UPDATED SCIENTIFIC-METHOD

VERSUS

CRUCIAL GAPS IN OUR THEORY

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ABSTRACT

This paper seeks to report developments in "ToK" (the Theory of Knowledge) and, where feasible, apply them to long-standing problems in physics. One key issue is to explain the nature of observation/perception, and hence criticize the 1900s' emphasis on experimental observation as harmfully overzealous empiricism. Meanwhile theory (and hidden-but-indispensible processes) have both been undervalued and their processes misunderstood.

Much of the impetus for this work came from the late Professor J.Piaget, best known for the impact of his work on developmental psychology; but he also argued that similar principles apply to *society-as-such* (including the *collective scientific-world*) — and that amounts to a new approach to "Scientific Method" which has been used with apparent success in the biological sciences closest to physics, including neurophysiology.

The account concludes with a brief application of this approach to two old problems in physics (Special Relativity, and Wave/Particle indeterminacy) — meanwhile noting other work which seems to have produced plausible models by actually breaking those "taboos" criticized here.

CONTENTS

1: Questioning the Foundations

I will suggest that the main foundational weaknesses lie, not so much in physics itself, but rather in our methodological rules, and our social assumptions. I do offer brief criticisms of physics "findings", though most such points have surely been made many times already. However perhaps my method-and-context analyses may add somewhat to existing commonsense criticisms. At any rate, I have found such analyses useful in attacking comparable "impossible" problems within the biological sciences.

1.1: Conflicting cultures can hamper progress

This FQXi project effectively invites us to think '*outside the square*' about a '*hard-science*' topic — so it might help to be aware of a cultural clash here, which may be hindering discussion. Pask & Scott (1971/2008) showed conclusively that (at least in some teaching environments) there are at least two different cognitive styles which they called "**holists**" (typically story-tellers and Arts-faculty types, who appreciate embellishments and side-track thinking), and "**serialists**" (who like to keep to strictly logical one-track paths, typically engineers). Holist teachers with holist students: — that works fine. Likewise Serialist teachers with serialist students. But in a mis-matched classroom, there is much less success.

I have sometimes thought that physics needs more holists, for that very purpose of thinking 'outside the square' — but any typical page of unexplained one-track mathematics is likely to scare them away prematurely! I like to think I have a foot in both camps (R.Traill, 2000: *"Physics and Philosophy of the mind"*!) , and no doubt other physicists can also wear two hats. However we might like to check whether a serialist collective-ethos drives the *policy-makers, editors,* and *accountants* who make the most crucial decisions.

[*A side-issue now, which serialists should bypass!*]: Meanwhile I suggest that, in publications, there should be more insertion of side-panels explaining mathematical context etc to the non-specialist holist, — panels and illustrations such as those offered in *Scientific American*. There used to be the excuse of limited journal space — hence the irritating habit of leaving out large sections of "obvious" calculation. But now with the boundless publishing space of the internet, that excuse looks very weak. I like to think I heeded this advice when I explained the details of an optics problem at length, firstly in www.ondwelle.com/UV2_trigger.pdf (2010) and then more briefly in the subsequent mainstream publication (R.Traill, 2011b) — but with appropriate back-reference.

In short, PHYSICS may be too much of a closed shop — jealously guarding its patch via its own 'secret' language — and I welcome this opportunity for serious critics to have their say.

1.2: What sort of "plumber" might best treat the "blocked pipes" of physics?

Mainstream Physicists will obviously be involved in further tackling of impasses; but they alone will not suffice because (by definition) they will have already failed to resolve these problems. Sometimes useful leads can come from **maverick physicists** like David Bohm or Herbert Dingle — though such rebels may not always have a deep understanding of the overall position which includes social forces and age-old philosophical issues.

In 1972 Dingle himself wrote (regarding problems with Special Relativity Theory: "SRT"):
"the difficulty, I believe, lies only in the discarding of false notions that have been automatically accepted as true, and not grasping the actually true ones. It is therefore largely *psychological*, and, *being no psychologist*, I can only record what *commonsense* indicates concerning the various attitudes which physicists have adopted towards criticisms of the theory." [Ch.6, p121; — *my italics*, RRT]

So should we ask psychologists to analyse impasses in physics? Well maybe, though we should recognize that there are at least three different psychological specialties involved here:
(i) **Social psychology** including peer-pressure and funding-patronage, not to mention pressures from the media. (ii) **Perceptual psychology**, especially details of just what is involved in that concept of *"observation"* which is so highly regarded by most physicists.
(iii) **Cognitive Psychology** — what it is to "know" something, and how that state is achieved.

This latter *cognitive issue* leads us into the somewhat broader domain of **Epistemology** (or "**T**heory-**o**f-**K**nowledge") — and that takes us beyond Dingle's dependence on "commonsense" (which still has its uses, as we shall see), into a more systematic considerations about knowledge and its acquisition. Conveniently this discipline deals with both ● how an individual acquires and stores knowledge, and also ● how society/science-establishment gains-and-stores its knowledge — i.e. "Scientific Method" — and that seems to be very much what we are looking for here. (Note that *individual* knowledge is different from *socially-accepted* knowledge — just ask Copernicus or Galileo! More about this later.)

The version of epistemology discussed here, is that of the late Professor JEAN PIAGET (1896-1980) of Geneva. He is best known for the somewhat controversial application of his theories to the schoolroom — but that need not concern us here. His real importance lies in his more-than-usually explicit account of how concepts must be built up within the memory &/or utilized from hereditary sources — notions which have since been developed further (R.Traill, 2008, 2011a). Moreover he also tackled the *Scientific Method* aspect of epistemology, so his work seems directly relevant here: (Piaget, 1949, 1970; Piaget & Garcia, 1983).

Unfortunately his prolific works are not very readable. For those unfamiliar with with his approach, a useful starting-place might be my recent PowerPoint presentation: www.ondwelle.com/MolecularScheme.ppt plus its notes-and-references: www.ondwelle.com/MolecularSchemeNotes.pdf — (R.Traill, 2012) — That presentation was addressed to a rather different problem, though the comparison may be useful — as follows:

2: A Reconstructed Approach to this Knowledge Theory

2.1: The Paradox of "Epistemological Impossibility"!

The fundamental paradox which underlies epistemology is that ***some*** minimum pre-existing knowledge seems essential, just to make some sort of sense of the jumbled cacophony of signals received from the environment. But then how do we explain that "initial minimum pre-existing knowledge"? Does it just come out of no-where, or what? As individuals, we do certainly inherit some learning skills (and I have been particularly interested in the likely mechanisms); but at some stage in our evolutionary past, each such inherited "sub-skill" has had to be learned the hard way — and that usually means by brutal Darwinian trial-and-error for our distant ancestors.

To put it another way: It is like explaining how *life* could have started — a transition which seems impossible if we believe that life always needs some *pre-existing* life, just to get it going. In fact that is no mere analogy, because life itself is an embodied list of DNA-based *knowledge*-instructions, usually assisting the owner to negotiate an unreliable world. (R.Traill 2011a, 2012, plus Chapter 4 of 1999 — and references to Popper, Jerne, and others cited in those works).

Note that by now we all have a vast store of often-trivial skills of which we are usually totally unaware. Some of them we acquired in infancy, but others must be genetic. — And if you doubt the importance of such ready-made repertoires in even the simplest tasks, just try using basic "machine language" programming to get a naïve robot just to move its arm in an orderly way, and then how to hold a cup! That sounds easy, and even insects can do that sort of thing unconsciously — but sometime try to formulate it systematically yourself, starting from scratch without using any pre-written subroutines.

2.2: Facing the Paradox

The important point here is that this hidden array of skills is both a blessing and a curse. Obviously the skills are usually **very useful**, given that they have survived evolutionary pressures (whether Darwinian, or our practical learning processes in infancy). On the other hand, even if we are explicitly aware of such skills, we **may not know or understand their "pedigree"**, and so we are encouraged to avoid applying them in serious research. — And in avoiding them, we are sometimes tying our hands behind our backs unnecessarily, though we should obviously use explicit caution if we do follow such leads — like Kekulé's "intuition" about the benzene molecule being ring-like, or my tentative interpretation of scattergrams (1999, Chapter 6). Of course such intuitions could turn out to be misleading (and I will explain later why I think *Einstein's intuition* of 1905 was actually wrong); but I think we have to accept that all scientific conclusions must always be considered as remaining "on probation" —– though that is not as bad as it sounds, as can be explained in terms of *successive approximation* (Piaget & Garcia 1983; R.Traill 2012).

A more serious curse is that we are often so totally **unaware of our own hidden skills**, such as "commonsense" and social skills, that we are in no position to make allowances for their fallible aspects. We might just assume we are somehow "naturally clever" (and that may even be a valid interpretation!); but in putting it that way, we come close to invoking occult inexplicable powers, and that is usually unhelpful in a scientific context. The danger is not in using such untamed powers (with due caution), but in our failure to see that we are *depending on hidden assumptions* which cannot actually be rigorously justified to the standards we profess. As Piaget (1949) put it:

"Whenever one reads the main 'logisticiens', such as Russell, Wittgenstein, Carnap, etc., one quickly realizes that they all depend on certain intuitions: — intuitions which are taken for granted, exactly in proportion to the extent that they evade verification."

One practical lesson from all this is that we should not try to be too purist in any well-meaning quest for rigorous methodology. That is where Logical Positivism failed in its worthy endeavour, as one of its leading apologists (A.J.Ayer) admitted on TV in 1978; (R.Traill, 2000, pp3-7).

3: Precedent: Epistemology tackles Impasses in Psycho-biology!

3.1: When Observation Fails Us

It might not be immediately apparent that physics and psychology have a methodological issue in common! The important point is that both offer problems where "observation" is effectively impossible — and yet during most of the 1900s we were expected to depend on nothing but observations! So what was one supposed to do? In **physics**, the Copenhagen convention took the Heisenberg indeterminacy so seriously that *Operationalists* insisted that it made no sense to even contemplate substructure or (hidden) determinacy within quantum phenomena.

Meanwhile **psychology** (keen to keep up with "big brother" physics) was rife with *Behaviourists* like B.F.Skinner (1972), who refused to consider the legitimacy of such notions as "consciousness", because there was no way of observing or measuring them.

Yet commonsense suggested otherwise in both cases. Of course commonsense can be wrong, but so can rules such as "insist on observability" (and indeed the supposed validity of that rule does not always pass its own test — suggesting it is inconsistent, a serious flaw as we shall see). In fact there are many situations in like where observation is impossible, if only temporarily. An important childhood lesson, much studied by Piaget, is to learn that a toy which disappears behind an obstacle is probably still there.

Likewise in stone-age times one gained more-or-less confidence that the Sun would not vanish permanently at sunset! [1]

3.2: In fact observation is never fully rigorous anyhow!

To neurophysiologists and to epistemologists like Piaget, human observation is a complicated process which depends heavily on *internal processing*, and not just on "photography" at the retina.
To make actual sense of the visible world, we have to interpret the existing evidence via a search for *internal self-consistency* (or "equilibration" as Piaget would put it). This becomes clear when we look at a deliberately-ambiguous drawing such as Wittgenstein's "duck/rabbit" — and we find our interpretation of it flipping from "duck" to "rabit", and back again, in our attempt to to find the best equilibrated solution to reconstructing "reality". In real life such visual ambiguity is rather rare, so this process usually works very well, even though (i) it is not as rigorous as it seems, but (ii) mostly we are totally unaware of the *Hereditary+InfantExperience* basis behind the "programming" of such skills. (Note that similar equilibration processes are at work in trying to solve interpersonal relationships, but there it is quite clear that even our best "solutions" are inevitably *fallible-in-principle*, and bio-social reality can be particularly elusive — unlike our present visual-identification example.)

3.3: In other words:

How can one resolve such debatable issues? Piaget speaks of an "equilibration" process among the various pieces of sub-evidence (which he called "schèmes") — and Thagard draws networks of links between pieces of scientific evidence (Thagard 1992; R.Traill 2005). This all amounts to the illusive fallible search for *internal consistency* — and that perhaps indicates the disirability of *a roughly-equal agenda of seeking both* "external coherence" (adjusting to whatever tends to fit in with reality in the world outside)
— *AND* "internal coherence".

In short, we need both *experimental practice* and *theory*[2] — theory which tests itself in some way such as fitting in with a dynamic model which is self-stabilizing or (better still) self-organizing. E.g. that works neatly for Newtonian planetary systems; but it also works well for Piaget's account of how intelligence self-organizes during infancy.

The reason that this internal approach tends to work (in conjunction with *some* contact with the outside), is that ●*minds*, *and* ●*dynamic self-stabilizing scientific theory-models* — must *both* surely share something with ●*any real-world dynamic phenomena* they are trying to depict; — a sharing of some common dependence on stabilizing mechanisms. Without such mechanisms, any one of the three is likely to fall apart spontaneously; and *with* such mechanisms they must automatically have something in common which transcends "observation only". (R.Traill, 2011a).

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[1] This raises questions of **logical induction** — the *semi-legitimate* strategy of arguing (e.g.1) that just because the Sun always has risen in the morning, then it will "necessarily" do so again tomorrow; or (e.g.2) that "as all the swans I have ever seen have been white, then all swans must be white". Popper's famous (1934) book was mainly aimed at attacking that assumption within science — and not directly aimed at favouring experiment at the expense of theory, though that latter is what his disciples have usually concentrated upon!
Anyhow, in the case of the Sun, we learned to bypass that primitive induction by investigating the "substructural mechanisms" involved. Psychology has hitherto lacked any such clearcut knowledge about substructure — though I have made it my business to try to remedy that handicap; (e.g. 2011a, 2012).

[2] "*Those who have handled sciences have been either men of experiment or men of dogmas. The men of experiment are like the ant, they only collect and use; —— the reasoners resemble the spiders, who make cobwebs out of their own substance. But the bee takes a middle course: it gathers its material from the flowers of the garden and of the field, but transforms and digests it by a power of its own. …*" (Bacon, 1620: aphorism 95)

4: So — Which of Our Basic Physical Assumptions Are Wrong?

I will briefly suggest specific mistakes within two actual physics theories, although (as mentioned above) I would expect that most such comments have already been made by someone at some time. More importantly though, I will attempt this whilst still wearing my "social-sciences hat" and meanwhile emphasize the apparent "*procedural inefficiencies*" within physics &/or within science generally.

4.1: But first some measurement-issues raised by Ellis (1966):

He points out that the way we organize our units of measurement is somewhat arbitrary, and could be different. In particular,° the way we space out the steps in our **temperature scales** (°F, °C, or °K) are all *what we think of as linear* — and they all cease to have meaning below "absolute zero" (0°K) as we call it. However there is a plausible case for using a logarithmic equivalent, reflecting the increasing difficulty of approaching 0°K in the lab. Thus if we used *log*(°K) instead: (i) equal intervals would represent equal thermodynamic effort, and (ii) "absolute zero" would now actually be the never-quite-reachable "–∞", which arguably makes good sense in that context.

So how do we choose one such system rather than another? He suggests that it is only a matter of convenience — and that seems reasonable, but it gives us cause to ponder the deeper implications of some of our formulae. Then again we could re-state this in Piagetian terms, and assert that we choose whatever fits best into our conceptual *equilibration* system — whether within our individual minds, or within the collective "mind" of society/science.

A further complication is that it is impossible to use the same "thermometer" for every temperature-range, so in practice we are forced to use a patchwork medly of different devices. It is only when we get deep into the thermodynamic theory, that we achieve a properly coherent concept of the whole scale.

He also offers a rather more bizarre-but-simple case, where we might consider measuring lengths in "dinches" rather than "inches", where "1 dinch" = (from memory!) the length of the diagonal of a 1-inch square. That would obviously generate unnecessary complexity, and that is why it loses out in the equilibration competition. That also has an important bearing on Occam's Razor criteria of ' "simplicity", or "beauty", or "utility" '.

4.2: Errors specific to Special Relativity — (Einstein 1905)

Let us start by accepting the legitimacy of Lorentz contractions of length, and the relativistic slowing of clocks — though one might still dispute details about causality and consequences, etc.

The basic "Relativity" concept

(Page 37): " the phenomena of electro-dynamics as well as of mechanics possess no properties corresponding to the idea of absolute rest."

Fine, as far as it goes, but he effectively defines "ether" as being a universal *rigid* framework — and as that seems incompatible with the above sentence, he thereby rules it out also. Thus:
(Page 38): "The introduction of a 'luminiferous ether' will prove to be superfluous inasmuch as the view here to be developed will not require an 'absolutely stationary space'…"
Meanwhile he says nothing about any "*flexible* ether" which might correspond to (e.g.) Faraday's fields of force — or what modern writers simply refer to enigmatically as "space" — e.g.[3] — as they avoid the taboo word "ether".

[3] "… in fact space itself can expand faster than light. We tell people nothing can travel faster than light but we lie. We actually have to be like a lawyer and parse it more carefully. Nothing can travel through space faster than light, but space can do whatever the heck it wants and it's doing it right now." (Krauss, 2012)

The trouble is, that if we now allow any such extra entity into the picture, especially if it is a widely extending field (like a flexible ether), then the tacit relativity condition of "other things being equal" become much more difficult to imagine — and perhaps vacuous anyhow.

His denial of "simultaneity" based on operationalist criteria

(Page 38-39). From the viewpoint of Piagetian epistemology, this seems to be just plain false! Yes, there are practical difficulties in comparing clocks which are unavoidably influenced by real conditions — but if all observation entails some degree of internal equilibration anyhow, why not formulate an *abstract* construction which transcends this limitation in a meaningful way. (Galileo was faced with a comparable problem in trying to postulate how a rolling ball on a level surface would go on forever *if it were not for the friction*. His answer then was to postulate a frictionaless state — something actually unobservable and unattainable from his viewpoint, but nevertheless a meaningful concept.)

In short, practical impossibilities need not necessarily quash the legitimacy of a theoretical concept, especially if it offers good "internal coherence" with all-or-most of the other relevant concepts.

In fact Einstein himself gives the game away in his footnote on page 39: "We shall not here discuss the inexactitude which lurks in the concept of simultaneity of two events at approximately the same place, which can only be removed *by an abstraction*". [My italics] — Surely there is no *in-principle* difference! In fundamental theory it surely does not matter how small an effect may become: The important point is whether it is theoretically *there or not*. (Of course a practical *engineer* will legitimately ignore "trivial" influences — but that can still lead to problems when the situation is extrapolated, as when Lord Kelvin ventured into under-water cables, still assuming that the effect of electrical inductance "L" was as negligible as it had been in previous applications.)

The Speed of light, "c"

If we accept the above arguments, especially concerning "flexible ether" and the related footnote ([3]), there seems no need fo me to say more at this stage. — (Indeed I, like many others, have written on this topic previously). However the paper (D.Traill, 2012), already submitted to this competition, seems to resolve the enigma of the Doppler effect for light — arguing that, after all, it is essentially the same as any other Doppler effect!

4.3: Errors about indeterminacy

Wave versus Particle?

Note that "particles" can be meaningfully explained in terms of self-interacting waves — both in principle, and in the form of working models, see below. Meanwhile it is difficult-or-impossible to see how waves at the quantum level could be made up of particles. Conclusion: Waves must be more fundamental. Does anyone challenge that?

Bypassing the Copenhagen convention!

This doctrine insists essentially that there is no meaningful substructure within a wave/particle quantum — so the best we can expect is a statistical interpretation of proceedings. This notion is justified by the Operationalist insistence that every scientific "fact" must be observationally supported — and of course the Heisenberg indeterminacy principle tells us that we cannot possibly measure as much as we would need to ever know the relevant situation in sufficient detail.

Once again, the Piagetian epistemological view would simply reject that argument, for the same reason as before. If we can devise a plausible working model which seems to have the right dynamic properties, then surely that has some right to be taken seriously. After all, isn't that what Copernicus did, given the limited observation-possibilities of his day?

Be that as it may, D.Traill (2004, 2005/2008, 2011*a*, 2011*b*) has bypassed the supposed bans of both conventions, and postulated self-interacting waves — culminating in dynamic models for both electrons and positrons. Insofar as these models do faithfully model the particles, then our collective equilibration process should presumably accord them provisional acceptance — despite the two restrictive "bans".

5: Where should we go next?

It is worth noting theat human intelligence (for the individual) seems depend crucially on a specific type of hierarchical organization — not exactly a feudal command-structure, but rather a system which keeps updating the parameters of the level below, according to developments. (R.Traill 1999, 2012,+Thesis:1978) — based on the works of W.R.Ashby (1952), and (quite separately) the prolific works of Piaget.

Bearing in mind that science/society-as-such is a separate epistemological entity we might expect that it could benefit from a similar organization of mentoring-and-support from the top. (In epistemological systems which lack this sort of hierarchy, there is much more dog-eat-dog Darwinian competition among the sub-entities. In fact such hierarchies are evidently the way to transcend wasteful competition, and indeed develop a form of collective intelligence). I suggest that science/society might benefit considerably by improving or developing such organization. At present it seems that individual or localized research is seldom assessed publicly on its merits. Instead any unsolicited innovation is likely to be treated as "just another irritating mutation" and condemned to a Darwinian battle "for the survival of the lucky" unless some critical mass of mutual supposrt can arise. Perhaps then there should be regular "up top" monitoring of the whole process — taking the brain as a provisional model. However I am not confident that there is yet the political will for such measures.

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