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CONSTRUCTIVE EMPIRICISM NOW¹

INTRODUCTION

Constructive Empiricism, the view introduced in *The Scientific Image*, is a view of science, an answer to the question “what is science?” Arthur Fine’s and Paul Teller’s contributions to this symposium challenge especially two key ideas required to formulate that view, namely the observable/unobservable and acceptance/belief distinctions. I wish to thank them not only for their insightful critique but also for the support they include. For they illuminate and counter some misunderstandings of Constructive Empiricism along the way. That leaves me free to focus on those two main challenges.

The three of us share a good deal of common history. So it is perhaps only remarkable, and not astonishing, that we now share a common leaning to Pragmatism in philosophy. Of us three I am clearly the most conservative in this respect, especially as pertaining to truth, reference, and belief.² Arthur Fine showed very nicely how Constructive Empiricism could have been conceived under the canopy of Dewey’s Instrumentalism. Much of it could appear as a Corollary to that sort of Instrumentalism, I agree. But in fact I would not be happy to land in that general Pragmatist position.

I. LIGHT IS NOT OBSERVABLE

As my example of something unobservable I will take light. I say “something” but of course it is crucial for me that if it is unobservable then it is possible to be agnostic about its reality, even possible to say that it does not exist. We have the noun “light” and the way we speak betrays the way we think of it, sometimes as a kind of



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stuff and sometimes as something travelling. I will “speak with the vulgar” as Berkeley would say, for the time being.

Would someone please turn the light on and off. Thank you; so you all saw the difference in illumination when the switch was flipped. The tables, chairs, walls are clearly visible when the light is on. But what about the light itself? Searchlights, car lights and flashlights produce visible beams; but they do so in our dirty air, not in space, nor even in pure air. A physicist I know, Arthur Zajonc, constructed a tabletop demonstration of this.³ A light bulb and lenses are so set up that light shines directly into a box without illuminating either anything inside the box or the interior walls. There is a viewport; if you look into it, you see only pure black absolute darkness. But there is a little lever to move objects into view: these are brightly illuminated. They are made visible, but the light itself you cannot see, touch, smell or hear.

There has always been reason to think of light as a sort of thing that travels and/or fills the spaces it travels into or through. The ancient Greeks already developed a geometric optics in which light travels in straight lines. They knew that a concave mirror can so focus the sunlight on wood as to heat it up and set it aflame. So the atomists theorized that light consists of very small fast travelling particles. These particles were supposed to be so small that they can go through miniscule, also invisible, pores in the glass. Aristotle mentions this theory in the *Posterior Analytics* (II, 11; 94b, 27–32) and in *De Anima* (II, 7; 418b, 20–24). But in the latter Aristotle also gives his own alternative account.

He rejects the atomists’ theory first of all because he has concluded that if light traveled, it would be travelling faster than any material body can travel. When the sun comes up, the whole Earth is illuminated from East to West, with no detectable interval. As second reason he mentions that if light is a thing then that thing can occupy the same place at the same time as air, glass, or water; but distinct bodies cannot do so (48b, 17–18). Thereupon he gives the following rival account. Things are visible only if there is an intervening medium. That medium, he says, is something very fine, present in bodies or stuff which can be transparent, such as air and water. It is also found in the uppermost ethereal shell of the physical Cosmos. So “ether” is the right word. Fires and the sun can affect

such ether containing bodies so as to render them transparent. That is when objects become visible to us. Light is neither fire nor a body nor an efflux of any kind of body. To say that light is present is to say that the intervening medium is in a certain kind of state, namely, transparency.⁴

Compare this with the 19th century theory that brought light under the canopy of electromagnetism. Place iron filings on a sheet of paper above an electromagnet; then turn the magnet on. Suddenly the filings arrange themselves in a pattern of lines of force. What happened? According to the theory, when you pressed the switch, you affected the state of the very fine all-pervasive medium, the ether. This new state is manifest in the arrangement of the filings. Similarly, turning on the light affects and alters the state of this ethereal medium. This altered state is manifest in the illumination and visibility of the objects in this room. You see how similar the electro-magnetic field theory is to Aristotle's, in broad outlines.⁵ Both imply that there is no such *thing* as light.

What is the point of this? If you followed the example, you were throughout making a distinction between the observable phenomena and the theoretical entities assumed or introduced in scientific accounts of those phenomena. I used an example at a theoretically low level so that you could say to yourself:

Yes, I believed that light exists, that it is real, that it is a sort of thing. But without denying the ancient observable phenomena, or the success of a particle theory of light in fitting those phenomena, I could still have been agnostic about the existence of light. I can see that precisely because I now appreciate that light is not observable. If it is a thing at all, it is something unobservable.

If you can say that to yourself, then you are drawing the observable/unobservable distinction where it matters and in a way that it matters. In *The Scientific Image* and again in my reply to critics in *Images of Science*, I presented this distinction more theoretically and less rhetorically. But it would gratify me most if you should see yourself drawing it in practice.

II. SO-CALLED "OBSERVATION BY INSTRUMENTS"

We can detect the presence of things and the occurrence of events by means of instruments. But in my book that does not generally count as observation. Observation is perception, and perception is something that is possible for us without instruments. I want to extend my reasoning on this topic by means of a long and careful look at just what it is that we do in fact by means of those instruments that appear to disclose the unobservable.

Window or Engine?

The most appealing argument to undermine the above line of reasoning submits that our situation has changed radically since Aristotle, because of the development of instruments of observation. This change started after the Renaissance with the development precisely of optical instruments: the microscope and telescope. Catherine Wilson's book *The Invisible World* (pp. 57–65, 218–220) describes how enthusiastic the English scientists of the 17th century were about this achievement. One writer remarked that of course the ancients would have greeted the atomic theory with skepticism, because they lacked optical instruments. Soon the microscope would be showing us those atoms, the inner works of nature. This claim was echoed by Robert Hooke, and his contemporary Henry Power even added that we would get to see 'the Solary Atoms of Light' (by means of the optical microscope, mind you!). The microscope, in other words, was conceived of as a sort of window into the invisible or sub-visible level of nature.

We still tend to think about it that way, and we now add into this family of windows such devices as electron microscopes, spectrometers, brain scanners, particle accelerators and so forth. I want to persuade you to think of them in a different way. Each of these devices too creates new phenomena, truly humanly observable phenomena. Their importance too lies in our use in the systematic creation of new phenomena that must also be saved by our theories, and suffice to refute theories to be discarded.

This is meant to be a change in view: assimilate those instruments as well, not to windows into the nether world, but to experimental arrangements that produce telling new effects for us to see, and for

us to give a place in our representations of the world. The instruments used in science can be understood as not revealing what exists behind the observable phenomena, but as creating new observable phenomena to be saved.

The Phenomenology of Microscopes

Paul Teller comments on this ‘phenomena creating’ view of instrumentation. He thinks it is convincing enough when applied to certain instruments, such as spectrosopes, voltmeters and bubble chambers. But he rejects it for the microscope. The basis on which he rejects it is quite different from what I have seen in earlier arguments. As he points out, there is a difference between those which produce ‘straight’ phenomena (such as electric discharges, electrically produced heat, etc.) and those which produce images. For Paul suggests that if we look attentively into the experience of seeing with and without microscopes, the phenomenological kinship is so close that we lose the observation/instrumentation boundary. This argument is part of his urging, in several different ways, that we should look much more closely into observation itself. He points to its phenomenological character and to its information-gathering role as especially important aspects.

I accept the critical point. But I am not ready to accept the conclusion, so I will respond with a closer look at both observation and the creation of new phenomena. I hope to go a little further toward a better, broader as well as more articulate view of observation to do my part.

Public Hallucinations

The little experiment I described earlier with the electromagnet produced a new phenomenon. From a realistic point of view you can describe it as a detector for the electromagnetic field, as a window allowing us to look into those mysterious lines of force in the ether. In theory it was certainly of a piece with optical phenomena. At the phenomenological level too it was of a piece with such phenomena as reflection and refraction which happen spontaneously in nature, although this phenomenon was artificially produced. Now I want to direct attention to a different sort of phenomenon that nature also

produces spontaneously. I will suggest that the analogous point for microscopes should be made with respect to those phenomena.

The examples I have in mind are optical phenomena too. They include reflections in the water, mirages in the desert, and rainbows.⁶ The subject is once again ancient: Plato's *Sophist* struggles with the questions of how or in what sense reflections in the water are real or unreal and Aristotle began the theory of the rainbow in his *Meteorology*.

The first point about these, as about light itself, is that we use nouns. In fact we use count nouns; we talk about them as if they were things.

The second is that the phenomena themselves show that we are wrong to do so. They refuse to allow us to represent them to ourselves as things, or even as properties of things in any straightforward way. Consider the rainbow. We realize pretty soon that there is no real material shining arch standing above the earth, although at first it looks that way. As a second guess we might think that certain parts of the clouds or haze are colored. But that cannot be maintained because if we move, we see the rainbow in a different location on that cloud or haze background.

In fact, we realize then that our usual way of speaking involves us in falsehoods. I see a rainbow and you say *you see it too*. See what too? You are not seeing the rainbow I see, for yours is located in a different place. (I assume that you are not looking from inside my head.) If I say there are two rainbows, and you agree, we are not even counting the same things; in fact, we are not counting things at all.

But thirdly, we are not hallucinating. Hallucinations are private, subjective. These observations are like hallucinations, in that they are not of real things, but they are public. *Nature creates public hallucinations*. So public, in fact, that the camera captures them too. The observations are scientifically significant in part because they can also be made indirectly, so to speak, with the camera as instrument.

Let me put all this a bit more technically. In some sense we do represent shadows, moving spots of light, reflections, mirages, and rainbows as things. But the phenomena themselves refuse to let us maintain this representation. The reason is that certain crucial

invariances are lacking. If the rainbow were a thing, the various observations and photos would all locate it in the same place in space, at any given time. However, there are significant invariants here that dreams and after-images lack. In the case of rainbows this invariance is found in the relations between sun, cloud, and location of the eye or camera. The subtended angle is always 42 degrees, with that location between sun and cloud. The larger situation exhibits invariances that allow us to represent it to ourselves as a structure in nature independent of our subjective experiences.

A New Sort of Engine

Nature creates these public hallucinations. Already in ancient times, concave mirrors and lenses were used to do the same. It took more than a thousand years before this art of imitating nature provided the resources for a systematic exploration of nature. That is how I suggest we should understand the microscope (or at the very least the electron microscope).

There is an immediate objection to this. The images produced by the microscope we can represent to ourselves as images of real things (with the same structure as those images). In fact, we can represent what we see as indirectly observed real things behind the microscope's lenses.

I agree. The similarity I am pointing out (and which I propose as basis for a possible way of thinking about microscopes) is not a similar lack of invariances in comparison to real objects. The success of that family of instruments (microscope, electron microscope, radio telescope) derives in part from the possibility of representing their products as images of real things existing independently of any relations to those instruments. *But their products are images*; they are optically produced, publicly inspectable images. It is these images that are like the rainbow (they cannot themselves be represented as independent things). The difference, that we cannot think of the rainbow as image of a real arch, while we can think of the microscope image as of a similarly structured object, is important but irrelevant for my point.

Paul Teller objects that if you have your eye glued to the microscope you do not have the experience of seeing an image. I agree,

but do not agree to the conclusion he wants to draw from this. I have two replies to his point:

- (A) The microscope's output can be sent into a scanner which transmits to a computer or projector – then we see the paramecia on the wall or the monitor. We are having a different sort of experience then, for we say after only a little urging that we are seeing an image. Nothing about the status of the microscope can follow, it seems to me, from concentration on one of these three experimental arrangements to the exclusion of the others.
- (B) Well, no more do we see a rainbow-image when we, as one says, see a rainbow. We never see images, because images do not exist.

But, it may be objected, don't we have the experience of seeing an image, as distinct from seeing a real thing, and aren't those kinds of experiences different?

Since we can't see things that don't exist, the phrase "seeing an image" is code for something we are describing metaphorically or analogically. It is similar to "Macbeth saw a dagger" in the scene where he reports that sort of experience although there is no dagger there. Let me explain how I understand this.

My experiences are the events that happen to me of which I am aware. Such an event has two sides, so to say: what really happens to me and the spontaneous judgement I make in response, which classifies that event in some way. In good cases the two coincide, but often they do not. For example, I trip over a marmot but take it to be a cat. What happened to me was that I tripped over a marmot, but I "experienced it as" tripping over a cat.

We classify someone's experience as the experience of seeing an image of an X in three cases:

- (a) if we judge that s/he is seeing a real thing that we classify as a 'picture' of an X (this would apply if the microscope is hooked up to a projector or monitor);
- (b) if s/he judges that s/he is seeing a real X, and we take that to be an illusion, or hallucination, whether private or public;
- (c) if s/he judges that it is as if s/he is seeing a real X but s/he takes that to be an illusion.

If the someone is oneself, obviously the second case cannot happen. Also in case (c) we might sometimes have to add: s/he is having the experience of seeing an X-image, but is mistaken: s/he is seeing a real X.

In other words, this “seeing as an image” is a code for a classification of experiences that refers to that spontaneous judgement on the experiencer’s part, as well as to what really happens to that person. Sometimes of course we can’t be sure whether it is an illusion or not; the question of how to classify it becomes a factual question, and we may or may not be content to remain agnostic about it.

What then is the important fact that Teller is pointing out about looking into a microscope? It is that he spontaneously judges that he is seeing e.g. real paramecia, and that he has no inclination to correct that judgement as illusory. He is quite rightly contrasting that sort of experience with such experiences as those of seeing rainbows, mirages, or reflections which are not delusory. In other words, he contrasts it with experiences in which the spontaneous judgement includes a classification of that very experience as what I would call a public hallucination. But this difference pertains in the first instance not to what is really happening to him, but to his response to what is happening to him. That is, it pertains not to the first but to the second side of the two-sided thing an experience is. The question whether the experience of ‘seeing’ in a microscope is or is not a public hallucination is not settled by this. Instead, that question becomes a theoretical question about what happens in the optical microscope.

To sum up then: in the case of images I want to describe a division into several kinds and subkinds.

- *Type 1.* On one side are the images which are in fact things, such as paintings and photos.
- *Type 2.* On the other extreme are the purely subjective ones like after-images and private hallucinations. These are personal, not shared, not publicly accessible. Indeed, we are pretty clearly dealing there with discourse that reifies certain experiences which are “as if” one is seeing or hearing.
- *Type 3.* In between these two are a whole gallery of images which are not things, but are also not purely subjective, because they can be captured on photographs: reflections in the water,

mirror images, mirages, rainbows. For those I will use the term “public hallucinations”.

Some of these public hallucinations are actually pictures of real things: e.g. the reflection of a tree in the water. Some are not; e.g. the rainbow. But of those which are not, some would lend themselves to being conceived of or identified as pictures of real things. If an image would so lend itself, we can ask: *is it really a picture of something real, or is it not?* That is always a question of fact transcending the experience itself.

A Further Objection: The Force of Reason

I can imagine a further objection: if we can represent the image to ourselves as an image of a real thing, should we not do so? Is it not perverse to say: all those images are such that *it is [only] just as if* they are images of real things, as close to their sources’ structure as are reflections in water and mirror images? Among philosophers at least there appears to be a conviction that in science, when certain images are produced by instruments, and these can be conceived of or identified as pictures of real things, we must (if we accept the associated theories) believe that they are, i.e. that there are real things of which they are the pictures.

I contest the “must”. Of course there is one major difference that springs to the eye. If you see a reflection of a tree in the water, you can also look at the tree and gather information about the geometric relations between the tree, the reflection, and your vantage point. The invariances in those relations are precisely what warrant the assertion that the reflection is a picture of the tree. If you say similarly about the microscope’s images that they are pictures of e.g. paramecia, then you are asserting that there are certain invariant geometric relations between the object, image, and vantage point. But now you are *postulating* that these relations hold, rather than *gathering information* about whether that is so.

To counter the sense of perversity in our resistance to any disanalogy, I must make the agnosticism more appealing and more natural. The best way would be to give you familiar examples in which we have much better bases for reification than the rainbow, and still end up admitting the possibility of such agnosticism. But I think we may all be too sophisticated for that now.

However, scientific realists do have examples of this sort. Concisely put: if science is true, there are real examples of precisely that sort. To make the logical point, that is enough. I am thinking here of elementary particles. For about a century now the general educated public has conceived of nature as built up out of sub-atomic particles held together by various forces. Science began to represent nature in this way around the second-last turn of a century. Rutherford's gold leaves, Millikan's oil droplets, Perrin's experiments, Einstein's photo-electric effect, Bohr's atom Pretty well everyone knows a lot of the history which solidified that picture of nature. The phenomena observed and produced *not only allowed but invited* their representation as manifestations of that sort of building-block universe. The particles of which these phenomena produced indirect images displayed the requisite invariances required for representation as things: enduring, moving, massive individuals.

But half a century or so later, particle-number turned out first of all subject to superposition, fading in and out of definiteness, and secondly, not relativistically invariant either. Philosophers of science that I won't name (but some participated in this symposium) question those particles' claims to individuality and haecceity. I would especially suggest that if the number of particles can vary from one frame of reference to another, then they are after all rather like shadows and rainbows. So indeed, certain images had been produced, by means of instrumental arrangements, that could be conceived of as pictures of real things. Arguably, they still admit this conception now. But equally arguably it is better still not to insist that if they admit of that conception then we must (ought, had better) conceive of them that way!

What about the force of reason? After someone has had to accept a correction, we may well hear him say: "I was wrong, that is true; but at the time I could not have believed anything else, and I had to believe what I did believe then. For I could not have foreseen the later evidence." Just think of Poisson when first faced with Fresnel's version of the wave theory of light. Poisson was convinced of the particle theory, and set out to refute Fresnel by designing a now famous experiment. It is now famous precisely because it provided telling evidence for Fresnel's theory. So we can imagine Poisson saying precisely the above: he had to admit that he had been wrong,

that his earlier belief was false, but could maintain that no rational person could have believed any differently.

I submit that this is in fact not so. Perhaps, in view of the evidence at the earlier time, a forced choice between Newton's and Fresnel's theories of light would have been won hands down by Newton's. But the choice is not forced. How much we believe, going beyond our evidence, is to some extent up to us. Poisson was not irrational to believe as he did, but to have been agnostic would not have been irrational either. To say otherwise would land us in the uncomfortable position of maintaining:

We may sometimes be rationally compelled to believe hypotheses which are in fact false, and will later be able to say truly that we were in a position in which rationality forbade disbelief in something that we now classify as false.

I say that this is uncomfortable, although I realize that it is implied by any epistemology according to which e.g. induction or other 'rules of right reason' determine a uniquely correct belief (neither too strong nor too weak) on the basis of the given evidence. That has indeed been a classical sort of position in epistemology. I find it unacceptable, and I think of it as a flight from personal responsibility for one's chancy choices.⁷

Summing Up

I submit that without stretching ourselves very far, we can report on our sightings through a microscope in the same way that we report our rainbow-observations. This certainly introduces a sort of observation report that I did not have in mind while writing *The Scientific Image*. Then I was thinking quite simply in terms of a classification of objects, events, and processes as observable and unobservable. Rainbows are not objects, events or processes. Our use of the noun hides from view the more sophisticated understanding that we'll immediately display when pressed. But our common way of speaking has not actually changed. I think we can relate to our experiences with microscopes in the same way. This despite the fact that, in one straightforward sense, we do not have the experience of seeing a rainbow-image, nor of a paramecium image, but of seeing a rainbow and paramecium.

Finally, I should add that I really don't mind very much if you reject this option for the optical microscope. I will be happy if you

agree to it for the electron microscope. For optical microscopes don't reveal all that much of the cosmos, no matter how veridical or accurate their images are. The point of constructive empiricism is not lost if the line is drawn in a somewhat different way from the way I draw it. The point would be lost only if no such line drawing is considered relevant to our understanding of science. Arthur Fine certainly, and possibly Paul Teller as well, suggest that there is no relevant such line to be drawn.

I, on the contrary, see such a line appearing in a number of contexts, not solely in the debate over theoretically postulated entities. Here is a quote from Steven Weinberg, in his 1998 review of Kuhn's work:

It is important to keep straight what does and does not change in scientific revolutions, a distinction that is not made in [*The Structure of Scientific Revolutions*]. There is a "hard" part of modern physical theories ("hard" meaning not difficult, but durable, like bones in paleontology or potsherds in archeology) that usually consists of the equations themselves, together with some understandings about what the symbols mean operationally and about the sorts of phenomena to which they apply. Then there is a "soft" part; it is the vision of reality that we use to explain to ourselves why the equations work. The soft part does change: we no longer believe in Maxwell's ether, and we know there is more to nature than Newton's particles and forces.

The changes in the soft part of scientific theories also produce changes in our understanding of the conditions under which the hard part is a good approximation. But after our theories reach their mature forms, their hard parts represent permanent accomplishments.⁸

Weinberg is not exactly known for sympathy with philosophy.⁹ In any case, I want to submit a challenge to philosophers, in Weinberg's terms: *if you are going to distinguish between a hard and soft part of science, in some such way, tell us how you draw the line.*

You can't get out of this by pointing out that there is a continuum on which the line is drawn, or that the line will be drawn differently in different contexts, historical or social. For that is the case for all or almost all distinctions we make, and does not make those distinctions unreal or unimportant for understanding. Contrary to Sextus Empiricus there is a real distinction between touching your mother's toe with your little finger and having incestuous intercourse, even if the difference is a matter of degree, and the line is drawn differently in different contexts.

III. ACCEPTANCE AND BELIEF

In *The Scientific Image* it was hard to stay clear of epistemology, though I tried. As Paul Teller explained, constructive empiricism is a view of what science is, it is not a view about what we ought to believe. Yet at the same time Arthur Fine was quite right that the book helped to shift the focus of debate toward epistemology. Constructive empiricism was going to be a pretty useless view unless it was backed by a suitable epistemology. For example, if the evidence made it irrational not to believe our best scientific theories, then the whole point would be moot. So the book already came with a bit of epistemology, namely whatever was needed to allow for the epistemic attitude of acceptance without belief.

Both my respondents demur when it comes to this belief/acceptance distinction, though in different ways. Teller suggests that full-blooded belief and full-blooded acceptance come to the same thing. He says, "I conclude that . . . all of what passes for what van Fraassen calls belief is really acceptance". Fine does not seem to want to argue that belief is not a distinct category of epistemic attitude. But he advocates a different attitude toward theories, namely simple reliance on them as guide to life, which is closely related to what I called acceptance. So from Fine's point of view, if I understand it, any notion of belief as opposed to acceptance could also as well be discarded.

More than Lip-Service?

If I accept a theory then I believe that it is empirically adequate, and I also commit myself to seeing nature through that theory's eyes. Thus, in addition to that belief in the theory's empirical adequacy, there is a pragmatic aspect to acceptance. Nature is confronted and/or appreciated within that theoretical framework, the theory guides experimental design and new projects for observation, new theories are required to be compatible with it, and so forth. These assertions express commitment rather than belief, though there is obviously some sort of coherence connection between a commitment and opinion about its chances of being vindicated. The accepted theory is thus the guide both to theoretical and practical life.

Suppose that in addition to all this I say that I do not believe the theory to be true. Suppose that I am agnostic about whether it is true; it may, as far as I am concerned, be false in respects that do not affect its empirical adequacy. That may certainly sound a bit hollow; what is that reservation? Is it just a bit of lip service to a, you might say, pious agnosticism?

Of course if we did reach that conclusion, we'd have to say the same about belief. If there is no substance to the disavowal of belief in the truth of the theory as a whole, then there is also no substance in disavowal of disavowal. In that case avowal of belief and agnosticism would be equally vacuous. So I can quite understand that if we reached this point, we wouldn't see any real distinction at all any more.

That Belief is Real and Distinct

I do not agree with Fine and Teller about this. Epistemology has in the past been guilty of a really big sin of omission. There is in fact an enormous variety of epistemic attitudes, with many nuances and distinctions to be drawn. Prior to epistemological controversies we should have had a descriptive epistemology, to canvass this variety. What we have instead at this point is a patchwork, to which items are proposed for addition or deletion from time to time. Some philosophers who began to see subjective probability as important concluded that belief is for dummies. We are all dummies in that sense: we cannot calculate rapidly or precisely enough, so we need rule of thumb shortcuts. That dismisses belief as a real subject. But traditionalists were and are guilty of the opposite extremism. They write as if belief, disbelief, and neutrality are the only epistemic attitudes there are. I do not see any justification for either sort of procrustian measure, any more than for assimilating belief to practical reliance or for regarding it as too ideal an idealization.

First of all, it seems to me that there is a good place within the epistemic enterprise for having *one* picture of which you just say "that is the way things are". Within that picture there remain many possibilities. It rules out a lot of alternatives, that is its function; but it still leaves a lot of alternatives open. It seems to me to be phenomenologically correct, that something has that status for a given person normally.¹⁰ Surely only a Zen master could

reach the epistemic state of having nothing so fixed. What Richard Jeffrey suggested, that we should give probability 1 only to tautologies, I think would need about twenty odd years of strict spiritual discipline.

Within belief there are strata: some beliefs we will revise quite readily, to fall back on other beliefs that have a superior status for us. Our safety does not lie in being faultless; the thing to rely on is rapid response error correction. Trying to describe this, and to outline the logical connections between full beliefs and gradations of belief is a technical project that is admittedly still using rather crude models.¹¹

But meanwhile we can say this much. If two propositions are different to the extent that one could be true without the other, and we realize this, then it is possible to believe one without believing the other. If it is possible to distinguish between the observable and the non-observable, then it is possible to distinguish between empirical adequacy and truth.¹² Therefore I conclude that it is possible to believe that a theory is empirically adequate without believing that the theory is true.

Reasons and Motives

I've argued on other occasions that at a quite ordinary level of understanding we see a difference between belief and acceptance, in terms of what we count as good reasons and what as ulterior motives. If someone says "I believe in angels because and only because I derive comfort from the idea", we reply that there is something wrong. Deriving comfort from the belief does not make it any more likely that angels exist, and the speaker must realize this. The same is true of such considerations as that the theory explains a lot, is audacious, unifies a previous hodgepodge, is beautifully simple, and so forth. All of these are desirable features, however, so they can all be cited as good reasons to make that theory part of the science we accept, as long as we do not see that as endangering empirical success.

Teller points out that we do actually believe a good deal for such "ulterior motives", and that does not make us irrational or culpable. I agree. We have or choose a personal risk quotient (to use Levi's term), which is not the same for all topics or in all circumstances. So we are readier to form or maintain some beliefs, and quite resistant

to others, and we differ from each other in that respect. But I don't see how that affects the distinction. A belief held for ulterior motives is still a belief. It does not become acceptance instead of belief that way. The distinction between what a person believes and what s/he merely accepts is not made on the basis of why s/he has that attitude, but only on the basis of what that attitude is.

Risk and Coherence

After I wrote *The Scientific Image*, and in order to provide it with a hospitable setting in epistemology, I tried to work out a liberal epistemology.¹³ Well, to some eyes it may look not so much liberal as libertine Let me sketch some outlines of this epistemology here.

There are important coherence constraints on our opinion overall. You can't be committed to using a theory as a practical guide if you actively disbelieve that it is practically reliable. You can't be committed to the idea that all theories should now be logically compatible with e.g. quantum theory, and also say that this requirement will likely lead to more and more false predictions. The general point I am making here is all of a piece with the point that you can't claim to have your own beliefs because of reasons that do not tend to make them more likely to be true. These constraints are not ones of purely logical consistency but of a broader sort of coherence.

How strong are those constraints? Perhaps we tend to be uncomfortable with professions of belief precisely because we suspect that the constraints of reason are very strong, that they have to be strong enough to give us a guarantee against epistemic humiliation if we impose them on ourselves. I do not accept that at all. Just consider: I believe many things. I believe that I had a father, that I wrote a book, that we are still in America and not on a flying saucer and so on. Could I be wrong? Are there circumstances under which I would correctly give up some of these beliefs? Certainly. My beliefs could be false, simply because I don't just believe tautologies. And if I can imagine how a belief could be false, I can usually also imagine how I might correctly come to the conclusion that it is false. None of this makes my state of opinion incoherent. And I shouldn't be so scared of possibly being wrong. There is no way to avoid that. Subjective

probabilities can also be all wrong, even if it is a bit easier to fudge that.

Believing is like doing. But so is not believing. If you don't believe something you risk the loss of a valuable, worthwhile way of seeing your own situation and the world you are in. In that respect there is a certain symmetry. Once you recognize the element of choice in the way you fashion your beliefs and opinions, you also see how you might have chosen differently. As I see it, science and its participation do not require all that much belief from us, and while I respect the rationality of those who prefer to have those supererogatory beliefs, my arguments are meant to show the rationality of those who'd forego them.

In our beliefs and other epistemic attitudes I think we are as free as birds in the air. There are no rules of right reason, rationality is only bridled irrationality. But even a bird in the air could sabotage itself, by desynchronizing its wings and tripping all over itself in the air. Coherence means: no self-sabotage. The constraints of coherence are really empty, because they don't limit the factual content of belief at all. The yoke is easy, the burden is light. Yet most of us don't feel it that way, we have quite a lot of trouble trying to look graceful in flight . . .

NOTES

¹ Contributed to the Symposium "*The Scientific Image, Twenty Years After*" at the American Philosophical Association (Pacific Division), Albuquerque April 2000. I want to thank Richard Otte for organizing the symposium as well as Arthur Fine and Paul Teller for their insightful, challenging contributions.

² With respect to truth and reference I am closer to Scott Soames' *Understanding Truth*, the subject of another symposium at the same conference.

³ Zajonc, *Catching the Light*, pp. 2–3.

⁴ Park ridicules and completely fails to understand Aristotle's theory (pp. 14–17, 41–43, 49). But here Aristotle is in good company since Park also misrepresents the violation of Bell's Inequalities later in the book (pp. 324–325).

⁵ If we think of a later field theory, after the demise of the ether, we will say that turning on the field alters the state of that region of space. This is, in relevant respects, not so different from Aristotle's theory, though the ether theory is obviously closer. In neither case does light exist, literally or strictly speaking.

⁶ The rainbow is unlike reflections in the water because it is not the image of some real arch. That is important to illuminate the point below. But the more important feature is the status they share with mirages (and share, I will argue, with microscope images), which makes them “public hallucinations.”

⁷ I don’t think it gets any better if the determination is not of a full belief but of a subjective probability judgement. In that case the implication would be that we have sometimes been in a position in which the only rationally permissible state of opinion was one that was totally miscalibrated, and that we now classify as thus misaligned with the facts. My diagnosis would instead be that we were not unreasonable to have had such an opinion, but at the same time reason did not compel that choice.

⁸ Steven Weinberg, “The Revolution That Didn’t Happen” *The New York Review of Books* XLV, 15: Oct. 8, 1998: page 50.

⁹ However, his footnote at that point indicates that he wrote this passage in response to a comment by Christopher Hitchcock.

¹⁰ “Has” is misleading: belief is a continuing project of construction. But phenomenologically it does not feel that way, because we are not very transparent to ourselves. That is why we say, “I don’t know what I think about that, let me reflect on it, I will try to figure out what I believe about it”.

¹¹ See further my “Fine-grained belief . . .”

¹² A quick technical point. To get it right about the observable part of the world, must a theory get it right about whether clouds or mountaintops are electrically charged during a thunderstorm? To answer this question you have to first ask: does “x is electrically charged” imply the existence of unobservable objects? The relation of *implication* is a function of the semantic structure of the language in which the statement is formulated.

¹³ See for example the sections on epistemology in *Laws and Symmetry* and “The False Hopes of Traditional Epistemology”.

REFERENCES

- Churchland, P.M. and Hooker, C.A. (eds.) (1985): *Images of Science: Essays on Realism and Empiricism, with a Reply by Bas C. van Fraassen*, Chicago: University of Chicago Press.
- Fine, A. (2001): ‘The Scientific Image Twenty Years Later’, *Philosophical Studies* 106, 107–122.
- Park, D. (1997): *The Fire Within the Eye*, Princeton University Press.
- Soames, S. (1999): *Understanding Truth*, New York: Oxford University Press.
- Teller, P. (2001): ‘Whither Constructive Empiricism?’ *Philosophical Studies* 106, 123–150.
- van Fraassen, B.C. (1995): ‘Fine-Grained Opinion, Conditional Probability, and the Logic of Belief’, *Journal of Philosophical Logic* 24, 349–377.
- van Fraassen, B.C. (1989): *Laws and Symmetry*, Oxford: Oxford University Press.

- van Fraassen, B.C. (2000): 'The False Hopes of Traditional Epistemology', *Philosophy and Phenomenological Research*, 253–280.
- van Fraassen, B.C. (1980): *The Scientific Image*, Oxford: Oxford University Press.
- Wilson, C. (1995): *The Invisible World: Early Modern Philosophy and the Invention of the Microscope*, Princeton, N.J.: Princeton University Press.
- Zajonc, A. (1993): *Catching the Light*, Oxford University Press.

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