the problems with Sen in 527 words with 25 references (Bowbrick P. , 2022a, pp. 2,3), so he cannot plead ignorance. I have showed that Sen's theory was physically impossible. *'I presented 13 rigorous, formal, evidenced, refutations, six of which were fatal to Sen's entire theory and seven of which were fatal to significant parts of it. They are independent: Even if one, or several, could be shown to be invalid, the rest would remain unchallenged. In fact, neither Sen nor anybody else has even attempted to challenge any of them. I refuted Sen's theory of the cause of the 1943 Bengal famine (Bowbrick, 1986, 1987, 1988, 2008). I presented 13 rigorous,*

formal, evidenced, refutations, six of which were fatal to Sen's entire theory and seven of which were fatal to significant parts of it. They are independent: Even if one, or several, could be shown to be invalid, the rest would remain unchallenged. In fact, neither Sen nor anybody else has even attempted to challenge any of them. Nor has there been any attempt to counter my evidence that Sen systematically misrepresented the facts in his sources in more than 30 instances'. Sen did not attempt to deal with the accusations of misstating some facts and suppressing others, but produced more falsehoods. His replies concentrated on personal abuse and diverting attention from my criticisms. For example, I concluded that '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.' (Bowbrick P. , 1986). Sen did not challenge this statement, which refutes his claims in their entirety: instead, he sneered at my use of English – correct English used by a native English speaker. This is both a diversion and personal abuse.¹⁰ 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 reinstated. I don't think Sen can really answer your criticisms so he is trying to mock them.' (Blackman, 1986)

Comments directed towards the author rather than the evidence

Daoud adopts the policy of accusing me personally of a large amount research misconduct or gross carelessness, telling the readers that he will demonstrate this later, or that he has done so already. He then fails to discuss the issue, let alone to produce any evidence. Readers may accept these claims and not notice that no evidence is presented. This is seriously unethical. COPE (2021) requires that *'the content focus on the substance of the article rather than comments directed towards the authors...'* Daoud avoids the substance and produces these personal comments. COPE (2021) requires that he should produce *'evidence or data to support the claims made'*. Daoud produces none. COPE mentions libel and, indeed, there is a possibility of a libel action here. COPE (2021) requires that the response should not raise issues that are *'trivial, inaccurate, incorrect, or invalid'*. Daoud's response is issues that I have shown to be trivial, inaccurate, incorrect, or invalid'. This means that these statements are 'Abuse' under COPE's criteria.

In his response Daoud claims, inter alia,

- That he will show that I *'have misinterpreted the empirical evidence [he has] used to develop [his] theory.'* (Daoud, 2023, p. 1)
- That he will expose how *'Bowbrick is mischaracterizing key empirical statements,'* (Daoud, 2023, p. 1)

¹⁰ I have documented his misconduct in (Bowbrick P. , Toxic famine research: and how it suppresses its critics, 2020).

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- That he will be *'showing how Bowbrick is misstating historical facts and empirical data.'* (Daoud, 2023, p. 1)
- That I am *'still mischaracterizing [his] empirical work. Thus, next, I address two central mischaracterizations that Bowbrick leveraged.'* (Daoud, 2023, p. 3)
- That I made *'contradictory statements'* (Daoud, 2023, p. 4)
- That I end *'up formulating arguments that are beset by contradictions.'* (Daoud, 2023, p. 5)
- That Bowbrick *'then (falsely) claims that Daoud (2017) is stating that there was plenty of food (macro abundance) in Bengal (see his section 3).* (Daoud, 2023, p. 5)
- That *'Nonetheless, Bowbrick erroneously asserts that "Daoud claims a Malthusian cause of this famine, that Bengal's population grew so fast that the province could no longer feed itself. . ." (Bowbrick, 2022, p. 22).'* (Daoud, 2023, p. 4)
- That *'Seventh, he [Bowbrick] states that British colonialism did not suppress Indian interests and that there was no political bias against Indian rural farmers (his Section 8)'* (Daoud, 2023, p. 6).
- That my *'contradictions have already been addressed by Sen (1986, 1987) but keep on resurfacing.'*

He either presents no analysis or evidence to support these claims, or produces more falsehoods.

COPE would characterize these aspects of Daoud's paper as personal abuse. In view of the ethical criteria of COPE it is surprising that the Journal of International Development should have published Daoud's Response.

5. MY OWN EXPLANATION OF THE FAMINE

Daoud repeatedly complains that I do not present my own explanation of the famine. It would be improper for me to do so – only a book could do justice to so important and so complex a subject, and the book would not overlap with Daoud's work.

6. DAUD'S 'THEORETICAL APPROACH'

Daoud suggests that *'As Bowbrick stands completely silent about the validity of my theory of famines, I am inclined to assume that he actually agrees with it'* (Daoud, 2023, p. 3). Nothing could be further from the truth.

His discussion of his 'HASAS theory' diverts attention from the subject of my paper, Daoud's falsehoods and myths, which is unacceptable.

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*'my reply will primarily focus on the core principles of my theory of famines, ... That strategy of replying will hopefully be more illuminating for the readership of the Journal of International Development than showing how Bowbrick is misstating historical facts and empirical data.'*¹¹

Most of Daoud's 'Response' presents a revised version of what he called his 'HASAS approach' which he now calls 'SAS' or his 'theory of famines'. I did not mention this approach in my paper, so it has no place in a Response or Reply, nor does it address the subject of my paper, 'Falsehoods and Myths in Famine Research'. Are we to expect that the dozens of other people working in the research programme who have produced the same or similar falsehoods and myths in their work will claim that this gives them the right to publish articles on their theories in the Journal of International Development, or elsewhere?

My refutation was not just a refutation of Daoud, but a refutation of most of a wider research programme on famine that uses these falsehoods. I did not suggest that these falsehoods arose because of his theory: on the contrary, the same failures occur in papers written by people using different theory. If I had mentioned Daoud's theory, other offenders would be able to say, 'I am using a different theory so I can ignore all the criticisms of his evidence and analysis'. This would be disastrous, permitting these falsehoods and myths to continue to dominate the research programme.

Daoud's presentation of his HASAS theory suggests that it is strongly dependent on his interpretation of the Bengal Famine, his model, an interpretation which is clearly false: I have refuted it a dozen times over. It would, therefore, be irrational to discuss this theory even if it were relevant.

It would be irrational in the extreme to give any attention to a theory which produces the same results whatever the evidence, however false it is.

Trust is of key importance. Few of us have the time to analyse pure theory and check it, and fewer still are competent to evaluate theory outside our specialty. For example, few of us understand how the statistical tools we use routinely were developed and proved and we have to take them on trust. But today 20% of medical papers are believed to be fraudulent (Smith, 2021) and we have no reason to believe that economists are any better. If economists accept fraudulent research – which they may do quite innocently – they may waste years of their time, and if they are working on famine, they may kill millions. It would be irrational to place trust in theory produced by someone who produces so many factual and analytical errors in their empirical research: one must start with the presumption that there are similar errors throughout the theory. We can have even less trust if the author has a record of refusing to admit to errors, let alone to correct them.

¹¹ The readers of the Journal think otherwise: my paper was the most downloaded paper published by the Journal that year.

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We may contrast the theory that Daoud uses in his model with the standard tools used by agricultural economists. Food crises and famines are agricultural markets under extreme conditions, and most of the structures of a food sector remain much the same as it moves to 'poverty and malnutrition', then 'supply shortage' then famine. Over the centuries the appropriate theory to analyse famines has been the agricultural economics used to deal with agricultural and food markets and policy. Agricultural economics has been the largest single area of economics up to the last one or two decades of the twentieth century, and it has a vast amount of theory, tried and tested in a wide range of situations, on hundreds of different products in different countries. Our theory routinely deals with famines caused by supply falls, demand changes, population increases, invasion, inability to buy food and the problems of subsistence agriculture, for instance. We use market economics, marketing, production economics, farm management economics, consumer theory, speculation theory, the economics of quality, economics of the firm, agricultural statistics, marketing statistics, and of course rural sociology¹² and sociology (I have had to use all of these branches of the subject in my real-world economics). We can rapidly build up models of very different markets in different situations, and we produce dynamic models that can handle the rapid changes in a market as a famine progresses. We would usually have knowledge of the practical aspects of food marketing, of production, processing, storage, packaging, transport, wholesaling and retailing, and of the types of firms and individuals who carry out these tasks. When we deal with food sectors, we are acutely aware that each market and each sector is different, with different factors coming together to cause or mitigate problems so we have to build situation-specific models. The factors relevant to a given situation may be related to the simplistic concepts that Daoud calls 'FAD', 'FRI' or 'FED' explanations, though agricultural economics was using far richer versions long before these names were invented. We are aware that food markets keep changing, by season and in response to government action if nothing else, so we build dynamic models. I would expect my colleagues to identify several key falsehoods in Daoud's paper in an hour or two, using this theory.

Nothing in Daoud's work gives us any reason to believe that he has these skills in his toolbox, that he knows they exist, that they are included in his theory, or that they are compatible with his theory.

In my Rejoinder I do not discuss Daoud's theory for another reason. Any discussion of his theory draws readers' attention away from the subject of my refutation, that his work is full of falsehoods and myths.

7. KNOWINGLY PUBLISHING FALSEHOODS

The Rejoinder I wrote at the request of the Journal of International Development, and which passed their refereeing process, flagged up falsehoods in Daoud's Response. The Journal and the publisher published the Response knowing it contained falsehoods, and suppressed the this.

¹² Agricultural economists and rural sociologists have shared the same abstracting services since 1956, as there is an overlap, so we have at least some idea about the issues.

It might be argued that this is UK's Consumer Protection from Unfair Trading Regulations 2008 b Under The Consumer Protection From Unfair Trading Regulations, (Great Britain, 2008) which is identical to EU legislation, it is an offence to fail to inform the consumer of a key fault, that a paper is known to be wrong, for instance, and to continue to sell it.¹³

8. WHAT THIS MEANS TO FAMINE RESEARCH

The famine literature contains a lot of falsehoods and myths, including examples of research misconduct and fraud. Researchers may quite innocently believe what they read and use it. Indeed, many of the false statements made by Daoud are taken from the falsehoods to be found in the literature, especially that of Sen and his followers.

Daoud (2018, pp. 458, 459) repeatedly accuses me of being an 'FAD advocate' which he uses as a term of abuse, solely on the grounds that, '*A series of natural disasters that occurred at the end of 1942 should be seen as the major cause of the famine (Bowbrick, 1986, 1987)*' – that is, because I showed that **one single famine** was caused by a crop failure. He would therefore accuse the Indian statistics profession, Bengal agricultural officers, plant pathologists, traders, civil service whistle-blowers and others of being 'FAD advocates'. Because of this belief he suppresses all the evidence and economic analysis that we use. Other writers have made similar abusive statements, though using FAD (Food Availability Decline) in a range of contradictory senses (Bowbrick P. , 2022b). And their views are taught to students. One result is that public servants, politicians, economists and agriculturists may be persuaded that it is irrational to think that a famine may be triggered by a crop failure, which is extraordinarily dangerous as famines (including the 1943 Bengal famine) have been allowed to happen because decision makers chose to believe that there was enough food available.

¹³ I have listed elsewhere (2022) some other legislation which may be relevant to publishing, 'The Consumer Protection from 'Unfair Trading' Regulations (Great Britain, 2008a; European Commission, 2005) require that customers are given any information that might stop them from buying a product. It is up to the courts to determine whether the following legislation is applicable. The offence of 'knowingly or recklessly engaging in a commercial practice which contravenes the requirements of professional diligence' (Great Britain, 2008a) implies an ethical obligation to prevent others from 'Unfair Trading'. The law specifies a lot of offences called 'Abuse of position' (Great Britain, 2013; Great Britain, 2006). Under the Fraud Act, for example, someone who 'occupies a position in which he is expected to safeguard, or not to act against, the financial interests of another person,' has special obligations¹³ (Great Britain, 2006). Other legislation that exists or did exist and may be relevant included the Business Protection from Misleading Marketing Regulations (Great Britain, 2008b). The European Directive on Consumer Rights (2011), the Theft Act 1978, Control of Misleading Advertisements Regulations 1988, The Business Advertisements (Disclosure) Order 1977, The Control of Misleading Advertisements Regulations 1988. The legislation is constantly being updated.'

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Thus, key people may reasonably believe that it is career-limiting to report what they have observed or their analytical conclusions – they may be labelled ‘FAD advocates’.

When dealing with a food crisis one should always start with the hypothesis of a food shortage, because it can take well over a year to source food aid, finance it, import it, and transport it to the famine area. If orders are not placed until officials have finished examining unlikely but theoretically possible alternatives, the food will not arrive in time. A manageable food crisis is turned into a famine. If the initial hypothesis of a shortage is examined first, and is shown to be wrong, there is time to switch to other measures, if it is not correct.

These are some of the ways in which bad research can cause famines and kill millions.