

The case of the Padmanaban Committee

The general perception among Indian scientists in leading institutions, most of whom were trained abroad, is that they are being discriminated against. Research papers from India sent to top international journals seem to be reviewed with a bias. Even if I manage to publish one of my papers in one of the best journals, it will rarely be quoted or have an impact, unless I have a US–Western pedigree or a connection with an inner circle. There seems to be an inherent disbelief in the West that high-quality research can be done in India.

If I left it at that, it may not raise many eyebrows. Such sentiments are common in India. But in fact those are not my words (nor my opinions). Those lines are from a letter that G. Padmanaban wrote to *Science*¹ in 1998, in his capacity as Director of the Indian Institute of Science, Bangalore.

Let us suppose I used that paragraph without credit. It is not identical to what Padmanaban wrote: a few words have been altered or rearranged in each sentence. But if I pointed out these differences as evidence that the text is my original work, and claimed that the similarities arose from our addressing a common topic, I would be laughed out of court. And no investigatory committee would dare declare that they had seen my original manuscript, which was different from Padmanaban's, and therefore were satisfied that my published text too was 'indeed different'.

Yet this is exactly what has happened in the recent investigation of alleged fraud at NCCS, Pune, by an investigatory committee chaired by Padmanaban.

The case has been covered by *Current Science* in its June 10 issue², and a letter from Padmanaban also appears in the July 10 issue³. Brief coverage has appeared in *Science*, *Nature* and elsewhere. The question is of nine Western blot images, in two papers published in the *Journal of Biological Chemistry (JBC)* by the group of Gopal Kundu at NCCS, Pune, that are alleged to be duplicates of other images in those papers. The papers are Rangaswami *et al.*⁴, which we call Paper I, and Rangaswami *et al.*⁵, which we call Paper II.

The Society for Scientific Values⁶ has listed the offending images in its report, and I extracted them from the original

articles on the *JBC* website, scaled them and overlaid them atop one another. Images from the second paper were consistently smaller and grainier than those from the first, and there were differences in brightness and contrast; but other than that, the correspondence seemed to be exact, in every case. These images are available at <http://www.imsc.res.in/~rsidd/kundu/> and before reading further, I would urge the reader to have a look at them.

We ignore the question of whether the duplicated images are fraudulent or a genuine mistake. But are they indeed duplicated? Nearly every biologist whom I have met is categorical that Western blots cannot turn out to be so identical, even when the intention is to reproduce the same data, let alone when they purport to discuss different data. The Padmanaban Committee disagrees: it categorically states that the images are different, not admitting the possibility of either an error or deliberate deception.

I had the opportunity to meet two committee members recently, and they too agreed privately that such correspondence is unlikely, but said that it was still just possible that the images were genuine – one of them estimated that the probability of this is 5%, or 1 in 20: not low enough to condemn a scientist.

But it is enough – because there were nine such images. If we accept the 5% estimate, the probability of all nine being genuine is 1 in 20⁹, or less than one in 500 billion. There have not been enough Western blots done in the history of the technique to make it likely.

It turns out here that not only do the bands align perfectly, so do the black borders around them – and these are artifacts of the cropping software, not features of the Western blot. Or else, if we

choose to superimpose two offending images so that their borders align, the bands align too (except in one case where there was further cropping, to retain only four columns out of five, and a lateral inversion). A shift of even one pixel in any direction worsens the alignment. All of these are illustrated on the webpage cited above; but I reproduce one particularly curious example in Figure 1. These are two strips from Paper I that superimpose as a single unit, on two strips from Paper II, with only the intervening axis of numbers relabelled.

This makes it easy to sharpen the calculation. We focus on the horizontal alignment of the bands. How likely is it that the horizontal positions of these bands would line up to one pixel accuracy? The nine offending figures themselves can be used to estimate the typical gaps between bands, and their variance. It turns out to be typically 10–20 pixels – that is, 15 ± 5 pixels. It would not be unreasonable to take the probability of perfect alignment of two bands as 1 in 10.

But suppose we assume the absurdly high value of 1 in 3, i.e. suppose we expect independent bands to align with an error of at most one pixel, to the left or right. Then the probability that all five bands in a strip will align perfectly is 1 in 243, and the probability that this will happen in eight strips (discounting for the moment the cropped strip) is less than 1 in 10¹⁹. The universe is believed to have existed for 15 billion years, or about 5×10^{17} s. If we had been doing ten Western blots per second since the Big Bang, we still would not expect to see such data.

A thorough forensic analysis would take into account a more realistic estimate of the horizontal shifting, the vertical alignment, the positions of streaks, smears,

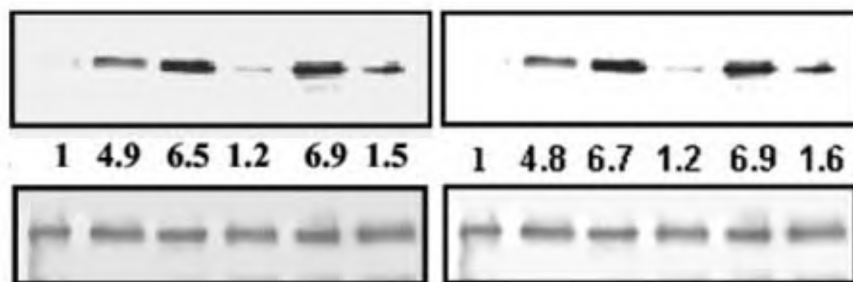


Figure 1. (Left) Figure 7 c of Paper I. (Right) Figure 6 a of Paper II.

spots, and so on – all of which are evident to the naked eye, and all of which line up as soon as one aligns the black borders. Even if every one of these metrics has one chance in three of lining up, the probability of all of them doing so would quickly become infinitesimal, even for a single image.

However, as already noted, the presence of nine duplicated images makes such an effort unnecessary. If we unthinkingly estimate that a single image has a 5% – or even 10% or 20% – chance of being genuine, the probability of all nine being authentic is already vanishingly small.

This is a trivial conclusion: an undergraduate could have arrived at it. But there were no undergraduates on the Padmanaban Committee.

Perhaps they had good reasons for their conclusions, but we do not know them. They have chosen to dismiss the whole matter in just over two pages (their report is available on the website of the Society for Scientific Values); and they go on to say, with similar lack of evidence, that ‘the e-mails (*sic*) have been sent with malicious intent to spoil the reputation of NCCS’.

Why should an investigation of such importance produce such a meagre report, with absolutely no available supporting information? If Padmanaban and his committee members sincerely believe their conclusions, should they not make their analysis public, in order to thoroughly clear the names of Kundu and his colleagues? Other fraud investigations around the world have made their reports, totalling dozens or hundreds of pages, publicly available. This culture of secrecy in Indian science does it no credit.

Padmanaban has twice (June 10 and July 10) chosen to defend his committee’s report in *Current Science*. His main defence is that they examined the original autoradiograms and they were authentic. This is akin to declaring that though two photographs are the same beyond any statistical doubt, the original negatives proved to be authentic. Did the autoradiograms shown to the committee correspond to the published images? If this were so unambiguously true, why not make all the data and analysis public?

We live in times when whistleblower protection is vigorously being debated in all walks of life; in a retrograde step, Padmanaban names the alleged senders of the original e-mails in one of his articles. As far as I can see, he is the only one to do so in public. It is a fine message to send out

to young scientists concerned by possible misconduct, as I am sure Padmanaban is perfectly well aware.

The main theme of the defence of Kundu *et al.*⁷ appears to be that the results are correct regardless of the authenticity of the images (though we have no independent evidence of this), and that the doubts are only about unimportant control blots (which is not true). Even if these claims were correct, it is appalling to realize that senior Indian scientists consider this a serious and adequate justification.

As noted above, Padmanaban has been outspoken in his belief that Indian scientists do not get sufficient respect in the West. His opinion is not universally held; but even if it is true, matters will hardly be improved by our brushing aside serious issues of scientific ethics with bogus investigations and absurdly inadequate reports. Things will not improve by ‘shooting the messenger’: questioning the whistleblowers’ motives and heaping abuse on Sohan Modak. We will earn no respect, either, by pleading publicly with a reputed international journal, which has withdrawn the disputed paper following its own investigation, to reconsider, while supplying absolutely no evidence that its decision was wrong. Nor will respect be earned by, when all else fails, imputing discrimination by the West against Indians. Quite the contrary.

I hope our senior scientists and our scientific establishment recognize one day that openness and honesty are the only way to earn respect in the academic world. Meanwhile, their words and actions destroy the credibility of Indian science, and thereby damage and demean all of us.

1. Padmanaban, G., *Science*, 1998, **281**, 175.
2. Padmanaban, G., *Curr. Sci.*, 2007, **92**, 1471–1473.
3. Padmanaban, G., *Curr. Sci.*, 2007, **93**, 6.
4. Rangaswami, H., Bulbule, A. and Kundu, G. C., *J. Biol. Chem.*, 2005, **280**, 19381–19392.
5. Rangaswami, H., Bulbule, A. and Kundu, G. C., *J. Biol. Chem.*, 2004, **279**, 38921–38935.
6. <http://www.scientificvalues.org/Padmanaban%20Committee%20Report.pdf>
7. Kundu, G. C., *Curr. Sci.*, 2007, **92**, 1469.

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Response:

I am in a catch-22 situation! Earlier, I had decided to put a stop to this correspondence from my side. In fact, even in the beginning I was maintaining silence when Sohan Modak was sending all his e-mails, till I had to write what I wrote. Rahul Siddharthan has cast aspersions on the openness, honesty, etc. of the committee, for which I assume responsibility. He has repeated the charges of Sohan Modak. The target for these people is the committee (or me) rather than Kundu’s data or the explanations offered by the authors. I am, therefore, forced to respond. I can even accept an error of judgement, if I am convinced, but not a charge of deliberate attempt to cover up something, which is what Siddharthan’s tirade would suggest.

I wish Siddharthan had read my letter in *Current Science* (2007, **92**, 1471–1473) more carefully. To be specific, I still think that figure 7c from paper 1 and figure 6a from paper 2 are from different original autoradiograms. I had given these earlier, but I am once again enclosing a scanned picture of the original autoradiograms (see Figure 1). One can see the artifactual bands below the main bands in figure 7c, but not in figure 6a. When these are processed, the intensities have become lighter and look similar. The originals were even cut at different corners. If the autoradiograms are not from independent experiments, they can only be obtained by giving the same blot different exposures. The other possibility is that the autoradiograms used for the journal are different from the ones shown to the committee and that is where comparison of the published figures to the autoradiograms shown become relevant. The original nine allegations were an attempt to throw at every thing, hoping something would stick and that would speak for the intent. Thus, similarity between control blots and patterns of free probe at the bottom of the gel and even reengineering 5-slot blots into 4-slot blots have been suggested to allege manipulation! If anyone has to resort to such malpractices, sky is the limit and beyond anybody’s imagination. It is not as if the committee did not discuss these possibilities. This is where discussion with the actual workers and data from repeat experiments help to understand the motivation, rather than just basing the conclusion from published material. My

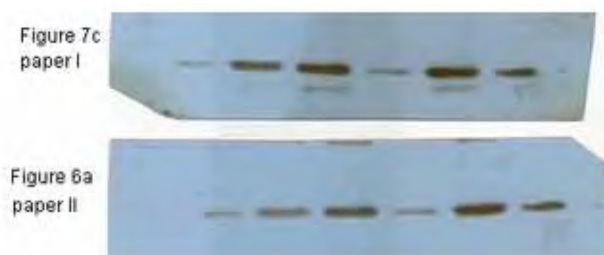


Figure 1. Comparison of figures 7 c and 6 a from papers I and II respectively.

point on control blots was also from the perspective of understanding the motivation, not that they are not important. Why would anyone need to use the same control blot, when there are so many available, especially when they are all expected to look similar, indicating that the same amount of protein was loaded in all the slots. I went to the library and looked at about 12 control blots from different *JBC* papers (such a waste of time!) and at least two looked very similar to the naked eye! Maybe Siddharthan can look at 100 control blots, especially generated from a single gel apparatus, and establish their similarity or otherwise and publish a paper, perhaps with the help of undergraduates! I had mentioned that a

brief report was sent in the first instance, although we had all the analysis. I had also indicated that I was in the process of compiling all the data to be submitted to DBT, New Delhi as required. I have done this almost a month ago and it has all the analysis amounting to 118 pages. It is uncharitable to propagate that the committee took its job lightly. I believe in the innocence of people until proven guilty. The detractors would like to have it the other way around.

Siddharthan has chosen to comment on my letter to *Science* in 1998. That was written in a totally different context of India being targeted for sanctions after the Pokhran nuclear test. I was trying to explain the euphoria in India, despite my

own aversion to nuclear tests, as due to a feeling of alienation and discrimination, especially among the scientific community. I do not think it is correct to quote sentences out of context. At that time I had received close to 500 e-mails (mostly NRIs), with 98% totally agreeing with my views. Two letters criticized me for being a cry-baby. That should satisfy Siddharthan!

I can only state that openness, honesty, credibility of Indian science, etc. need not be the prerogatives of a chosen few, who can vehemently proclaim the same. After interacting with the scientific community in various ways for over 40 years without any axe to grind, it is a sad day for me that a couple should preach me on these virtues, using this episode as a pretext. I guess scientists should never take things for granted or cease to learn! I wish Rahul Siddharthan the very best!

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NEWS

MEETING REPORT

Lunar and planetary science*

The 38th Lunar and Planetary Science Conference (LPSC) was attended by planetary scientists from diverse fields. More than 1500 participants from 24 countries were present. The conference comprised of 43 sessions on different themes spread over five days. Two poster sessions in the evening also covered a variety of topics.

With several nations, including India, preparing to launch lunar and planetary missions and both USA and Europe seriously considering the setting up of scientific

bases on the moon and mars, conferences such as LPSC are increasingly assuming greater significance. The focus this year at LPSC was clearly on Mars, with nearly a third of the sessions devoted to the red planet. Currently, three orbiting spacecrafts (Mars Express, Mars Reconnaissance Orbiter (MRO), 2001 Mars Odyssey) and two rovers (Mars Exploration Rovers) are studying the red planet. Further, with two high-profile missions (Phoenix, Mars Science Laboratory) to be launched soon, Mars is the most extensively explored planet at present.

In a special session on MRO, John Mustard (Brown University, USA) reported detection of phyllosilicate assemblage with unaltered olivine-bearing

lithology on top at Nili Fossae. This observation, based on high spatial (~25 cm/pixel) and spectral (few nanometres in the 0.3–3.92 μm range) resolution, provides an important constraint on the timing of aqueous activity on Mars. This area was studied based on earlier results obtained from OMEGA instrument on-board Mars Express. The same capabilities were also put to good use for evaluating possible landing sites for the Phoenix mission, scheduled for launch in August this year. Presentation by M. P. Golombek (JPL, USA) dealt with the size frequency distribution of rocks in the candidate landing sites, for identifying areas relatively free of boulders to facilitate safe landing of the Phoenix mission. K. D.

*A report on the 38th Lunar and Planetary Science Conference held at the South Shore Harbour Resort and Conference Center, Houston, USA between 12 and 16 March 2007.