Constructing Facts
Bt Cotton Narratives in India

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CULTIVATION BIAS. This farmer was the first adopter of Bt seeds in a village in Warangal District. At Rs 1600 (around $40 US), the box of seed was four times the cost of conventional seed. When several rainless days had passed after planting, she went into the field with a hired worker to hand-water each seed to ensure germination -- an extraordinary level of attention to a cotton field. Such fields were later used to calculate the “yield advantage” of Bt seed.

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A group of researchers and industry writers have constructed a narrative of technological triumph for Bt cotton in India, based on an empirical record of superior performance compared to conventional seed. Counterclaims of Bt cotton failure are attributed to mutually reinforcing interactions among non-governmental organisations which avoid rigorous comparisons. However, researchers and the biotechnology industry are also engaged in a similar authentication loop for generating, validating, and publicising such facts. With Bt cotton, the convention of routinely ignoring the effects of selection bias and cultivation bias benefits researchers, journals and the industry, but keeps us from drawing meaningful conclusions about the relative performance of the technology. But as poor as the case for isolating the technology impact of Bt cotton in India has been, it is useful in helping us understand the social conventions for creating one's "own facts".

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You are entitled to your own opinions, according to the saying, just not your own facts.

In the global controversies over genetically modified (GM) crops, few "facts" are as pivotal as Bt cotton's performance in India. The developing world in general, and its smallholders in particular, have long been central to the GM debate (Stone 2002), and India has far more smallholder adopters of GM crops than any other country. GM technology also arrived at a critical moment in Indian agriculture: Bt cotton seeds, designed to kill bollworms, entered field trials just as the widespread adoption of hybrid pesticide-intensive seeds was sending smallholder farmers onto a catastrophic treadmill of pesticide and debt (Vasavi 2011). Therefore the impact of this one GM crop in this one country has taken on unusual importance.

Proponents and opponents staked out their positions during the field trials (James 2002; Shiva and Jafri 1998). But the official release of Bt cotton for the 2002 season should have set the stage for an empirical verdict on its performance relative to conventional seed.[1] Many assumed this was a straightforward question; "[m]easuring inputs and yields in agriculture is not rocket science", note Herring and Rao (2012: 46).

Yet by early 2003, starkly divergent accounts had emerged, one claiming a technological triumph and the other an abject failure. Economists Matin Qaim and David Zilberman (2003) used test plot data from seed manufacturer Mahyco to report an 87% yield advantage for Bt cotton over a popular seed. Meanwhile, a study of cotton farmers in Andhra Pradesh (AP) by agricultural scientists Abdul Qayum and Kiran Sakkhari (2003) reported a 53% yield disadvantage for Bt as compared to conventional seed.[2]

There followed a steady stream of field-level studies of Bt cotton's impacts in India (for reviews, see Smale et al 2010; Smale, Zambrano and Cartel 2006; Tripp 2009), with new writing appearing daily in the scientific literature, newspapers, industry publications and blogs. Yet the facts on Bt cotton's impact remain in heated dispute, with most writing falling into the two polarised narratives signalled at the outset. The "triumph narrative" flows mainly from economists and the biotech industry (and its academic allies), including more peer-reviewed writing (recent examples are Herring and Rao 2012; James 2012). It claims Bt seed to be a "remarkable success" (Choudhary and Gaur 2010) and a revolution that has raised yields by 70% (Government of India 2012: 7). The counter-narrative comes largely from non-governmental organisations

(NGOs) along with some journalists and academics, usually in non-peer-reviewed writing; it depicts Bt cotton as a failure, a farce, and a cause of farmer suicides (recent examples are Kuruganti 2012; Shiva 2011).

Each narrative is highly empirical, based on its "own facts", although generated from utterly different systems for collecting, authenticating, and disseminating those facts. I will argue that both "authentication systems" serve the interests of their constituent parties, but that both are so flawed that we simply cannot say how Bt seed has affected cotton production in India. Both categories of writing usually cite other works emerging from its own authentication system, emphasise empirical specifics, and generalise about Bt cotton being a disaster or triumph. Both obscure the fact that they are generated by, and designed to be propagated by, authentication systems that are structured by their own social conventions for creating certain forms of knowledge while nullifying others.

The Indian case has been a poor laboratory for isolating the impacts of Bt seeds, and the time for valid comparisons is past because non-Bt cotton seed has virtually disappeared; what it does offer is a showcase on how very different authentication systems create their own facts. The general problem of how groups of people establish their own rules for facticity has long interested scholars, notably including Kuhn (1962), Foucault (1970) and Latour (2010), and the social nature of this process has been a central topic in science and technology studies (see, for instance, Fischer 2009). My focus here is much more limited. I am specifically concerned with the key issue of Bt cotton's effects on Indian smallholder farming, and with identifying and analysing the dynamics of the two authentication systems that provide us with most of what we know on the topic. We can understand very little about Bt cotton's impacts without recognising and understanding the functioning of these systems.

We will look first at the "reciprocal NGO authentication system", drawing on Herring's analysis of how anti-GM knowledge is created through interactions among separate interested parties (for example, Herring 2008a, b, c, 2009; Herring and Rao 2012). We will then look at the "industry-journal authentication system", which creates pro-GM facts through the interaction of a different set of interested parties. As background to this system, we will first consider the rise of a particular set of assumptions on how to study technology impacts; we will then examine how these assumptions allow for conventions on ignoring bias, and finally at the components of (and interaction among) the parties of this authentication system.

The NGO Reciprocal Authentication System

Herring and Rao (2012: 45) note that most accounts of Bt cotton failure in India have come from writers and researchers working with NGOs. With few exceptions, this work is authenticated and propagated without use of peer-reviewed journals. Clearly impatient with what he saw as yarns being spun by GM opponents, Herring analysed how a narrative of field-level failure is generated and maintained. He terms it a "reciprocal authenticity dynamic" – a "Janus-faced brokerage" in which (2009: 19):

> ex-colonial powers and their press authenticate global narratives for local networks; local reports legitimated by indigeneity provide confirmation for global narratives. The concreteness of local stories finds credibility where abstract numbers fail.

Herring's example is United Kingdom (UK)-based GM Watch relying on Hyderabad-based Deccan Development Society (DDS) to provide local accounts of "GM catastrophes" to be distributed and incorporated into anti-GM discourse. The DDS does indeed construct local accounts of GM catastrophe that are picked up by the European anti-GM press. In December 2002, with the first season of legal Bt cotton cultivation wrapping up, the DDS convened a coalition of NGOs under the title of "AP Coalition for the Defence of Diversity" and held a major meeting in Warangal City. The meeting agenda included the "launching" of the aforementioned Qayum and Sakkhari (2003) study, which it had commissioned, a summit of NGO leaders, and a plenary session where individual farmers could report on their experience with the new seed.

The Qayum and Sakkhari study (2003) did have some qualities associated with peer-reviewed research – the authors had scientific credentials, and some respondents had been selected randomly. However, the study was "largely facilitated" by the head of a Warangal NGO active in mobilising farmers against GM crops (DDS 2006). At the launch, there was little doubt that the study was intended as a shot across the bow of the Bt cotton project, and meant to provide fodder for the canons of European greens. I have elsewhere described the plenary session (Stone 2005), which was engineered to provide the ever-present video cameras with farmer accounts of agronomic disappointment. The same farmers appeared in the video (DDS 2003) about "the dream which became a nightmare", each refusing to plant Bt seeds again. The report and video were soon featured in anti-GM narratives in Europe, such as the Amsterdam-based Friends of the Earth International (nd: 48) and the UK-based Combat Monsanto (2008), as well as organisations in the south, such as Malaysia-based Biosafety Information Centre (2004). Herring's general point is an important one: this system for authenticating and publicising accounts of the cotton's performance hinges on the dynamics of reciprocity and the interlocking of interests among the parties.

However this analysis takes the NGO "reciprocal authenticity dynamic" to be essentially non-empirical, acting to "prevent confrontation with empirical findings" and to avoid "facts" (Herring 2009: 19-20):

> the radical freedom of movement leaders from the dull compulsion of economic facts means there is no penalty for getting it wrong... Because of the extra-local nature of knowledge consumption, facticity itself retreats from salience.

Actually, the anti-GM dynamic is heavily based on empiricism and "facticity". In fact, the primary deliverables from Indian NGOs are both the concrete factual exemplars of failure and statistical data from the field. "Name and face" examples of Indian farmers with bad Bt cotton experiences appear in documentaries, such as the Warangal documentary mentioned above, and the gallery by the Southern Action on Genetic Engineering (2011). Statistical field data are even more important. The Warangal meeting was intended to highlight Qayum and Sakkhari's empirical findings, and the DDS later published two more highly empirical reports by the same authors (Qayum and Sakkhari 2004, 2005). P V Satheesh, head of the DDS, has published his own articles on the NGO's website emphasising empirical "farmyard truths" (Satheesh 2007). Vandana Shiva, Greenpeace, and Friends of the Earth offer a stream of statistics on cotton yields, farmer profits, and pesticide use (for example, Shiva 2006).

Not surprisingly, claims of an empirical basis for the failure narrative leave many observers unimpressed. While Herring offers a sceptical analysis of the system of knowledge production, some simply ignore the NGOs' "facts" because they do not appear in peer-reviewed forums. Biotechnologist Shanthu Shantharam (2005) is infuriated by the failure to follow what he sees as the rules of scientific rigour:

> A critical review of all the reports from NGOs on Bt-cotton in India serves as fine examples of how not to conduct a field survey. These reports claim to be 'independent' and 'scientific' whatever they mean!!! The things that they seem to be 'independent' of are scientific rigor and objectivity. Most of them have conducted either post-ante polls or post-harvest surveys or memory recall opinion surveys, and none of them have been designed with any standard scientific methodology, sampling is spotty and size so small that they cannot be used to draw any meaningful conclusions for the entire country. Bias against Bt-cotton becomes glaring when one notices that they descended in those villages in AP only after they heard that Bt-cotton had failed.

These critics are not without merit; the main proponents of the failure narrative would be certain to reject positive impacts even if they could be convincingly shown (Stone 2002), while they accept dubious claims of negative impacts (for example, of Bt plants killing livestock; for a critique see Kranthi 2011a). The narrative is facilitated by the NGO reciprocal authentication system which generally avoids peer-reviewed journals and often breaks rules such as those listed by Shantharam. But the problem with these critics is their rather credulous conviction that the rules being followed by the authors of the peer-reviewed literature allow for the isolation of the impact of Bt seed. The critics' important lesson is that we are naïve in swallowing empirical claims without a careful consideration of how vested interests affect the creation of facts. However, could this caveat be relevant to the triumph narrative? Having looked critically at the NGO authentication system, let us consider its counterpart, beginning with a brief consideration of the history of its key assumptions.

Whose Numbers Count?

While writing in the triumph tradition appears in many non-peer-reviewed sources, most of it refers to peer-reviewed research that is at the centre of the authentication system. This research distinguishes itself on the quality of its facts, based on agreed-on conventions of evidence and peer review.

Yet facts do not fall simply onto a linear scale of quality. Even in peer-reviewed literatures, conventions on what constitutes significance and empirical support vary between disciplines and schools of thought; conventions also vary through time. Herring's (2008c) question of "whose numbers count" is particularly important in peer-reviewed studies of technology adoption. Research on this was pioneered by sociologists, who focused on the "laws of imitation" that generated the s-curve pattern of adoption. The seminal work was Bryce Ryan and Neal Gross' study of the diffusion of hybrid corn (maize) seed in Iowa, United States (US) (Gross 1942; Ryan and Gross 1943), where they had been hired by breeders to investigate farmers' apparently irrational reluctance to adopt the new hybrid seed. Ryan and Gross focused on the social aspects of adoption, such as who adopted early versus late and how farmers influenced each other. There followed a stream of sociological studies showing variations on the themes established by Ryan and Gross. Although social scientists would later refine the models – for instance, recognising a pro-innovation bias and an under-appreciation of the effects of marketing (Bulte and Lilien 2001) – the key drivers of technology adoption seemed to have been largely figured out, and research on the subject declined sharply after the 1950s. It was, as Rogers (2003: 59) put it, "a victim of its own success".

Innovation adoption studies then quickly came to be dominated by economists (Ruttan 1996), who brought about a shift: the social questions were replaced by an emphasis on isolating the economic impacts of specific technologies. The new direction was signalled in studies of hybrid corn adoption by Zvi Griliches (1957, 1960), which saluted the agronomic superiority of hybrid corn and then modelled adoption not as a social process but as a function of the spatial aspects of yield advantage. Griliches attributed the rise in corn productivity from the 1930s to the 1950s entirely to hybrid seed; his data on spatial patterning of hybrid adoption came from geographer Andreas Grotewold's (1955) "Regional Changes in Corn Production in the United States from 1909 to 1949".

The problem was that while claiming to analyse the impact of hybrid corn, Griliches actually did nothing of the kind, as is clear in Grotewold's actual study. Grotewold did write that hybrid seed was an important factor in the rise in corn productivity between 1935 and 1955, but that other key technologies were being adopted simultaneously. Use of commercial fertiliser increased steadily with the introduction of hybrid corn, more than doubling during the years of hybrid adoption; chemical weed and insect control were spreading throughout the Corn Belt, the most effective chemicals appearing since the war; and farm machinery was being widely adopted, including a fourfold increase in mechanical harvesters and tractors between 1935 and 1955. Griliches' assertions about economic benefits that he had not actually isolated established a pattern that has continued, and which is particularly evident today in the "counterfactual issue" in research on Bt cotton effects.

Statements on impacts of agricultural technology are by their nature comparative: they comprise claims about what would have happened had the technology not been adopted. Whatever the adopters are compared to is called the "counterfactual" (Ravallion 2005: 3; Smale et al 2006: 30). But since history is an experiment run with only one iteration, there is almost never a perfect counterfactual. "The essential problem", as Ravallion points out (1994: 4),

> is that we do not observe the outcomes for participants if they had not participated. So evaluation is essentially a problem of missing data. A 'comparison group' is used to identify the counter-factual of what would have happened without the program. The comparison group is designed to be representative of the 'treatment group' of participants with one key difference: the comparison group did not participate.

The effect of the "treatment" (adoption of Bt seed) cannot be isolated without a proper counterfactual, but almost all peer-reviewed Bt cotton studies in India have problems with the counterfactual that leave it unclear if anything significant has been learned. Following Griliches' precedent, Bt cotton impact studies routinely fail to isolate the effect of Bt cotton by failing to control for key factors that affect yields. I will look specifically at the two most glaring and systemic problems with counterfactuals in these studies: selection bias and cultivation bias.[3] We will then consider the interests shaping the peer-reviewed authentication system to try to understand why the system tolerates – in fact, celebrates – the findings and consensuses that it does.

Selection Bias

This results from the tendency for early adopters of agricultural technologies to not be a random or representative sample of all farmers, but rather a group biased towards high production. This effect is well documented in various kinds of innovation adoption (Rogers 2003), but especially in agriculture, having been identified in the early hybrid seed studies noted above: Ryan and Gross found early adopters to be better travelled, and to have larger farms, higher incomes, more education (Gross 1942; Rogers 2003: 34). Many subsequent studies of agricultural technologies show that early adopters in general tend to be more educated, of higher social status, and possessed of larger farms, not to mention more intelligent and better able to cope with uncertainty (Barrett et al 2004; Rogers 2003: 287-91).[4]

Selection bias is the elephant in the room because early adopters are the kind of farmers who would get relatively high crop yields even if they had not adopted. Since we expect early Bt adopters to get higher yields, finding that they get higher yields tells us nothing about Bt, regardless of whether the research is authenticated by peer review or not. The problem is not new or obscure, having been noted by various writers, economists included (Glover 2010b: 492-94; Smale 2012; Smale et al 2010; Smale et al 2009; Stone 2011). It has also been recognised in the economics literature on Indian Bt cotton (Crost et al 2007: 25-26):

> Generally speaking, the problem is that if the adoption of Bt crops is endogenously determined (that is farmers choose themselves whether to adopt or not) it is impossible to determine a priori the direction of the causal effect underlying an observed correlation. If a correlation between Bt adoption and high yields is observed, it could be due to a positive effect caused by the technology, but there could also be a self-selection effect if farmers who are already more efficient than their peers adopt the technology more eagerly. It is well known that, especially in the first years after their introduction, agricultural technologies are adopted unevenly by different parts of the population, as some farmers are constrained by their level of access to information and credit.

Selection bias is noted by Morse et al (2007b), Morse et al (2012) as "farmer effect", and by Bennet et al (2006: 61). It has recently been recognised by Matin Qaim who, after co-authoring numerous articles claiming substantial yield benefits without taking selection bias into account, wrote that Kathage and Qaim (2012: 11652):

> 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. As these successful farmers may have higher crop yields and profits anyway, this can result in inflated benefit estimates.

In India, whenever researchers have taken the trouble to look, they have always found that early Bt adopters have the hallmarks of early adopters elsewhere. Morse et al (2007a, Table 3) found that Bt adopters in Maharashtra owned an average of 58% more land and 75% more non-land assets; Sadashivappa and Qaim (2009: 175) found Bt adopters in four states owned 8-36% more land; Lalitha et al (2009: 143) found Bt adopters in Maharashtra had 91% more land and 18% more education; Mal et al (2011) found that early adopters in Punjab and Haryana were more educated and more committed to cotton farming; Narayanamoorthy and Kalamkar (2006) found early adopters in Maharashtra had 33% more education, 7% more landholdings, and 22% more land under irrigation.

One study even quantified the effects of selection bias in Bt cotton adoption. Working in Maharashtra in 2002-03, Morse et al (2007a) compared yields obtained by non-Bt adopters with yields on the non-Bt fields of partial adopters (i e, farmers who planted both types). The partial adopters' non-Bt fields produced 29% more than the non-adopters' fields in 2002, and 43% more in 2003 (Morse et al 2007a: 47). The finding is worth noting: In the only study to measure the effect of selection bias on yields, the bias alone was found to account for 29-43% of Bt cotton's "yield advantage".

It is no secret why researchers would prefer to ignore selection bias: it is a nettlesome problem in field research. Crost et al (2007) suggest four strategies, two of which actually avoid selection bias and two that attempt to neutralise it statistically. The first is to dictate adoption decisions – for instance, by getting farmers to adopt according to a random scheme. This is rarely possible. The second is to use a natural experiment in which adoption is shaped by a factor unrelated to farmers' characteristics – "arbitrary reasons such as regulatory differences". The third is the instrumental variables approach, in which a correction factor is based on a variable that for some reason correlates with adoption but not with yields. The last is the fixed effects approach in cases where more than one year of production is available.

Each of these approaches poses its own problems, and several observers now agree that the problem of selection bias has rarely been managed with any success (Crost et al 2007: 25; Kathage and Qaim 2012: 11652; Smale et al 2009). This can be seen in a selective survey.

The Mahyco-instigated study noted above (Qaim 2003; Qaim and Zilberman 2003) was unique in using the first method: farmers were recruited to grow test plots of Bt cotton, the non-Bt isoline, and a locally popular hybrid. However company data on the plots showed an unbelievable 87% yield advantage over the popular hybrid (80% over the non-Bt isoline); this has never been approached with independently collected data, and many questions remain about this heavily criticised study (see, for instance, Crost et al 2007: 28; Sahai 2003; Shantharam et al 2008).

Bennett et al (2006) report on early adopters also used data from Mahyco. They assert (2006: 61) that "[i]n most cases, farmers grew Bt and conventional cotton varieties on the same farm", omitting any numbers on this point. As with the other Mahyco data, the Bt yield advantages were suspiciously impressive: 45% and 63% in the two years.

The Barwale et al (2004: 25) study, authored by Mahyco employees, reported on Mahyco's surveys in six states in 2002. It reported an overall yield advantage of 61% for Bt cotton, with the counterfactual simply being identified as "non-Bt". Although peer-reviewed, the article provided no information on how the Bt or counterfactual farmers were selected.

The Naik et al (2005) study interviewed farmers in four states, with no control for selection bias and no figures offered on the characteristics of adopters except that their farms were 9% larger.

Dev and Rao collected data in 2004-05, finding a 32% yield advantage for Bt seed, which they said "clearly shows the superiority of BT cotton" (2007: 40). They too ignored selection bias, but did provide figures suggesting the bias to not be very large in this case. Bt adopters had somewhat larger farms with more irrigated plots and more black cotton soil but the differences were not great. (In this case, the more striking bias was cultivation bias, discussed below.)

The instrumental variables approach has not been used successfully in India, and with very limited success elsewhere (Crost et al 2007).

A series of the studies by Qaim and colleagues, based on data series begun in four states in 2003 and updated three times (Krishna and Qaim 2012, Table 1), reported yield advantages of 30-40%. This project accounts for a plurality of the peer-reviewed articles on Bt cotton in India that neglect selection bias (Krishna and Qaim 2012; Naik et al 2005; Qaim et al 2006; Sadashivappa and Qaim 2009; Subramanian and Qaim 2009, 2010). Actually, "most Bt adopters" in their sample also grew conventional cotton (Qaim et al 2006: 51), but rather than using partial adopters' conventional fields as a check on selection bias (as Morse et al 2007a did), their conventional fields were lumped with non-adopters' fields (Qaim et al 2006: 51). Few details were provided on characteristics of adopters and counterfactual farmers, until an addendum to a 2012 paper, which revealed that Bt adopters in the different periods had 10-55% more education and 1-15% more land (Kathage and Qaim 2012). The latest paper in this series, uses a fixed effects model to adjust for selection bias (ibid). This study acknowledges that previous work has failed to address selection bias, and then does a much better job of isolating Bt effects, producing an average 24% yield advantage.

A longitudinal study in Warangal, AP essentially avoided selection and cultivation bias by comparing samples of farmers in four villages before and after the virtually complete adoption of Bt seed between 2003 and 2007 (Stone 2011). Such a study is sensitive to differences in growing conditions between the two years (though fortunately rainfall differences were slight). It found a yield advantage of 18%, but with enormous variation among villages, and significant decreases in pesticide use. (However it also raised the question of whether short-term yield advantages were at the expense of exacerbating underlying problems in farmers' fraught relationship with agricultural technology.)

There is no escaping the problem: selection bias poses an enormous obstacle to isolating the effects of Bt seed, and recognising selection bias would pose an enormous obstacle to the flow of peer-reviewed publications on this much-followed topic. Ignoring the elephant accords not only with the interests of key players as argued below. But this is not the only bias that has been ignored; let us next consider cultivation bias.

Cultivation Bias

This results from seeds that are relatively costly, or for which the farmer has particularly high expectations, being planted in preferred locations and given greater care and expense than other seeds. This has major impacts on yields, as farmers' fields vary greatly in their water supply, their distance from the residence, their level of attention and investment. Especially in its first few years of availability, when it was much touted and very expensive, Bt seed would have received preferential treatment. For instance, this author's fieldwork found a farmer who was the first adopter of Bt seeds in a village in Warangal district.[5] At Rs 1,600, the box of seed was four times the cost of conventional seed. When several rainless days had passed after planting, she went into the field with a hired worker to hand-water each seed to ensure germination – an extraordinary level of attention to a cotton field.

Cultivation bias also is a vexing challenge in field research. Agricultural labour inputs can be accurately tracked only with intensive fieldwork and daily records (for example, Stone et al 1990; Stone et al 1995). Measuring plot-specific material inputs also requires concerted effort and time by the researcher, as farmers rarely keep specific records.

Cultivation bias has been ignored in most Bt cotton studies in India, and even where it is noted, there is little or no explanation of how these difficult data were collected. But some reflection of cultivation bias is apparent. One study found Bt fields receive 30% more fertiliser and 38% more irrigation, although 6% less labour and 58% fewer sprayings (Mal et al 2011: 167). (No data were recorded on whether Bt seed was planted on better soils or more accessible locations, or how much more labour was invested.) Another found Bt fields received 40% more chemical fertiliser, 80% more manure, 9% more labour, 35% more tractor hours, and 110% more irrigation;

pesticide use was 5% lower (Narayanamoorthy and Kalamkar 2006).[6] In another, Bt fields received 28% more human labour and 21% more machine labour, 83% more irrigation, and 27% more manure (but 2% less chemical fertiliser) (Dev and Rao 2007: 41). Qaim and colleagues reported that Bt fields received 23-26% more irrigation, 13-25% more fertiliser, and 11-18% more labour (Kathage and Qaim 2012: Supporting Information).

Most studies claim to isolate the impacts of Bt without taking cultivation bias into account. In some cases, authors use the Cobb-Douglas production function to adjust for a few of the more measurable inputs (Naik et al 2005; Sadashivappa and Qaim 2009). This is a very partial solution even if reliable plot-specific data on inputs are available; the approach itself is viewed by many as flawed (McCombie 1998), and it has come in and out of fashion. The convention of using this function, or more commonly simply ignoring cultivation bias, and still claiming to have isolated a technology's impact, is a major expedient to researchers. But the result is clear: while purporting to isolate the field-level impact of Bt cotton in India, the peer-reviewed research actually focuses on the most favoured fields of the most productive farmers, and thus isolates nothing.

If Indian research has helped solidify the convention of playing fast and loose with counterfactuals, research in less scrutinised areas has carried the convention to the extreme. Consider the recent triumph narrative of Bt cotton in Burkina Faso. Reports of an 18.2% Bt yield advantage over "conventional cotton" appeared in peer-reviewed articles, with no information whatsoever on how farmers or fields were chosen for the counterfactual (Vitale et al 2008; Vitale et al 2010). Upon further inquiry, it turns out that the Burkinabe farmers have little choice and adoption is usually close to 100%; for a counterfactual, economists used yields from the refuge areas planted around Bt fields (Vitale 2011). Refuges are small strips of cotton that farmers were obliged to plant to support the Bt fields by hosting populations of Bt-sensitive caterpillars. It is unclear that there is any scientific value in such a comparison, but the "facts" (including the trappings of the "scientific rigor and objectivity" demanded by Shantu Shantharam), have provided the basis for a triumph narrative in academic literature and industry-supported media (James 2011). Peer-reviewed journals did not even require the authors to explain what Bt cotton yields were being compared to.

How and why could such research, in India and elsewhere, have acquired its "facticity"?

The Industry-Journal Authentication System

Like the "failure" narrative, the triumph narrative has a problematic empirical basis that has been generated, authenticated, and disseminated by a particular system of interacting parties with overlapping interests. The key components of this industry-journal authentication system are (1) the biotechnology industry, (2) academic journals, and (3) a set of professional scholars (in particular, academic agricultural economists). The vested interests of the components in this authentication system are relatively straightforward, even if the interactions among them are not.

Industry plays a more active role in promoting empirical impact narratives than with previous agricultural technologies; given opposition to the technology, and the industry's public relations strategy of emphasising developing world applications (Glover 2010a; Stone 2002; Witt et al 2006), biotechnology firms are understandably anxious about field performance in India.

Scholastic journals have the goal of maximising the impact and perceived quality of the articles they publish, and their prestige and thus power as an authentication agent. The common measure of impact factor, based on how frequently a journal's articles are cited in other peer-reviewed journals, indirectly indexes "quality" and the level of interest in their articles. As authentication agents, journals rely on prestige gained from their use of peer review, which requires them to extract unpaid work from reviewers.

Scholars are rewarded for the number and the impact of their publications; impact results from numerous factors, including how much it cited, discussed, and covered in the press.

Scholars submit articles to journals in the hope of being accepted for publication; journals solicit the uncompensated work of peer review by scholars. Industry sometimes provides scholars with data (examples above) and funding, and provides economists and journals with publicity; economists and other scholars provide journals with high-impact articles; journals provide economists with career advancement and industry with the raw material for a success narrative.

The overlapping interests of all three stakeholders promote the routine neglect of the biases described above. For economists and other scholars, the benefits are clear: taking selection and cultivation biases into account would both slow down the pace of publication and make for much more qualified conclusions, resulting in fewer publications, in less selective journals, with lower impact. It would also lead to results showing lower yield advantages, which could mean less responsiveness from biotech firms and less propagation of their findings in non-peer-reviewed forums.

For journals, there are two paramount benefits. The first pertains to the refereeing process. The essential pillar of scholarly literature is the skilled and time-consuming work of peer review. The business model of most peer-reviewed journals is to capitalise on this uncompensated work as a free good.[7] There may be a component of altruism in scholars consenting to referee manuscripts, but there are also instrumental incentives pertaining to their own work: refereeing provides opportunities to insist that their own work be recognised and to validate the types of methods they use themselves. A study on Bt cotton impacts is unlikely to be panned for ignoring selection and cultivation biases if the referees ignore the same biases in their own work. In this way, authors, referees, and journals implicitly collude in ignoring biases. The second benefit for journals is obviously that this collusion in overlooking potentially disqualifying biases allows them to attract more manuscripts on a hotly debated topic, with favourable effects on their impact factor.

For the biotech industry (and its academic interlocutors), the benefits are enormous because ignoring biases allows studies to find exaggerated yield advantages for their products. Industry therefore participates in the authentication system in various ways, including providing funding for certain kinds of studies and data and taking a pivotal role in disseminating the success narrative. Industry expenditure on media dwarfs that spent by GM critics, with results from favourable field studies being repeated in advertisements, web postings, pamphlets, and through neutral publications via press releases, and various pronouncements by allied researchers. The Council for Biotechnology Information, the industry public relations entity, which has aggressively promoted the success narrative of Bt cotton in India, had a starting budget equivalent to Rs 1.4 crore (Lambrecht 2000).

Included in this authentication system is the International Service for the Acquisition of Agri-biotech Applications (ISAAA), an industry-supported entity whose main mission is "sharing and disseminating scientific knowledge", particularly emphasising "the benefits of crop biotechnology to various stakeholders, particularly resource-poor farmers in developing countries" (ISAAA 2012). None of the ISAAA's stream of reports is peer-reviewed, but they are routinely cited in the peer-reviewed literature. The ISAAA propagates the narrative of "remarkable success" (Choudhary and Gaur 2010), citing favourable findings on advantages of Bt cotton. Thus industry-supplied test plot data are published with claims of 80-87% yield advantage, which are then broadcast by ISAAA reports and pamphlets (ISAAA 2003) and announced in the mainstream press (Neergaard 2003) with the assumed validity imparted by peer review. The ISAAA often touts the studies' rigour, including in the Burkina Faso study with its dubious comparisons of Bt yields to refuge yields, which was heralded as a "well-conducted survey" (James 2011).

ISAAA announcements then feed into an echo chamber of interlocutors, such as when its recent list of economics studies – lacking valid counterfactuals and allowances for cultivation bias, and several based on the same data – was blogged about by Calestous Juma (2012b) who also tweeted (Juma 2012a) to his thousands of followers, many of whom retweeted the "fact"-based narrative about how evidence was "stacking up against biotechnology critics".

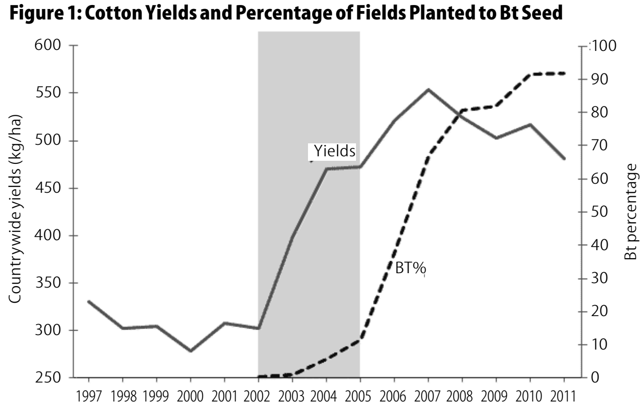
Inconclusions

As we pass the decade mark since Bt cotton was approved in India, we should admit that we cannot say what the effect of the technology has been. Isolating the technology's impact is much more difficult than is commonly assumed, and few comparison studies have done it well; most have not done it at all. It is not "rocket science" (Herring and Rao 2012), but it certainly has been "rough terrain" (Smale 2012), and since conventional seed has virtually disappeared, the time for comparative impact research has passed.

Nor can we look to adoption rates as prima facie evidence of field-level impact; after all, the cotton hybrid-pesticide package was rapidly and enthusiastically adopted in the 1990s, bringing not prosperity but the agricultural train wreck of 1998-2002 (Stone 2011). It was because of that catastrophe that there was an eager market for Bt as a new way to kill bollworms.

Statistics on trends in cotton production are also inconclusive. It is true, as GM enthusiasts stress, that cotton yields have risen in the Bt era: nationwide average cotton lint yields climbed 59.3%, from 302-481 kilogramme/hectare, between 2002 and 2011 (Figure 1). But 94% of this rise (from 302 to 470 kg/ha) occurred between 2002 and 2004, before adoption had even topped 6% (see the shaded portion of Figure 1). Cotton yields were probably climbing in the early 2000s because of factors that, in the tradition of Griliches, many economists and industry writers have ignored in their haste to claim to have isolated the impacts of Bt cotton. K R Kranthi, director general of the Central Institute of Cotton Research, points out that bollworm infestations dropped over the past 12 years "mainly because of a significant decline in the use of the insecticide 'synthetic-pyrethroid' coupled with enhanced usage of some potent bollworm-controlling insecticides such as Spinosad, Emamectin and Indoxacarb" (Menon 2012). He concludes (2011b) that "it is probable that the new pesticides, new hybrids, new micro-irrigation systems, new areas, and Bt-cotton together may have been effectively contributing to the enhanced rate of production and productivity" since the early 2000s. Gruère and Sun's (2012) analysis of trends suggests a 19% overall contribution of Bt to cotton yields, but finds this inseparable from changes in fertiliser, hybrids, labour, pesticides, and irrigation. Bt seed was being planted only by a small group of early adopters until 2006, when it reached 38% (Figure 1). Since then, adoption has topped 92% and yields have dropped 7.6%.[8] State-specific trends are also completely inconclusive; for instance, in AP, the surge in yields began eight years before Bt became popular, and since then yields have dropped back to where they were before (Stone 2012).

Figure 1: Cotton Yields and Percentage of Fields Planted to Bt Seed



Source: Yield data are from the Cotton Corporation of India, Bt adoption data are from ISAAA.

What we do know is that farmer experiences with Bt seed have been highly variable (Smale 2012; Smale et al 2009; Tripp 2009), and single numbers can obscure as much as they reveal. But the demand for the "bottom line" is ravenous and we now have three longitudinal studies that give us a glimpse, through a glass darkly, of the technology's isolated impact. These studies offer a somewhat surprising degree of agreement on numbers that are nothing like the early claims of very large yield advantages. Kathage and Qaim's (2012) multi-village fixed-effects study gives us the number 24%; Stone's (2011) multi-village before-and-after analysis gives us 18%; and Gruère and Sun's (2012) trend analysis gives us 19%.

But we should expect no letting up in the dichotomous flow of claims about this crucial case. Indeed, at the same time that the authentication systems are in direct competition over the acceptance of empirical narratives, they are also deeply co-dependent; and in some ways, they encourage each other. The NGO reciprocal authentication system, with its sometimes dubious claims and disregard for peer review, irritates many scholars and policymakers and leads to a fetishising of journal standards that have their own flaws. The industry-journal authentication system, with its myopic view of Bt as a readily isolatable technological tweak and its cosy alliance between GM manufacturers and ostensibly independent researchers, encourages many to seek out the more critical perspectives offered by NGOs. Therefore demand for both narratives will persist and both authentication systems will continue to entitle both sides in the GM debates to "their own facts".

NOTES

1 In India, the cotton season often spans two calendar years. To simplify the wording, I refer to the season by the year of planting: for example 2002, rather than 2002-03.

2 Even before we consider the issues behind the conflicting accounts, it is worth noting that the simple question of yields, especially short-term yields, is blinkered and potentially misleading. Both studies were looking at farmers' first brush with a technology about which they had little knowledge and none of the locally developed management skill that is the sine qua non of smallholder farming (Brookfield 2001; Netting 1993; Stone 2007; Stone 2011). Claims such as Qaim and Zilberman's (2003) that the 2001 trials of Bt cotton "were managed by the farmers themselves using customary practices" make little sense.

3 My focus here is on yield advantage. Economic impacts, usually assessed through partial budget analyses, raise their own problems, some of which Glover (2010) has examined.

4 Moreover, later adopters have been shown convincingly to be driven in part by cultural factors that have little to do with yield advantage (Henrich 2001; McElreath 2004; Stone 2007).

5 An accompanying photograph, placed on the *Economic & Political Weekly* website, documents cultivation bias. It shows this farmer on her field hand-watering each seed.

6 Morse et al (2007: 47) compare cultivation costs for Bt fields, adopters' non-Bt fields, and non-adopters' fields. The results are curious, showing adopters as applying less fertiliser and irrigation but *more* bollworm insecticide on their Bt fields; see Glover (2010b) and Stone (2007: 85).

7 Since some journals turn a profit, this model clearly offers grounds for resentment. A group of mathematicians has recently announced a boycott of one publisher, denouncing "a system in which commercial publishers make profits based on the free labour of mathematicians" (Lin 2012).

8 These figures are the most widely accepted but they differ from numbers from the Directorate of Economics and Statistics (Government of India 2012, Figure 1.8).

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