Epistemological Depth in a GM Crops Controversy

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Forthcoming in *Studies in History and Philosophy of Biological and Biomedical Sciences*

Abstract

This paper examines the scientific controversy over the yields of genetically modified [GM] crops as a case study in EPISTEMOLOGICALLY DEEP disagreements. Appeals to "the evidence" are inadequate to resolve such disagreements; not because the interlocutors have radically different metaphysical views (as in cases of incommensurability), but instead because they assume rival epistemological frameworks and so have incompatible views about what kinds of research methods and claims *count as* evidence. Specifically, I show that, in the yield debate, proponents and opponents of GM crops cite two different sets of claims as evidence, which correspond to two rival epistemological frameworks, classical experimental epistemology and Nancy Cartwright's evidence for use. I go on to argue that, even if both sides of the debate accepted Cartwright's view, they might still disagree over what counts as evidence, because evidence for use ties standards of evidence to what is sometimes called the "context of application."

Keywords: genetically modified organisms; GMO; feed the world; evidence; scientific controversies; Nancy Cartwright; controlled experiments

1. Introduction

It seems to be a widespread assumption that *evidence resolves factual controversies*. As Thomas Kelly puts it, "Objective inquiry is evidence-driven inquiry, which makes for intersubjective agreement among inquirers"; he goes on to call this the NEUTRAL ARBITER role for evidence, in disputes "among rival theories and their adherents" (2014). Similarly, in his influential paper on the epistemology of disagreement, Richard Feldman presents the basic puzzle of disagreement as a rhetorical question: "how can there be reasonable disagreements when the parties to the disagreement have been confronted with a single body of evidence?" (2011, 143) Feldman expects that, if two parties have the same evidence, equal powers of reasoning, and are both reasonable, then they should not disagree with each other. The evidence, he thinks, would be sufficient to resolve the controversy.

For feedback on previous versions, thanks to Justin Biddle, Paul Thompson, and attendees at the Rotman Institute Postdoc Research Day 2013, the Communities of Integration 2014 conference, and the 2014 biannual meeting of the Philosophy of Science Association.

Of course, the assumption that evidence resolves factual controversies has been seriously and repeatedly challenged over the last five decades by philosophers, historians, and sociologists of science. Discussions of underdetermination, incommensurability, theory-ladenness of observation, and so on, have shown that, insofar as evidence must be characterized in terms derived from controversial theories, there is logical room for opponents of these theories to discount the evidence. (Of course, there remains some disagreement among scholars about the extent to which evidence really must be characterized in terms derived from controversial theories. For a review of this literature, see Bogen 2014, §§4ff.)

These discussions have generally dealt with what we might call METAPHYSICALLY DEEP controversies; that is, the interlocutors in these cases have radically different ideas about what kinds of things exist (or, at least, what kinds of things are involved in the phenomena of interest) and what kinds of properties and relations they might stand in. (Compare Feldman's discussion of cases in which "people ... have different fundamental principles or world views," 2011, 148-9.) For example, for Einstein but not for Newton, the mass of an object depends on its speed relative to an observer; and Priestly but not Lavoisier was willing to countenance negative mass (see, among hundreds of works, Kuhn 1961; [1962] 1996, esp. chs. 9-11; Newton-Smith 1981, 10-13, 114-21, 155-62; Hoyningen-Huene 2008).

However, many scientific controversies are not metaphysically deep: all of the interlocutors agree on what kinds of things, properties, and relations are involved. These include many socially significant controversies, such as tobacco, climate change, and vaccinations. Generally speaking, the interlocutors in these cases do not have radically different ontological assumptions; the tobacco industry and AMA did not have fundamentally different conceptions of the nature of cancer, for example. And yet the controversy may persist, if it is EPISTEMOLOGICALLY DEEP. If the interlocutors have radically different ideas — not about what kinds of things exist, but rather — about what kinds of research should be carried out in order to support or undermine a claim, then they will not be able to agree on *what counts as* evidence. Indeed, we would expect the two sides to offer two different sets of claims as evidence; consequently, "the" evidence will not be able resolve the controversy.

Environmental controversies provide a large class of metaphysically shallow yet epistemologically deep and socially significant controversies. For example, in recent work on the controversy over the causes of colony collapse disorder [CCD], Daniel Lee Kleinman and Sainath Suryanarayanan have shown that commercial beekeepers and toxicologists work with different epistemological standards. Beekeepers argue that a widely-used class of agricultural insecticides — neonicotinoids — play a major role in CCD, appealing to their own observations in the field. Toxicologists — and the US Environmental Protection Agency — require controlled experiments, often conducted in laboratory settings, and based on these conclude that neonicotinoids play little to no role in CCD. Beekeepers and toxicologists do not work with radically different conceptions of, say, the chemical structure or possible mode of action of neonicotinoids. Instead, the controversy persists because the observations collected by beekeepers are not regarded as evidence by toxicologists or regulators (Suryanarayanan and Kleinman 2013).

Suryanarayanan and Kleinman frame their analysis of the CCD controversy partly in terms of recent work on expertise by sociologists Harry Collins and Robert Evans (Collins and Evans 2007). As Suryanarayanan and Kleinman see it, the controversy is a struggle between credentialed and non-credentialed experts — toxicologists and beekeepers, respectively — for recognition and authority as sources of evidence and other knowledge claims.

In this paper, I take a more epistemological approach to a distinct epistemologically deep controversy, over the yields of genetically modified [GM] crops.[1] While some aspects of the broader controversy over GM crops are metaphysically deep — such as rival conceptions of nature (Lacey 1999, ch. 3; McLeod-Kilmurray 2009) — this does not seem to be the case with the specific controversy over yields. Even to the extent that there is ambiguity or disagreement about how yield should be measured — see note 3 — this does not seem to be ambiguity or disagreement about what kinds of things exist or what kinds of relations they stand in.

Just as a lack of metaphysical depth does not preclude epistemological depth, it also does not preclude AXIAL DEPTH, that is, radical disagreement concerning values. While I am primarily interested in epistemological depth here, in §5 I situate the yield debate in the broader controversy over whole systems of food production. Again, aspects of this controversy are metaphysically deep. But I emphasize differences about how decisionmaking authority should be organized and agricultural practices should be evaluated, which do not as such seem to involve deep differences about what kinds of things exist. This is axially (or perhaps "axiologically") deep disagreement, rather than metaphysical disagreement, and I argue that it underpins some of the epistemological depth in the yield controversy.[2]

Overall, the current paper has four tasks. First, empirically, I show that the two sides in this controversy appeal to different sets of evidence. Second, philosophically, I show that these two sets of evidence correspond to two rival epistemological frameworks. Third, I argue that, on one of these frameworks, there is a tight relationship between evidence and the context of application, and that the two sides in the GM

1. To be clear, this paper does not discuss other controversies surrounding these crops, such as their effects on human health or non-target insects such as butterflies. Thus, I am speaking here about *a* GM controversy, not *the* GM controversy. In §5, I do put the yield controversy in the context of a broader debate that might be called "the" food systems controversy. I thank an anonymous reviewer for pushing me to clarify this point.

2. I thank two anonymous reviewers for encouraging me to clarify the relations among metaphysical depth, epistemological depth, and values.

yields controversy disagree about the context of application. Thus, even if the two sides in the controversy could agree on the same epistemological framework, they would not necessarily agree on what counted as evidence. Both the second and third points indicate significant epistemological and axial depth in the crop yields controversy. This suggests that methodological, epistemological, and axial reflection will be needed to resolve the controversy. So, fourth, this paper aims to provide theoretical or philosophical background for a companion paper for practicing scientists actually involved in crop yields research. The current paper assumes a primary audience of philosophers of science, but except for a few technical discussions should mostly be accessible to researcher in fields such as science studies and agronomy.

I proceed as follows. In §2 and supplement S.1, I provide a brief background on GM crops and analyze three frequently-cited review reports on GM crop yields. Two of the reports are "pro-GM": they purport to provide evidence that GM crops have higher yields, and are cited by proponents of GM crops. The third is "anti-GM": it purports to provide evidence that GM crops do not have higher yields, and is cited by opponents. In supplement S.2, I discuss some issues related to funding, conflicts of interest, and the disciplinary training of the authors of these reports. In the body of the paper, I focus on the sources of evidence cited in each report. I show that the pro-GM reports tend to cite surveys of farmers, especially in "developing" countries, while the anti-GM report tends to cite controlled field trials conducted by scientists in the United States. In short, the two sides of the GM controversy work with two different sets of evidence.

In §3, I argue that these two different sets of evidence correspond to two rival epistemological frameworks or conceptions of evidence, and thus that the controversy is epistemologically deep. The use of controlled field trials corresponds to what I call classical experimental epistemology, in which causal relationships are investigated by holding fixed all variables except the purported cause and effect. By contrast, farmer surveys fit much better with Nancy Cartwright's evidence for use, which is more concerned with the ways in which the causal relationship depends on the presence or absence of other "support factors." §4 considers two philosophical responses to this epistemologically deep controversy.

In §5, I work within the framework of Cartwright's evidence for use to examine the relationship between research methods, evidence, and the context of application — the "extra-scientific" situations in which research findings will be put to use. In Cartwright's framework, a key question is whether research provides us with evidence that is relevant (in a technical sense) to some other situations of interest. Drawing on the work of rural sociologists, I argue that GM proponents and opponents are interested in different situations — they have in mind different contexts of application. Research methods and evidence that are relevant to one set of situations need not be relevant to the other. Consequently, the controversy will not necessarily be resolved even if we assume Cartwright's framework. Furthermore, insofar as different situations of interest reflect different values, this section also shows that the epistemological disagreement is produced, in part, by axial disagreement.

2. To Feed the World

In this section, I first make some preliminary comments about GM crops. In the following three subsections, I examine three review reports on GM crop yields, with attention to the kinds of sources cited as evidence by each report. Each of these reports has been cited frequently within the public controversy over GM crops. I show that the two pro-GM reports rely heavily on farmer surveys, while the one anti-GM report relies heavily on controlled field trials. Additional analysis of these reports is given in the supplemental materials. The philosophical discussion picks up in §3.

As I will use it here, GENETICALLY MODIFIED or GM describes transgenic organisms, that is, organisms whose genome includes a gene from another species. In its primary sense, a TRANSGENE EVENT is the introduction of a particular transgene into the genome of one particular cell; by extension, it is used to refer to all of the organisms descended from that cell with the transgene in question. The techniques for introducing a transgene into a genome are formally called DNA or GENETIC RECOMBINATION. There are a variety of recombination techniques, with varying degrees of ease, reliability, and placement precision. Note that recombination can also be used to insert genes from conspecifics, that is, it need not be used only for transgene insertion. Organisms produced in this way are usually not considered genetically modified, especially informal/general public contexts, even though some of the concerns about genetically modified organisms arise from uncertainties in recombination itself (Schubert 2002). ISOGENE and, more precisely, CONVENTIONAL ISOGENE are used to refer to an organism that is, other than the transgene itself, genetically identical to a particular transgenic organism. In the remainder of this paper, I use the more familiar "GM" and "GMO" to refer to transgenic organisms.

By area under cultivation, nearly all GM crops are either herbicide tolerant [HT] or insect resistant [IR or Bt] due to a toxin-producing gene from the bacterium *B. thuringiensis.* Some specific GM crop varieties have STACKED TRAITS, that is, they include several transgene events; typically the stacked traits are HT and Bt. Again by area under cultivation, nearly all GM crops are major commodity crops: soybeans, corn (maize), cotton, or canola (James 2010, 211, 217, 219; "GM Crops: A Story in Numbers" 2013).

Proponents of GM crops frequently argue that these crops are needed to "feed the world" — to ensure food security, especially when world population peaks at approximately 9 billion in the middle of this century (McGloughlin 1999; Borlaug 2000; Goklany 2000; Krattiger 2001; Ronald and Adamchak 2008; Juma 2011; Federoff 2011; *Food Biotechnology* 2013; Council for Science and Technology 2014). For instance, a biotechnology industry "Communicator's Guide" includes "Feeding the World" as one of its four "key messages"; uses such phrases as "food security," "a more reliable harvest," and "increase the amount of food that can be harvested" in each of the other three key messages; and even includes "feed the world" on its list of "Words To Use" (*Food Biotechnology* 2013).

As Hugh Lacey points out, these arguments are ambiguous about whether GM crops are supposed to be sufficient or necessary to ensure food security, the most efficient available means or merely one effective means, and so on (Lacey 2002). Any of these claims must be supported by what I will call the BASIC YIELD CLAIM: GM crops have higher yields than non-GM crops.[3] If the basic yield claim is well-established, then we would seem to have good reason to believe that GM crops are at least one effective means to ensure food security, and we are on the way to establishing stronger claims about sufficiency or necessity.

It might be objected immediately that speaking of "GM crops" generally or in the abstract is a mistake (Stone 2002). It is quite plausible that some GM events — more precisely, some specific traits, inserted in some specific crops, grown in some specific environments — will have higher yields, while other combinations of traits, crops, and environments will not have higher yields. For example, without any further information it is quite plausible that HT crops will not be more productive than an isogene; the HT gene simply enables farmers to use broad-spectrum herbicides in place of other weed management techniques. On the other hand, without any further information it is quite plausible that a transgenic papaya with an immunity to a prevalent, untreatable pathogen will be more productive than an isogene. Likewise, the potential for harmful effects on consumers or the agricultural environment will vary from event to event. Finally, while proponents of GM crops appeal to the need to "feed the world," the largest body of GM crop yield data concerns cotton, which is

3. My use of "yield" here is ambiguous in at least three respects. First, food production can be measured in either absolute units — tonnes, say — or relative units — tonnes per hectare. Second, various kinds of units can be used. Sticking with absolute units, we might measure food production in units of weight/mass, such as tonnes; volume, such as bushels; nutrition, such as calories; or finance, such as market value in constant US dollars. Third, we might be interested in food production under ideal, typical, or actual conditions. IDEAL YIELD refers to food production under the best possible conditions, the maximum food production that could be achieved (using whatever units); this is a strictly counterfactual notion. TYPICAL YIELD is less idealized but still simplified, assuming that all farms are as productive as some "typical" farm(s). For example, observations of US farms today could be used to make projections about global food production in twenty years; or observations of a sample of Indian farms could be used to make generalizations of all Indian farms. ACTUAL YIELD is minimally idealized and simplified; it refers to the amount of food that is actually produced by actual farms. Note that actual yield may still involve inductive inferences from a sample to a whole population. In the three reports that I examine below, "yield" generally — though not always — refers to actual or typical tonnes per cultivated hectare (or short tons per acre, for US observations). Yield comparisons — such as between GM and non-GM crops — are generally expressed in (unitless) percentages derived from tonnes per cultivated hectare or short tons per acre. I note that both of the pro-GM reports are also ambiguous about their use of "yield." I thank Paul Thompson for pushing me to clarify this ambiguity.

not a food crop. For these kinds of reasons, it might be thought, the basic yield claim is an overgeneralization. Instead, we should make only specific claims, concerning specific traits, in specific crops, grown in specific environments.

While this concern about overgeneralization is quite well-taken — indeed, in some respects it is supported by the view that I call evidence for use below — I do not think it is a major issue here. In each of the three reports that I examine below, the authors make both general and specific versions of the basic yield claim, for different kinds of GM traits and different crops. For the sake of brevity, I focus here on the general versions, but note at least some of the specific claims. Most importantly for my purposes, the epistemological issues that I identify seem to apply to both general and specific versions of the basic yield claim.

Unsurprisingly, the basic yield claim is a matter of ongoing scientific debate. In the remainder of this section, I examine three frequently-cited summary reports from different sides of the debate. Notably, this debate takes place on the border between the scientific community, as philosophers of science usually understand it, and the general public. From what I can tell, only one of the authors has an advanced degree in biology (the others have degrees in economics); none of the authors are academics; at least one of the reports did not go through peer review; all make extensive use of "grey literature," non-peer reviewed studies such as government and industry reports; none seems to have been based on a systematic search of published research, as in a conventional meta-analysis; and the statistical methods used to aggregate findings are either simple averages across findings or are not reported. Both of the pro-GM reports acknowledge funding from the biotechnology industry, and the anti-GM report was published under the auspices of an environmentalist advocacy organization.[4] Despite this marginal status, the reports that support the basic yield claim are frequently cited within the scientific community, including within peer-reviewed literature (Ronald 2011; Mannion and Morse 2012, for example). (Additional background information, on the authors of these reports and the uptake of these reports by the scholarly community and general public, is given in supplement S.2. In a future paper, I hope to address some of the concerns raised by this paragraph.)

4. Given all this, one might reasonably wonder whether the disagreement is simply a matter of cherry-picking: the authors have chosen evidence that supports the point of view they are being paid to advocate. (For discussions of the epistemological consequences of industry funding, see McGarity and Wagner 2008, ch. 4; Elliott 2011, ch. 4; Lexchin 2012; Intemann and de Melo-Martín 2014, among many others.) However, following Hicks 2014, §5, I suggest that taking this approach to analyzing the controversy is more difficult than it appears. It is highly improbable that we could gather evidence concerning the authors' reasoning and intentions as they were assembling their datasets, and so we will probably never know whether they were simply cherry-picking. In addition, even if the authors were simply cherry-picking, the pattern observed in the discussion below still seems epistemologically interesting: that the papers supporting the basic yield claim rely on farmer surveys, while the paper challenging the claim relies on field trials.

2.1. **Brookes and Barfoot, "Global Impact of Biotech Crops".** The first review report that I examine is the 2013 edition of an annual report by Graham Brookes and Peter Barfoot (Brookes and Barfoot 2013a, 2013b). Brookes and Barfoot are primarily interested in the economic benefits of these crops for farmers, but they provide data on yield effects as a key part of their economic models. Table S.1 is an abridged version of Table 5 in the 2013 peer-reviewed edition of this report, focusing on their yield findings for GM IR maize. They do not explain how their yield gain estimates were calculated, nor how their literature search was conducted.

Brookes and Barfoot find modest yield increases due to GM crops in the US and larger gains in "developing" countries, with an increase of around 20% in some countries and an increase of 38% for cotton in India. Information on the sources cited for GM IR maize is given in tables S.2 and S.3. The majority of these sources are unpublished (many of them were conference presentations) or otherwise unavailable to me. Of the 8 available sources, 4 are farmer surveys, and another 3 are industry documents; the sample sizes for the surveys range between 28 and 368.

2.2. **Carpenter, "Peer-Reviewed Surveys Indicate Positive Impact".** The second pro-GMO report that I examine was published as a Correspondence (which is not necessarily peer-reviewed) in the journal *Nature Biotechnology* by Janet Carpenter (Carpenter 2010). Carpenter's main findings are summarized in table S.4. As with Brookes and Barfoot, Carpenter finds modest increases due to GM crops in the US and more significant gains in "developing" countries. All of Carpenter's data come from farmer surveys. For "developed" countries, she relies heavily on government sources for soybeans and corn, and peer-reviewed sources for cotton. 16 of the 32 government results come from regional, provincial, or state farmer surveys conducted by Statistics Canada or the USDA Agricultural Resource Management Survey [USDA-ARMS]. Of the 134 peer-reviewed results, only 27 come from non-cotton crops. 11 of these surveys were small ($n < 100$); 14 were mid-sized (n between 104 and 610); and 1 publication with 2 results (notably, by Graham Brookes) failed to report the sample size. Of the 107 peer-reviewed results for cotton, approximately 12 were small, 57 mid-sized, 8 were USDA-ARMS data, and 1 publication with 8 results failed to report the sample size. Her aggregate findings seem to be simple averages (arithmetic means) of the findings reported in her literature dataset; as with Brookes and Barfoot, Carpenter does not explain how her literature search was conducted.

2.3. **Gurian-Sherman, "Failure to Yield".** The third report that I examine was written by Doug Gurian-Sherman, a scientist with the Union of Concerned Scientists, in 2009 (Gurian-Sherman 2009). Gurian-Sherman distinguishes INTRINSIC YIELD and OPERATIONAL YIELD. Intrinsic yield is "the highest that can be achieved, ... obtained when crops are grown under ideal conditions," which I called ideal yield above. Operational yield corresponds to actual yield, "when [there are] environmental factors such as pests and stress." He argues that "while operational yield is important, better protecting crops from pests and stress without increasing potential [read: intrinsic or ideal] yield will not do enough to meet the future food needs of an expanded population" (Gurian-Sherman 2009, 2). That is, if GM crops do not increase actual yields then the basic yield claim is false; and if they increase actual yields without increasing ideal yields, then the basic yield claim will be true, but will not provide good reason to believe that GM crops will help (much) to feed the world.

Gurian-Sherman goes on to argue that GM crops have not increased ideal yields and "delivered only minimal gains in operational yield" (Gurian-Sherman 2009, 2). The first point here seems straightforward: as noted above, nearly all commercially grown GM crops are either HT, Bt, or stacked (both), and in these cases the transgene events either substitute for the use of applied insecticides (Bt) or enable farmers to use a simpler herbicide profile (HT). For the second claim, Gurian-Sherman provides separate estimates for HT and Bt crops. According to his estimates, HT crops have not increased actual yields "compared to conventional methods that rely on other available herbicides" (Gurian-Sherman 2009, 2). Bt crops increase actual yields between 1.5 and 12 percent when susceptible insect pests are present; since these pests are not always present, he estimates an overall improvement of 1.3-5.5 percent (Gurian-Sherman 2009, 3). He does not explain precisely how these estimates were calculated.

In contrast with both Brookes and Barfoot and Carpenter, Gurian-Sherman looks exclusively at the US. He does not seem to explain this, but we can speculate that it is an attempt to limit variation in agricultural practices, crop varieties grown, and other potentially relevant variables. Note that Gurian-Sherman's yield increase estimates are only slightly lower than those of Brookes and Barfoot for the US and Carpenter for "developed" countries.

Gurian-Sherman's sources are summarized in table S.5. As with Carpenter and Brookes and Barfoot, Gurian-Sherman does not explain how his literature search was conducted. Note two major differences with the sources cited by Carpenter. First, for "developed" countries, Carpenter cites only 2 peer-reviewed publications, and relies more on several findings from 4 government reports; Gurian-Sherman cites 10 peer-reviewed publications, and only 2 government reports. (In addition, the 4 "other" sources in the field trial row are working papers and annual reports from public research universities.) Second, Carpenter relies exclusively on the findings of farmer surveys; Gurian-Sherman cites only 1 survey, and focuses on the findings of controlled field trial experiments.

All together, the two papers supporting the basic yield claim rely heavily on surveys of farmers from around the world, conducted by economists, and the one paper challenging the basic yield claim relies heavily on controlled field trials in the US, especially ones conducted at public research universities.

3. Rival Evidence Sets

3.1. **Classical Experimental Epistemology**. Neither Brookes and Barfoot nor Carpenter explain their focus on farmer surveys. By contrast, Gurian-Sherman argues emphatically for the use of controlled field trials:

> To determine the contribution of these transgenes to yield, research must be able to isolate their effects from the many other factors that influence yield .... For studies to accurately attribute yield increases to transgenes, they must try to control or account for these factors. (Gurian-Sherman 2009, 14)

This argument should sound familiar to philosophers of science: in our jargon, attributing yield gains to transgene events requires controlling all possible confounding factors. Field trials are designed to do exactly this: typically, fields are divided into narrow strips, with strips of the test crop(s) adjacent to strips of control crops. All of the strips are exposed to the same weather and insect pests, and (assuming this is not what's being tested) receive the same irrigation, pesticide, herbicide, etc. In the case of GM crops, the control and test crops should be isogenic, genetically identical except for the transgene event (Gurian-Sherman 2009, 14).

Of course, total control of all variables except the presence of the transgene is practically impossible. Field trials, by design, make reasonable efforts to control the most obviously relevant variables, including weather, the presence of insect pests and weeds, and agricultural practices. Examining a convenience sample of the surveys cited by Brookes and Barfoot and Carpenter, I find no good controls for variables such as weather and the presence of insect pests. Insofar as the surveys are conducted over *roughly* homogeneous growing regions, we might expect that *in general* all of the farmers surveyed would have *roughly* the same amount of rainfall, temperature ranges, insect pests, and so on. But the surveys do not seem to gather data to confirm these expectations.

In addition, anthropologist Glenn Davis Stone has argued that econometric farmer surveys routinely overlook two very significant potential confounders (Stone 2012). (Note that Stone focuses on studies of cotton cultivation in India, but argues that these two confounders are quite common in the adoption of new agricultural technologies.) Specifically, first, early adopters of agricultural technologies tend to be "biased towards high production .... better travelled, and to have larger farms, higher incomes, more education .... of higher social status, and possessed of larger farms, not to mention more intelligent and better able to cope with uncertainty" (Stone 2012, 65). Consequently, they "are the kind of farmers who would get relatively high crop yields even if they had not adopted" GM crops (Stone 2012, 65). This is a form of selection bias. Second, because GM seeds are more expensive than non-GM ones, especially when they are first introduced, they tend to be "planted in preferred locations and given greater care and expense than other seeds" (Stone 2012, 66). He cites reports that, for example, "Bt fields receive 30% more fertiliser and 38% more irrigation [than non-GM crops], although 6% less labour and 58% fewer sprayings" (Stone 2012, 66).

Stone argues — and Gurian-Sherman strongly suggests — that, since farmer surveys do not control for these kinds of potentially confounding variables, they cannot support the basic yield claim. That is, by the lights of the epistemological standards assumed by Stone and Gurian-Sherman, farmer surveys either do not count as evidence, or at best count as very weak evidence. The surveys might reasonably be taken to suggest the basic yield claim as a hypothesis worth further study (McKaughan 2008), but it is inappropriate to accept the hypothesis based on the surveys alone. On the other hand, precisely because they make reasonable efforts to control potential confounders, controlled field trials provide very strong evidence concerning the basic yield claim. Call this perspective, with its emphasis on rigorous controls of potential confounding variables, CLASSICAL EXPERIMENTAL EPISTEMOLOGY or CEE.

3.2. **Evidence for Use**. Nancy Cartwright's EVIDENCE FOR USE, henceforth EFU, provides a critical alternative to CEE (Cartwright 2006, 2007a, 2009, 2012; Cartwright and Hardie 2012; I focus here on the especially clear version developed with Hardie). EFU can be explained in a straightforward way by drawing on Mackie's analysis of causal relations as INUS CONDITIONS (see the next paragraph) and a distinction between RELIABILITY and RELEVANCE (Cartwright and Hardie 2012, 10, 25).[5] To preview the conclusion of the following discussion: field trials, as controlled experiments, are reliable but have only limited relevance, and in contrast farmer surveys are potentially much more relevant.[6]

C is an INUS CONDITION for E if C is an *I*nsufficient but *N*ecessary condition for an *U*nnecessary but *S*ufficient condition for E. That is, while C is neither a necessary nor a sufficient condition for E, it is a necessary condition for some $\mathbb{T}$, and this $\mathbb{T}$ is a sufficient condition for E. We can think of $\mathbb{T}$ as a set of conditions $\{C, C_1, C_2, \ldots\}$, each member of which is insufficient but necessary for the presence of $\mathbb{T}$; Cartwright and Hardie call $\mathbb{T}$ a TEAM and the other necessary conditions for $\mathbb{T}$ (that is, the $C_i \neq C$) the SUPPORT FACTORS FOR C (Cartwright and Hardie 2012, chapter II.A). Note that each individual support factor for C is a necessary condition for $\mathbb{T}$. If, for all that we know, C' might or might not be a support factor for C, I will call C' a POTENTIAL SUPPORT FACTOR.

5. Note that the use of Mackie's account seems to be a matter of exposition rather than substantive dependence. I suspect it would be possible to develop another exposition of EFU in terms of, say, recent versions of interventionism.

6. Cartwright and Hardie use "trustworthiness" where I use "reliability"; I make this change so that my discussion here coheres better with a companion paper on trustworthiness and the GM controversy.

Consider the example of Bt crops. It is plausible that (1) the presence of a Bt gene is an INUS condition for higher yields, but only in situations where (2) insect pests susceptible to the Bt toxin are present and (3) alternative pest management techniques are not used. That is, the Bt gene by itself is neither necessary for increased yields (we could use alternative pest management techniques) nor sufficient (if there are no insect pests around, it won't make a difference); but it is a necessary condition for the team $\{1, 2, 3\}$, and this team is a sufficient condition for increased yields.

A research methodology, conducted in a situation S, is said to be RELIABLE if it provides good reason to accept (or reject) a causal claim relativized to S, namely, in S, C causes E. (Cartwright and Hardie are more subtle and precise about the kinds of claims they're interested in; see 2012, 30-2.) By extension, a body of research that uses a reliable methodology is said to be reliable; as is a causal claim that is provided with a good supporting reason by some (actual) reliable body of research. A research methodology, conducted in a situation S, is said to be RELEVANT to some other situation S' if it provides good reason to accept (or reject) a causal claim relativized to S', namely, in S', C will cause E. By extension, bodies of research and particular causal claims can also be called relevant. Reliability and relevance are often called INTERNAL VALIDITY and EXTERNAL VALIDITY, respectively.

To put all this in less technical language, the evidence for use framework starts by recognizing that causal relationships are context-sensitive. Whether and to what extent a Bt gene increases crop yields depends on several support factors: the presence of insect pests, their susceptibility to the Bt toxin, the absence of other pest management techniques, and so on. A research method that is highly reliable provides strong evidence concerning causal relationships in a specific context, with a specific combination of support factors. But reliability doesn't tell us that we can "transport" our conclusions to other contexts, where the support factors might be quite different. A research method can be highly reliable in one context yet not relevant to other contexts.

One of Cartwright's longstanding criticisms of randomized controlled trials [RCTs] is that, while they are reliable, they are not BROADLY RELEVANT, that is, they are not relevant to many different kinds of situations other than the research situation S (Cartwright 2007a). A RCT is reliable because it controls all variables other than the purported cause C of interest. Specifically, it controls all the potential support factors for C. And a RCT as such does not examine the mechanisms by which C causes E in S. The last two sentences together imply that a RCT cannot determine which *potential* support factors are *actual* support factors, that is, which other factors C_i were necessary, along with C, to have a sufficient condition $\mathbb{T}$ for E in S. Now consider a situation S' in which some potential support factor C' is absent. (Indeed, suppose that this is the only difference between S and S'.) Will C cause E in S'? Because the RCT cannot determine whether C' is an actual support factor, it cannot provide good reason to answer either positively or negatively. That is, the RCT is not relevant to S', even though S and S' only differ by C'.

The argument of the last paragraph depends crucially on two features of RCTs: they do not examine either variations in potential support factors for C (crudely, their presence and absence) or causal mechanisms that connect C to E. It seems to me that any research method that — like RCTs — does neither of these things suffers, for that reason, from limited relevance; while any method that does at least one will give us some understanding of the support factors for C, and hence will be relevant to at least some other situations. Using either of these approaches, we can arrive at more subtle causal claims than the ones we considered above: given that $C_1, C_2, \ldots,$ are all present, C will be a sufficient condition for E. Then, examining the new situation S' for the support factors $C_1, C_2, \ldots,$ enables us to accept (or reject) the prediction that C will cause E in S'.

Cartwright and Hardie do not deny that reliability is important; their point is that relevance is just as important as reliability, and they believe (correctly, I take it) that the literature on evidence-based policy has focused on reliability and neglected relevance. In other words, a claim must be *both* reliable and relevant (to some explicit or implicit set of situations of interest $\mathbb{S}$) to count as evidence. A RCT — or other strictly controlled experimental method — can support the reliability of its findings, but other methods must be used to support its relevance to other kinds of situations.

Return now to the controversy over the basic yield claim. Carefully controlled field trials, like RCTs, can be highly reliable. But insofar as they do not look at variations in potential support factors or mechanisms by which GM crops could cause higher yields, they have only narrow relevance. Specifically, as Stone argued, it is highly plausible that farmers' level of education and access to capital are both important support factors, as well as a significant difference between the US and countries like Colombia or the Philippines. Insofar as controlled field trials are conducted by researchers on test plots, rather than on actual farms where actual farmers must make the relevant decisions, they cannot investigate the role of these potential economic support factors. Thus, even assuming that the field trials cited by Gurian-Sherman provide very good evidence about GM crop yields in the US, these findings cannot be extrapolated to the situations of farmers who have lower levels of education and access to capital than the average US farmer.

By contrast, farmer surveys can be much more broadly relevant, insofar as they carefully examine variations in and interactions among support factors on actual, individual farms. Again following Stone, farmers protect their investment in expensive GM seeds by providing them with greater care, ensuring that they have adequate water, and so on. In this case, it seems that the presence of the GM event is causing higher yields; but the support factors include the higher cost of GM seeds, farmer access to capital or other resources, and farmer willingness to devote these resources to the GM crops because of their higher cost, rather than any biological factors. On this basis, we can predict that GM yields will not be greater than conventional yields (or at least the difference will be smaller) when the price drops, farmers have access to fewer resources, or are unwilling to devote greater resources to GM crops. Thus farmer surveys can be both reliable and broadly relevant (if they are conducted well) to specific yield claims; hence according to EFU surveys can count as very good evidence, and specifically much better evidence than field trials.

It might be objected that the actual farmer surveys cited by the pro-GM reports do not carefully examine variations in support factors, and so are not actually broadly relevant.[7] For instance, it is Stone — not the agricultural economists he is criticizing — who observes the ways in which farmers protect their expensive investment in GM seeds. Likewise, it might be argued that the statistical methods used in these studies — t-test comparisons of arithmetic means between GM and non-GM plots, and linear regression models, for example — are inadequate for examining the role of support factors (Morse, Mannion, and Evans 2012).

On the other hand, at least some surveys do use more sophisticated instruments and methods. For example, one survey discussed by Stone includes the following in its analysis: seed cost, seed rate (that is, amount of seed sown per acre), and area under cultivation; use of irrigation, fertilizer, pesticide, and labor; yield, commodity price, revenue, total cost, profit; farmer age, education, land ownership; household size and household expenditures (Kathage and Qaim 2012). And government data cited by Carpenter for "developed" countries come from extremely detailed and rigorous national surveys conducted by the USDA and Statistics Canada.

Still, many of the survey studies do not seem to analyze the relevance of their research methods and findings; that is, they do not consider to what extent these methods and findings are applicable to a variety of different kinds of situations. Similarly, neither of the two pro-GM review reports does much to analyze relevance. They do separate "developed" from "developing" countries and different kinds of GM crops from each other. But this is a very coarse analysis, compared to the numerous possible support factors identified above.

All together, as a methodology farmer surveys fit better within EFU than CEE, in the sense that, while even well-conducted farmer surveys provide very weak or no evidence according to CEE, they can provide very good evidence according to EFU. I believe that this is sufficient to establish the main claim of this section of the paper: the two sets of evidence cited within the yield controversy correspond to two rival epistemological frameworks, classical experimental epistemology and Cartwright's evidence for use. *In principle*, then, the controversy cannot be settled by simply appealing to "good studies" that produce "good evidence," since the two sides

7. I thank Justin Biddle for stressing this point.

disagree about what, *in principle* counts as a good study and good evidence. In this sense, the disagreement is epistemologically deep. At the same time, I recognize that my analysis has raised some significant questions about the quality of *actual* farmer surveys, even according to the friendliest epistemological framework.

This helps us understand, at least in part, why the controversy has not been settled by the significant body of research summarized in these review reports: even given that each side is offering what is, by its own lights, very strong evidence, this evidence is at best very weak by the lights of the opposing side.

4. Philosophical Counsel

In the previous section, I showed that the yield controversy is epistemologically deep: the disagreement between the pro- and anti-GM sides extends to the level of epistemological frameworks, and so the two sides do not even agree on what kinds of evidence would, in principle, be adequate to settle the controversy. In this section, I consider two philosophical responses to this problem. First, we might try to produce better evidence, which can satisfy both epistemological frameworks. This proposal, read simply, runs into a potentially serious practical problem, and I suggest that multidisciplinary research projects might help. Second, we might require both sides of the controversy to accommodate all of the evidence, from both field trials and farmer surveys. However, this proposal is more philosophically tricky than it might first appear. I suggest that Heather Douglas' explanatory approach to dealing with complex evidence might be useful.

My claim in this section is not that I have solved the philosophical problem of epistemologically deep controversies, either generally or in the GM yield controversy in particular. Rather, my point is that recent work in philosophy of science offers some recommendations for working scientists faced with the problem of epistemological depth. Of course, whether these recommendations will actually be helpful in the yield controversy — or any other epistemologically deep controversy — requires actually implementing them, which is beyond the scope of the present paper.

4.1. **Better Evidence**. It might be suggested that the disagreement between CEE and EFU is, in principle, not so deep. It is possible to gather evidence that would be, in Cartwright and Hardie's terminology, both highly reliable and broadly relevant, and this would satisfy both epistemological frameworks. Perhaps farmer surveys — if they looked carefully at interactions among numerous variables, had large samples, and were conducted very rigorously — could be both reliable and broadly relevant. In this way, we might be able to resolve the disagreement by gathering better evidence — better according to the standards of both CEE and EFU.

Note that this proposal depends on the lack of metaphysical depth in the crop yields controversy. If the two sides of the controversy had radically different yet incompatible conceptions of crop yield, for example, we would probably not be able to find any research methods that would study crop yields as understood by both sides at the same time. That is, we would be faced with incommensurability. Fortunately, since the crop yields controversy does not seem to be metaphysically deep — it does not involve incommensurable conceptions of key concepts such as crop yields — it seems like, in principle, we can satisfy both sides.

Still, there are some practical problems with this proposal. A broadly relevant farmer survey must examine many more variables than merely whether GM or conventional crops were grown and what the final yield was. In turn, this requires designing and testing a much more complicated research instrument, much more researcher time conducting the survey, and careful analysis by a highly-trained statistician rather than off-the-shelf software. Consequently, given a more-or-less fixed set of research resources, the sample size will be much smaller, which will reduce the statistical power of the survey. And equivalently, conducting a more reliable survey — namely, one that is more statistically powerful — requires sacrificing relevance. (This could explain why many of the survey studies had small samples.)

In short, given limited resources, we might not be able to achieve both high reliability and broad relevance. We must balance these two desiderata. I suspect that Cartwright and Hardie would favor broadening relevance, even if this required significantly lowering reliability, since broad relevance is what's required for evidence to be useful. CEE, on the other hand, would prioritize reliability over relevance. In other words, the two frameworks would not agree on how the desiderata should be balanced.

Perhaps a multidisciplinary approach would help. Given Cartwright's endorsement of methodological pluralism elsewhere (Cartwright 2007b, ch. 3, for example), I expect that she would recommend multidisciplinary methods in any case. Farmer surveys might be very good instruments for studying social and economic support factors for crop yields. But they will probably not be very good instruments for understanding biological support factors for yield effects, at either the ecosystem or individual organism level. Understanding the interactions among these support factors will require complex research methods that combine, say, economics, ecology, and organismal biology.

CEE would probably be skeptical of multidisciplinary approaches, at least initially. Expensive multidisciplinary studies would not only muddy the epistemic waters — how could we draw causal inferences without rigorous controls? — but also, as with the argument above, reduce sample sizes and compromise data quality control measures, such as randomization and anonymizing. By the lights of CEE, farmer surveys are already highly suspect. An EFU-inspired move to multidisciplinary research would only make things worse.

However, multidisciplinary research projects might be capable of investigating multiple disciplinary research questions at once. For example, carefully constructed farmer survey studies could be used to investigate, at the same time, yield, economic, and ecological effects of GM crops. (And, indeed, interactions among agronomic, economic, and ecological processes, in the way needed for broad relevance.) This might enable us to pool resources that would have been provisioned into separate disciplinary projects. Perhaps as a consequence there would be financial space to improve relevance without excessively sacrificing reliability. For example, rather than constructing and validating three different surveys to study separately yield, economic, and ecological aspects of GM crops, a multidisciplinary team would only need to construct and validate a single survey.

4.2. **All the Evidence**. The underdetermination of theories by evidence seems to be a serious problem in part because rival theories are, *qua* rivals, NON-CONJOINABLE. For instance, given the empirically equivalent theories T_1 and T_2, in general we cannot choose the logical conjunction of both T_1 and T_2 (perhaps because this conjunction would be inconsistent, or would violate considerations of parsimony). For example, while the Copernican and Ptolemaic models of the solar system are empirically equivalent (they both make the same predictions about what we'll observe), we cannot simply combine them, because they make conflicting claims about the actual motions of the sun and earth.

It may seem that the yield controversy does not have this problem. Given our two sets of evidence E_1 and E_2, it may seem that we can simply use both sets together. Farmer surveys and field trials do not logically conflict, even if they seem to provide conflicting evidence, and so our assessment of the basic yield claim can and should take into account both surveys and experimental trials.

Indeed, it may seem epistemically irresponsible not to work with *all available evidence* in this way.[8] In the yield controversy, I have found one meta-analysis that

8. I thank Chris Smeenk for suggesting this point.

does this. (Finger et al. 2011; a longer, non-peer reviewed version was published as Kaphengst et al. 2011.[9])[10]

I take it that both CEE and EFU would agree with the generic advice to use all the available evidence. However, better advice would be to use all *and only* the available evidence. As Cartwright and Hardie put it, "Results that are not to be trusted taken as input produce unreliable results as output" (Cartwright and Hardie 2012, 37). Then, for both CEE and EFU, this advice tells against using the combination of field trial and farmer survey findings. According to CEE, the farmer surveys do not count as evidence, and so should not be used; and, according to EFU, the field trials do not count as evidence, and so should not be used. For both frameworks, the two sets of claims are non-conjoinable as evidence. This is not because of a logical incompatibility between the two sets of claims — again, there is no such logical incompatibility — but instead because of an *epistemological* incompatibility. The two frameworks disagree on what counts as evidence, and appeals to use "all the evidence" cannot settle that disagreement.

But that's a bit too simple. CEE and EFU can both find that farmer surveys and field trials, respectively, count as *very weak* evidence, rather than not being evidence *at all.* This suggests a more subtle version of the current proposal: we should consider all of the available evidence, each weighted according to its value as evidence. So, for instance, CEE can take into account both field trials and farmer surveys: field trials will be heavily weighted, and farmer surveys will be very lightly weighted.

This version of the proposal seems to concede that Finger et al.'s approach — which, in effect, weights both kinds of evidence equally — is problematic. More importantly, to operationalize the proposal, we must determine how to weight the various kinds of evidence. But it is not clear how to do this in a way that could settle the controversy. As Cartwright and Hardie put it, "there's no well-grounded system for 'weighing' up evidence of different kinds of different qualities" (Cartwright and Hardie 2012, 37; compare Kuhn 1977). As in the last paragraph, CEE gives more weight to field trials; but EFU gives more weight to farmer surveys. While the weighting proposal allows both sides to recognize all the evidence, it effectively just pushes the controversy back to the question of how the evidence should be weighted.

9. The article version was published in an open-access peer-reviewed journal published by the Multidisciplinary Digital Publishing Institute; the research was funded by the European Commission. At this time, this is the only review of GM crop yield data that I have found that (a) examines both farmer survey and field trial data, (b) is based on a systematic search of the peer-reviewed and grey literature, and (c) describes the literature search methods used. According to Google Scholar, this paper has been cited about 20-25 times. Because of this relatively low citation count, I have not discussed it as one of my main case studies.

10. While this paper was undergoing review, a second meta-analysis was published, Klümper and Qaim 2014. I have not yet had time to review this meta-analysis, but I believe it is subject to the concerns raised in the next few paragraphs.

Recognizing this problem with weighting approaches to dealing with complex sets of evidence, Heather Douglas' EXPLANATORY APPROACH tries something rather different (2012, esp. 151ff). On this approach, we deal with "explanatory accounts" rather than simple causal claims, such as the basic yield claim. An "explanatory account" is glossed in a few places as a "set[] of explanations," though it does not seem to be precisely defined. We might think of it as a model or model-schema involving the cause, support factors, effect, and the instruments that are used to investigate the relationships among these elements. (Compare Van Fraassen's point that the action of measuring "always involves a physical interaction between 'object' and 'apparatus'," and that the theory involved in this measurement must be able to give an account of the coherence and consistency of this interaction, 2008, 143ff.) In other words, explanatory accounts include causal relationships, but also the support factors surrounding them, and in addition the relationship between the system that is being studied and the researchers studying it. These additional elements are important because explanatory accounts must explain *not only* the causal relationships within the system *but also* the empirical observations of the system made by the researchers — the evidence, and indeed all of the evidence.

In the yield debate, this might work as follows. The two rival positions are not the affirmation and negation of the basic yield claim, but instead two models of the relationships among (various kinds of) GM events, crop yields, agronomic conditions, agricultural practices, and so on. Each of these models must explain not just the basic yield claim (or its negation), but also the observations of GM crop yields gathered using both farmer surveys and field trials (including both the observation processes and the data that they produce). Two relevant models might be called the SOCIAL-ECONOMIC ACCOUNT and the BIOLOGICAL ACCOUNT. According to the social-economic account, crop yields depend a great deal on the social and economic context in which the crops are raised. This explains why GM and non-GM crops have basically the same yields in field trials, where social and context economic context is controlled; and, following Stone's observations about the social and economic status of early adopters of GM crops in "developing" countries, it also explains why GM crops were more productive than non-GM crops in such countries. According to the biological account, crop yields depend much more on the biological properties and ecological context of the crops. HT crops are more resistant to insect pests, for example, and consequently are more productive than non-GM crops; this explains the findings of the farmer survey data. Note that, while these two accounts have different emphases, probably both must include both biological and social-economic variables, in order to explain the full range of observations.

The explanatory approach also emphasizes that explanatory accounts must make testable empirical predictions, and that it's especially useful to have conflicting predictions between the two rival accounts. For example, the biological account would seem to predict that yield gains from GM crops would be about the same in both "developed" and "developing" countries, while the social-economic account would seem to predict differences between the two kinds of countries.

To recap, in this section I have considered two philosophical responses to the problem of epistemological depth in the GM crop yields controversy. Both responses were designed less to resolve the philosophical problem, and more to aid scientists actively involved in the controversy.

5. The Context of Application

Suppose — in whatever way — proponents and opponents of GM crops were able agree on EFU, so that both sides of the controversy had the same epistemological framework. Based on the analysis so far, it might seem like this would be sufficient for both parties to agree on what counts as evidence; and then, hopefully, the evidence would be able to resolve the controversy.

In this section, I argue that this approach will not necessarily be successful. *Even given the same epistemological framework*, the crop yield debate remains epistemologically deep. Briefly put, EFU ties evidence closely to what is sometimes called "the context of application"; and, generally speaking, proponents and opponents of GM crops have in mind different contexts of application. Since claims that are relevant to one kind of context of application need not be relevant to the other kind, the two sides of the debate still need not agree on what counts as evidence.[11]

While my focus in this paper is on epistemological issues, in a fairly narrow sense, it's worth noting that disagreements about values appear at this point in the argument. As discussed below, the disagreements about the context of application amount to disagreements about what kind of food system we *should* have, especially in terms of institutions for property and democratic decisionmaking. Obviously these are ethical and political claims. In this way, my discussion in this section indicates that the epistemological depth of the crop yields controversy is driven, at least in part, by deep axial or axiological ("values") disagreements.[12] In turn, this suggests

11. The situation can also be analyzed as an equivocation. "The" basic yield claim is actually two different, situation-relative claims; using the terminology below, GM proponents argue for the claim that (BYC$_P$) GM crops have higher yields than non-GM crops in $\mathbb{S}_P$, while GM opponents argue against the claim that (BYC$_O$) GM crops have higher yields than non-GM crops in $\mathbb{S}_O$. Evidence with respect to BYC$_P$ need not be evidence with respect to BYC$_O$, and vice versa. On either this analysis or the analysis in the text, evidence cannot be expected to resolve the controversy: there is an epistemologically prior controversy over which situations are the relevant ones, which in turn is a heavily value-laden controversy between rival food systems. I thank an anonymous reviewer for suggesting this alternative analysis.

12. I thank Anna Alexandrova and an anonymous reviewer for pushing me to clarify this point about the influence of ethical and political values on the controversy.

that the controversy might benefit from being "repoliticized," that is, from addressing explicitly the axial disagreements (Sarewitz 2004, 398ff).

Again, farmer surveys can be more broadly relevant than controlled field trials because and insofar as the former, but not the latter, examine potential support factors, such as social and economic characteristics of farmers. For this reason, surveys can provide at least some evidence for causal claims about other farmers in other situations — for example, that other relatively affluent farmers will also see yield increases if they adopt GMO crops, and that relatively impoverished farmers will see smaller or no yield increases. Note, however, that this assumes that the support factors not examined in such studies remain the same. With a different climate, or different GM events with different traits, or different pest and weed management practices, we might not be able to extend the causal claim. Farmer surveys might be relevant to a broad class of situations, but they are not relevant to all possible situations, and perhaps may not be relevant to the situations we are most interested in.

More generally, given EFU, I suggest that we don't normally care about relevance in general or with respect to some arbitrary situation or set of situations. Rather, we care about relevance to some set of SITUATIONS OF INTEREST, typically because, in these situations, we would like to manipulate C in order to cause E. In other words, we care about relevance to some S' because, in S', we would like to ***use*** our causal claim that C causes E. Evidence that is not relevant to any S' of interest does not count as evidence, according to EFU, because it is not useful. It fails to be evidence for use. In this way, the context of application ends up having a significant influence on EFU's standards of good evidence. (For arguments that arrive at similar conclusions by several different routes, see Okruhlik 1994; Lacey 1999, ch. 11; Douglas 2000; Kitcher 2011, §24; Hicks 2012, §6.3.)

Below, I argue that, within the GM controversy, there is deep disagreement over the set of situations of interest. Proponents of GM crops generally assume one set, $\mathbb{S}_P$. Opponents generally advocate a very different set, $\mathbb{S}_O$. Furthermore, even if farmer surveys are reliable and relevant to $\mathbb{S}_P$, they are not obviously relevant to $\mathbb{S}_O$. For opponents, many farmer surveys are epistemologically irrelevant; they tell us nothing about the performance of GM crops in the situations of interest.

This epistemologically deep aspect of the debate is generally concealed by a common rhetoric of "feeding the world." Proponents and opponents might seem to agree that food production is important; and this makes it seem as though they would agree on the standards for evidence for the yields of GM and non-GM crops: more is better. But they disagree radically over the ways in which food should be produced, provisioned, and valued, and consequently disagree over the situations of interest.

To see this, we need to understand the broader GM controversy, which in turn requires understanding the broader food systems controversy. The GM controversy is often framed — especially by proponents — as a controversy over the risks of harmful health and, to a lesser extent, environmental impacts.[13] (For examples, see Borlaug 2000; Kuntz 2012; Lynas 2013; Whitty et al. 2013; DeFrancesco 2013; for scholarship on this framing, see Irwin 1995, ch. 2; Lang, O'Neill, and Hallman 2003; Cook, Pieri, and Robbins 2004; Demortain 2013.) However, as many observers of the controversy have noted, opponents are as concerned about social, economic, and political issues as they are about health and environmental issues (Sagar, Daemmrich, and Ashiya 2000; Stone 2002; Sarewitz 2004, 398; Herdt 2006; Thompson and Hannah 2008; Bronson 2010; Piesse and Thirtle 2010). Specifically, some observers argue that GM crops — especially in terms of issues amenable to quantitative assessment, such as health or yields — serve as a "surrogate" or a "proxy" for a much deeper controversy about the global food system (Nestle 2010, 141ff; Johnson 2014; Thompson 2014). As the journalist Nathanael Johnson puts it,

> People care about GMOs because they symbolize corporate control of the food system, or unsustainable agriculture, or the basic unhealthiness of our modern diet. On the other side, people care about GMOs because they symbolize the victory of human ingenuity over hunger and suffering, or the triumph of market forces, or the wonder of science. (Johnson 2014)

Johnson goes on to argue that, for this reason, the GM controversy is misplaced. The technical terms here would be synecdoche and the fallacies of composition and division: we are arguing about the part (GM crops) when the real disagreement is about the whole (the food system).

I do not think that the GM controversy is quite as misplaced as Johnson does, but I do agree that the narrow, GM controversy must be understood in terms of the broader, food systems one. Specifically, I take it that proponents of GM crops generally assume a system of agricultural production with the following features:

(1) land, agricultural inputs, and intellectual property are held and exchanged as private property;
(2) the production and distribution of food is governed by the global food market;
(3) crops are produced primarily as commodities, that is, to be sold into global food markets and in order to return a profit on investment; and
(4) since they are commodities, crops and the practices that produce them are valued primarily anthropocentrically and using economic standards of efficiency and productivity.

13. Versions of this and the following paragraph also appear in a companion paper, on trustworthiness and the GM controversy.

These features, in other words, characterize the situations in $\mathbb{S}_P$. By contrast, I take it that opponents of GM crops generally advocate a system of agricultural production with several of the following features:

(1′) land, agricultural inputs, and intellectual property are held as common or public property;
(2′) the production and distribution of food is governed democratically, especially small-scale systems of participatory democracy;
(3′) crops are produced primarily for the sustenance of the local communities that produce them; and
(4′) crops and the practices that produce them are valued primarily by integrated anthropocentric and ecocentric or biocentric standards, especially the cultural, ethical, and nutritional standards of the local communities that produce and consume them and the ecosystem function of the local biotic communities.

And so these features characterize the situations in $\mathbb{S}_O$, and the contrasts between 1-4 and 1′-4′ make for significant contrasts between $\mathbb{S}_P$ and $\mathbb{S}_O$. In terminology originally developed by Harriet Friedman, GM proponents and opponents generally assume or advocate rival FOOD REGIMES: proponents assume the "corporate food regime," while opponents advocate "food sovereignty."[14]

With respect to the basic yield claim, proponents of GM crops are interested in whether they have higher yields than non-GM crops *in the status quo food regime*, while opponents are more interested in whether GM crops would have higher yields than non-GM crops *under a radically different food regime.* Clearly surveys of actual farmers in the status quo are relevant to (many of) the situations of interest to proponents. But this is much less clear with respect to opponents. Specifically, if 1-4 are support factors for the basic yield claim, then it's reasonable to expect that surveys of corporate food regime farmers would not be relevant to the food sovereignty regime. Thus, for opponents of GM crops, these surveys would fail to provide good evidence for (or against) the basic yield claim *relativized to the food sovereignty regime.*

So, are 1-4 support factors for the basic yield claim? There are some speculative reasons to think that they are, at least for the specific GM events that are most widely used today: Bt and HT traits could be superfluous if other pest management practices were used, and the intellectual property regimes under which these crops

14. This paragraph is a short distillation of a significant body of research in rural sociology and food systems. For more nuanced applications of food regime analysis to GM crops, see Friedmann 2005; McMichael 2005; Pechlander and Otero 2008; Richards 2010; Holt-Giménez and Shattuck 2011; Holt-Giménez and Altieri 2012; Rosset and Martinez-Torres 2013. For more general discussion of contemporary food movements in terms of the corporate food regime and food sovereignty, see Alkon and Mares 2012; Fairbairn 2012; Werkheiser and Noll 2013. For a contrasting analysis, based on interviews with Chilean farmers, see Tironi, Salazar, and Valenzuela 2013; I thank an anonymous reviewer for this last reference.

are developed and marketed to farmers are incompatible with 1′ and perhaps 2′. Furthermore, under 3′ and 4′ the two main GM commodity crops (corn and soy) would probably not be grown as extensively as they are under 3 and 4. Indeed, under 4′, volume or mass of crop production per area of production might not even be a very important measure of the value of a crop.

However, the reasons in the last paragraph are highly speculative, and should probably be regarded as armchair guesses rather than good reasons to think that 1-4 are support factors for the basic yield claim. As far as I can tell, there has been no empirical research whatsoever on the food regime-level support factors for GM crop yields, in large part because there has been almost no systematic development of alternatives to 1-4 since the industrialization of agriculture (compare Lacey 2002).[15] Without such empirical investigation, it seems hasty to conclude that 1-4 either are or are not support factors for the basic yield claim. And so it is at best unclear whether or not surveys of actual farmers within the status quo agricultural system are relevant to the agricultural system advocated by many opponents of GMOs.

In this section I have argued that proponents and opponents of GM crops work with very different situations of interest. Proponents assume the agricultural systems of the status quo, in which agriculture is embedded in the global market society. (On the distinction between market systems and market societies, see Polanyi (1944) 2001, ch. 4.) Opponents, by contrast, are interested in radically different systems of agricultural production, which are not part of the market society. But, in order to provide evidence by the lights of EFU, a research method must be relevant to situations of interest. Thus, even if farmer surveys are reliable and relevant for proponents of GM crops, they may not be relevant for opponents of GM crops. And so, even if both proponents and opponents accepted — for whatever reason — EFU as their epistemological framework, they might still disagree about what kinds of research methods and evidence are relevant to the basic yield claim. In other words, the controversy would still be epistemologically deep.

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15. Perhaps collective agricultural systems in Marxist-Leninist countries were an alternative, at least with respect to 1. But, as far as I know, almost all such systems ceased to exist before the development of field-ready GM crops in the 1990s, and included both 3 and 4. Cuba is an important exception here; see Nelson et al. 2009.

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S.1. Tables

Country	**Yield gain** (%)	**Number of sources** (P/G/O)
US	7.0	5 (1/0/4)
South Africa	11.8	6 (3/1/2)
Argentina	6.4	2 (0/1/1)
Philippines	18.6	3 (0/0/3)
Spain	9.9	4 (2/0/2)
Colombia	21.0	2 (1/0/1)
Brazil	12.0	4 (0/0/4)
totals		20 (6/2/12)

Table S.1. Average (%) yield gains due to GM IR maize 1996-2011. After Brookes and Barfoot 2013b, Table 5, 80. P/G/O indicates sources that are peer-reviewed, government publications, or other/unknown, respectively.

Study type	**Number of sources** (P/G/O)
Farmer survey	4 (1/3/0)
Field trial	1 (1/0/0)
Industry document	3 (1/1/1)
Not available/unclear	15 (0/2/13)

Table S.2. Sources for yield claims for selected countries, from Brookes and Barfoot 2013b, Table 5, 80. For detailed breakdown, see table S.3. P/G/O indicates sources that are peer-reviewed, government publications, or other/unknown, respectively. Some sources report multiple study types, and appear more than once in this table.

United States:

- 11: early review (2002) published by IFPRI, examines mostly field trials
- 12: early review (2001 or 02) by Carpenter and Gianessi (NCFAP); combination of USDA survey data and unavailable
- 13: NCFAP; yield data not given, unclear, or USDA farmer surveys; assumes production is constant across GM/conventional varieties
- 14: update of 13
- 28: published in Science; no yield data
- 29: published in Plant Health Progress; field trial

South Africa:

- 31: Published by a small peer-reviewed journal; small survey (n=33) of large farmers and mid-sized survey (n=368) of small farmers.
- 32: "A survey of 135 farms in 2003/4 is used in a stochastic frontier model to show that Bt seed did not increase yields per kg of seed."
- 33: Published in AgBioForum; small- to mid-sized farmer surveys; GM maize about the same or more productive than conventional
- 34: Published by the "South African Maize Trust," no record on its website
- 35: Conference presentation; data not available online
- 36: Conference presentation
- 37: 2002 edition of an annual publication by ISAAA, a pro-biotech organization; yield data, if any, are buried

Argentina:

- 40: A book, apparently published in 2002 by a non-academic Argentine press. A search of both Google Books and the publisher's website turned up no matches for the book itself. A Google search turned up an unsigned translation into English; none of the figures or tables seemed to provide any yield data.
- 7: Published by INTA, an Argentine federal agency for agricultural technology. The link provided is broken.
- 41, 42, 43: Surveys of cotton farmers

Philippines:

- 44: Cover image is available online, but not the text; hard to tell what publishing organization does
- 45: Conference presentation
- 46: Pro-biotech organization, not available online

Spain:

- 47: Conference presentation by Brookes
- 48: Paper by Brookes; data from 1997, other publications by Brookes and Barfoot, 49, and field trials by Monsanto
- 49: Conference presentation
- 50: Published in a peer-reviewed Spanish ag science journal; 12.6% yield increase based on small survey (n=28 conventional, 57 Bt)

Colombia:

- 18: Same journal as Brookes and Barfoot; small study (n=20)
- 55: Conference presentation

Brazil:

- 10, 58, 59: Publications by a Brazilian research firm also cited by Monsanto in public relations literature; none available online
- 60: By Monsanto Brazil

Table S.3. Details on sources for yield claims for selected countries, from Brookes and Barfoot 2013b, Table 5, 80.

Technology-Crop	**Mean difference in yield** (%)	**Number of results** (G/P)
Developed countries		
HT soybeans	7	14 (12/2)
IR corn	4	13 (13/0)
HT cotton	0	6 (1/5)
IR cotton	7	24 (4/20)
HT/IR cotton	3	2 (0/2)
Developing countries		
HT soybeans	21	3 (0/3)
IR corn	16	12 (0/12)
IR white corn	22	9 (0/9)
HT corn	85	1 (0/1)
IR cotton	30	82 (2/80)
totals		166 (32/134)

Table S.4. Average impact on yield, by technology and crop, for developed and developing countries. Recalculation of Carpenter 2010, Table 2, using a value of 0% for results marked "NS" in Carpenter's dataset. G/P indicates the number of results from government and peer-reviewed sources, respectively. Note that Carpenter's sources are counted by individual finding rather than publication. For example, findings from three consecutive years in a single survey study would be counted as three sources in this table.

Study type	**Number of sources** (P/G/O)
Farmer survey	1 (0/1/0)
Field trial	15 (10/1/4)
Industry document	1 (0/0/1)
Not available/unclear	4 (0/0/4)
totals	21 (10/2/9)

Table S.5. Sources of yield data cited by Gurian-Sherman 2009. P/G/O indicates sources that are peer-reviewed, government publications, or other/unknown, respectively.

S.2. Background Information on Review Paper Authors

Graham Brookes and Peter Barfoot are agricultural economists and co-directors of PG Economics, Ltd., an agricultural economics consultant firm based in the UK. In the report that I examine in this paper, they acknowledge that Monsanto has funded their research, though they claim no conflict of interest (Brookes and Barfoot 2013b, 82; 2013a, 19 n10). (For insightful discussions of the depth of financial conflicts of interest and the limited effectiveness of conflict of interest disclosures, see Elliott 2011, ch. 4; Resnik and Elliott 2013; de Melo-Martín and Intemann 2009.)

Their report is updated annually, and goes back to at least 2005. The peer-reviewed version of the 2013 edition was published in *GM Crops and Food*, a small peer-reviewed journal published by Landes Bioscience and launched in January 2010; previous editions were published by AgBioForum, a significant journal in biotechnology economics. The 2014 edition of the report was published while I was finishing this paper (Brookes and Barfoot 2014). Using Google Scholar citation search data, I estimate that their reports have been cited on the order of 750 times as of February 2014.

I have been able to find only very limited background information on Janet Carpenter, the author of the second pro-GMO report. The author information for this report gives only a PO box in Boylston, Massachusetts, and the conflict of interest disclosure states that the report was funded by CropLife International, an agricultural biotechnology industry organization. She appears to be the owner and sole employee of J E Carpenter Consulting, LLC, which is based in Boylston. As of February 2014, Google Scholar lists 73 citations to Carpenter's report.

Testimony to the US House of Representatives Science Committee — undated but while she was associated with the National Center for Food and Agricultural Policy — states that Carpenter has a MS from the University of Maryland in Agricultural and Resources Economics (Gianessi and Carpenter, N.D.). The biographical blurb for an opinion piece in *The Guardian* runs, in its entirety, as follows:

> Janet Carpenter is an independent consultant based in Massachusetts, USA. Previously, she worked with USDA, USAID and the National Center for Food and Agricultural Policy. (The Guardian, N.D.)

The National Center for Food and Agricultural Policy describes itself as "foster[ing] and conduct[ing] objective, non-advocacy research, analysis, and education to inform public policy on food, agriculture, natural resources, environmental quality, and rural economics" (National Center for Food and Agricultural Policy, N.D.[a]). In 2003, this organization published a special project on the benefits of pesticide use, funded by CropLife America and reviewed by a variety of agricultural and biotechnology industry organizations (National Center for Food and Agricultural Policy, N.D.[b]). Carpenter is not listed as an author or contributor to this report; though, at about the same time, she coauthored a report on plant biotechnology and pest management with both of the authors of the pesticide special project (Gianessi et al. 2002).

According to a website biography, Doug Gurian-Sherman is a senior scientist at the Union of Concerned Scientists [UCS] who has previously worked at the Center for Food Safety, the US EPA, the US Patent and Trademark Office, the FDA, and the USDA ("Doug Gurian-Sherman" N.D.). He received his PhD in Plant Pathology from UC Berkeley in 1990. The UCS describes itself as "put[ing] rigorous, independent science to work to solve our planet's most pressing problems"; it is a registered nonprofit advocacy group concerned with environmental issues (Union of Concerned Scientists, N.D.).

Gurian-Sherman's report "Failure to Yield" was published by the UCS online in April 2009; the acknowledgements thank several charitable foundations for their financial support. There is no indication that the report was peer reviewed. A Google search finds on the order of 53,500 websites referencing the report; a Google Scholar search finds 69 scholarly papers citing it; note that this is on the same order of magnitude as Carpenter's report.

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