A Response to Herring and Rao

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I am grateful for the responses from Ronald Herring and N Chandrasekhara Rao. I particularly enjoyed Herring's roast of postmodern anti-empiricists, since I have devoted several decades to painstaking empirical field research on this complex thing called agriculture. But long-term commitment to empirical study obviously hones one's analytic view of the flood of data that comes out on the topic. My viewpoint, as evidenced in my 22 September 2012 EPW article, is that we have to evaluate each study on its own merits, including its research design, field methods, analysis, and interpretation – the exact opposite of "levelling the epistemic field" and writing off all facts as "modernist fiction." Herring need not be agitated by my willingness to critique peer-reviewed research, or fear that I am endangering the scientific enterprise by doing so. It is how science works.

Such an analytic viewpoint is nowhere more important than for research in the hotly contested and often emotional arena of genetically modified Bt cotton in India. Unlike some aspects of agriculture, the effects of the new seeds are notoriously difficult to isolate with confidence, particularly because it is so tricky to create an empirical model of what *hypothetically* would have happened without Bt seeds (a "counterfactual"). Bt seeds were approved in 2002 and initially planted only by a few early adopters, then after a few years they were adopted rapidly and conventional seeds disappeared from the market. Rapid adoption is hardly a straightforward indicator of "success" – the reason cotton farmers were so desperate for new ways to kill insects was their equally rapid adoption of pesticide-intensive hybrids a few years before (Stone 2011). But there had been only a very narrow window for comparing Bt seeds to counterfactuals, and even during that window, comparisons were confounded by biases in who planted the new seeds, how they cultivated the seeds, and by other developments in the rapidly-changing world of Indian cotton (Kranthi 2011).

But the Indian cotton sector has been a key test case for GM crops, and so there has been a loud demand for clear and citable numbers on Bt's impact here. Given the polarisation on the topic, there is a particularly strong demand for numbers cutting through the complexities of the agricultural enterprise to show "success" or "failure".

GM opponents brandish their own lines of evidence predictably showing failure, but these numbers rarely appear in peer-reviewed journals; they rely instead on different systems of authentication, as argued well by Ron Herring (whose analytic skills spring to life when attacking GM critics). Their claims sometimes show a recklessly low standard of proof; my article noted their charge that Bt cotton stubble kills livestock, but even more common is the claim that Bt seeds are responsible for farmer suicides – despite the fact that suicides peaked four years before Bt seeds were even released (Gruère and Sengupta 2011; Stone 2002).[1]

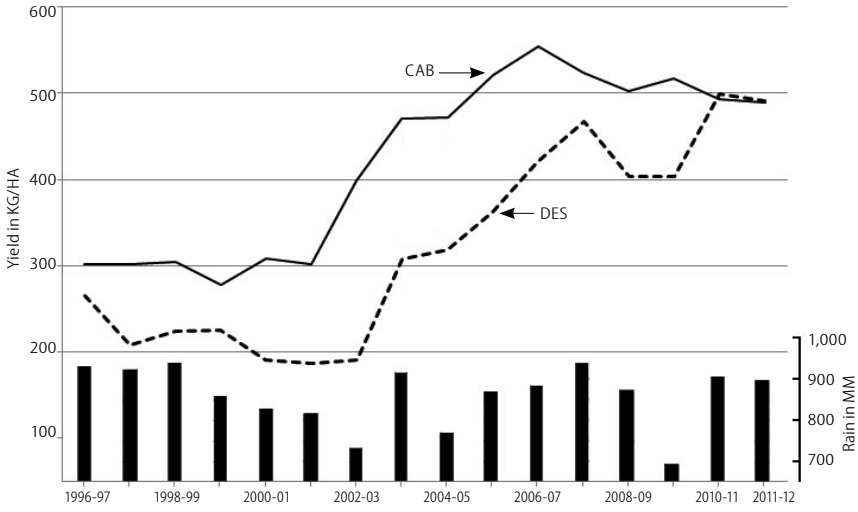
But activists are not alone in scrambling to fill the demand for an unambiguous verdict: peer-reviewed journals too contain a parade of claims that are overstated, oversimplified, scientifically dubious, and sometimes rash. There are claims of Bt seeds giving an eye-popping 87% yield advantage based on data straight from the seed company (published in *Science* no less); there is also a pattern of studies claiming to isolate Bt's yield advantage compared to counterfactuals tainted by selection bias and cultivation bias. The conventions behind these claims are interesting in that crucial details of the counterfactuals are often glossed over; I gave a peer-reviewed example from Africa in which the counterfactual, which was not described at all, turned out to be estimated yields from refuge strips.[2] My aim was not simply to critique specific studies or authors, but to address the larger question of how the peer-review system came to a point where it would accept and even celebrate such shortcuts and generate a narrative of triumph where the available evidence is ambiguous and inconclusive.

Part of the answer comes from the history of research. Long before genetic modified crops, research focusing on the social aspects of technology adoption was sidelined by research claiming to isolate its economic advantage. From the beginning, research in this paradigm showed an eagerness to claim to have isolated changes that were actually closely entangled with other changes (as seen in the history of hybrid maize studies).

Chandrasekhara Rao continues in this tradition with his quickness to attribute changes in cotton yields to the superior performance of Bt cotton, where others in cotton entomology (Kranthi 2011), agro-anthropology (Stone 2012b), and even economics (Gruere and Sun 2012; Smale 2012) see these changes as entangled in a range of other factors. Bt traits have surely benefited some farmers in some places in some years, but the rises in countrywide cotton yields simply do not fit the pattern of Bt adoption. Rao objects to my making this point with the Cotton Advisory Board (CAB) data that many economists also use (e g, Gruère and Sengupta 2011), but his preferred Directorate of Economics and Statistics (DES) data set shows the same thing: average yields climbing before Bt planting was common enough to have possibly accounted for the change (Figure 1, p 71). Rao's own figure shows that yields were up 66.5% by 2004-05, when Bt adoption was still under 6% – far too low to get credit for the countrywide trend. (In Andhra Pradesh, the rise in yields started in 1999, a full six years before Bt adoption took off (Stone 2012a).)

Chandrasekhara Rao is correct that the DES data set does not show the steady deterioration in yields after Bt was widely adopted, but he is incorrect that it shows a steady rise in yields interrupted only by adverse weather in 2008-10; 2008 actually had ample rainfall (Figure 1). There obviously are major factors affecting

Figure 1



Rainfall data are from India Meteorological Dept, downloaded 13 July 2013. CAB data are from http://cotcorp.gov.in and DES data are from http://eands.dacnet.nic.in, both downloaded 10 July 2013.

average yields, and while Bt technologies have been one such factor, we are unable to isolate the contribution. The data on trends are inconclusive.

Chandrasekhara Rao is also quick to accept that many studies have controlled for selection bias (or "the farmer effect", whereby early Bt yields are high partly because early adopters are high producers), but even Kathage and Qaim acknowledge that "most impact studies do not properly control for nonrandom selection bias…which may occur when more successful farmers adopt the new technology earlier or more widely" (2012; also Smale 2012, among others). The Morse et al Maharashtra study which Rao discusses is a good example of how difficult it is to separate yield increases from the farmer effect. Rao holds that the early Bt adopters in this study were not really high producers – they only appear so because in their non-Bt fields they were growing high-producing Bunny seed. But Bunny is not, according to my own detailed study in the neighbouring state, dependably more productive than other brands (Stone 2007a: Table 4, Figure 9); if it tends to have high yields in Maharashtra it is likely because it was being planted by high-producing farmers, as we would expect. Further clouding the situation is the fact that Bunny is marketed under four other names (Stone 2007: 74) and so non-Bt-adopters were almost certainly growing some of the same seed.

The farmer effect is difficult to control, but it is well established: early adopters of technologies are usually a select group as shown by an extensive record of research (Rogers 2003), and it strains credulity to believe that Indian Bt cotton adopters would defy this. I do agree with Chandrasekhara Rao that "The differences in yield effect cannot be attributed to selection bias alone, as other issues such as pest pressure, germ plasm and so on influence yield levels"; I never said that yield differences can be attributed to selection bias alone, but rather that the studies plagued by selection bias are inconclusive.

Early Bt adopters also lavished special attention on their expensive Bt seeds, as exemplified by the image of the early adopter hand-watering her Bt plants. On this point, Chandrasekhara Rao diverts attention to a different question – "whether biotech cotton yields higher in relation to the inputs applied". This is an issue separate from the simple yield advantage attributed to Bt seed. His argument that Bt cotton absorbs higher levels of input and then delivers greater yields is quite plausible. It would have been an interesting hypothesis to test during the narrow window around 2004-07 in a study designed to control for selection bias and carefully monitor inputs. But it was not; here too the record is inconclusive.

Ron Herring is also troubled by my point that Bt adoption trends fail to fit the countrywide rise in yields between 2002 and 2005 because Bt adoption was still very low. His claim is that Bt adoption actually did surge early on, because of illegal Navbharat-151 Bt seeds (sold mainly in Gujarat before the approval of Bt seeds in 2002). I have written about this (Stone 2007b) as has Herring, and I fully agree that illegal Bt was growing in Gujarat. However these "stealth seeds" support my position that the isolation of Bt's impact has been inconclusive. If the illegal Gujarat Bt really was responsible for the rise in yields, and it "was in fields illicitly for three years before the government of India discovered it" in 2001, then we obviously should see a surge in yields in 1999-2002 – but yields then were actually flat (according to the CAB) or dropping (according to the DES). (Moreover, yields in Gujarat had been climbing steadily since the early 1990s (Stone 2012a) – long before Bt landed in the country.)

But even more telling is that the biggest percentage jump in countrywide yields was in 2003 (in both CAB and DES data) – the year after Navbharat-151 was taken off the market. There were still illegal Navbharat-151 offspring being planted, but overall the acreage planted to illegal Bt must have dropped after 2002 when Navbharat-151 vanished from stores. Herring concludes that "all estimates" point to "rapid expansion of Bt acreage below the radar of official statistics". But his adoption "estimates" are largely uncited, unverifiable, and suspicious claims (e g, how did the Gujarat State Seeds Producers' Association get reliable figures on national cotton purchases?) which, even if accurate, would suggest an adoption pattern that fits the yield trends poorly.

But the real focus of my article was not the yield/adoption trends, but a larger issue in the social analysis of science: how such an ambiguous empirical record could be constructed as a narrative of a technology's effect being unambiguously isolated to show a triumph. After exploring the history of changing research paradigms, I gave a clear point-by-point analysis of the separate interests and rewards in the contemporary situation, which Herring has managed to garble badly. What "conspiracy" has to do with it is beyond me; there is no "coordination" and no need for any, much less an "astonishing feat" of it. If journals allow shortcuts with counterfactuals – partly because the peer reviewers themselves take the same shortcuts – then authors will happily take those shortcuts because they facilitate more (and more conclusive) articles. These shortcuts produce "inflated benefit estimates" (Kathage and Qaim 2012), and so the studies are praised and circulated by GM advocates in industry and the ISAAA (International Service for the Acquisition of Agri-biotech Applications). Authors in *World Development* are not given cheques from Monsanto – at least I was not, and I even reported a case of decreased insecticide use and modest yield gains with Bt adoption (Stone 2011). Academics do get promotions and raises by publishing in high-impact journals, but the cheques come from the university. Many of the peer-reviewed studies showing favourable results for GM crops were authored by employees of biotech firms, and I have no idea how their compensation works.

At the risk of being obvious, no research can be accepted as gospel or rejected as heresy on the basis of the journal in which it appears. There is no substitute for evaluating each study on its merits. This certainly applies to my own empirical research on Bt cotton in India, regardless of the journals in which it is published. I believe my fieldwork in Andhra Pradesh covers more diversity in farmer populations (Stone 2007a) and longer time periods (Stone et al 2012) and better avoids selection bias (Stone 2011) than the studies discussed above, but even then it only illuminates one tiny corner of an extremely complex and rapidly-changing cotton sector with enormous spatial, temporal and social variability (Smale et al 2006; Tripp 2009). Bt technology itself is also changing rapidly, as are the insect populations exposed to it (Stone 2011: 391). We may be eager for clear and simple conclusions about Bt's isolated impacts, but the science is simply inconclusive.

Therefore Herring is correct: I am indeed expressing doubt about the empirical basis of a particular narrative (although hardly about "facticity" itself). He is also correct that scientific doubt is sometimes generated for political-economic ends, tobacco "science" being a famous example (Michaels 2008). Tobacco scholars have even coined a term for this*: agnotology*, from the Greek for creation of ignorance (Proctor and Schiebinger 2008), although a more apt term would be *ainigmology*, from the root ainigma (from whence we get enigma), referring to riddles or language obscuring the true meaning of a story. But can my critique really be equated with tobacco industry ainigmology, or is that itself a reckless "levelling of the epistemic field"? As the war over GM crops creates a demand for conclusive findings, parties on both sides seize on claims that confirm their antipathies and hold them up as conclusive even when they simply are not. This may remind readers of Greek philosophy of another reference: claiming to know what one does not actually know is precisely the charge of Socrates against the Sophists in Plato's Dialogues, from whence we get the negative connotation of the term "sophistry".

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NOTES

1 For example, a report edited by Vandana Shiva claims to document that "suicides take on an epidemic proportion precisely when Monsanto began its illegal trials of the cotton"(Barker 2011), as if a tiny number of scattered test plots were somehow culpable for hundreds of farmers drinking pesticide.

2 Refuge strips are small areas of non-Bt plants grown from seeds provided in a small packet along with Bt seeds. Refuge strips are not there for their yield but because they breed insects that are not resistant to Bt.

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