



Essay Review

An Alternative Paradigm After All?

By

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A review of *The Origin of Minds: Evolution, Uniqueness and the New Science of the Self* by Peggy La Cerra & Roger Bingham. New York: Harmony Books (2002).

Setting The Stage In Three Steps: 1. Dipping into the History and Sociology of a Very Young Science

David Buss (1995a) claimed that Evolutionary Psychology (EP) was a new and potentially unifying paradigm. Buss' influential paper was published with commentaries by several authors more or less connected to evolutionary approaches to psychology and other social sciences. One of these comments was by Peggy La Cerra and Robert Kurzban – and made this claim: One cannot have a new paradigm where there has not existed an old paradigm. The first author was a graduate student with Cosmides and Tooby – the two most innovative theoretical forces within EP. At the time this seemed fair enough – the force and impact of EP and behavioural genetics was only starting to make a presence, and it was not easy to see how the existing cacophony of psychological theories actually could be considered a paradigm. It has for years been a simpler task noting how they differ, than how they actually

harbour similar beliefs – and actually false beliefs – about human nature and the mind. The initial attempt at lumping these together, Tooby and Cosmides' (1992) concept of the Standard Social Science Model (SSSM), was seen by most as an exaggeration or an aggregate of strawpersons. Buss' (1995b) conclusion was that EP might be the first unifying paradigm – and I agree (Kennair, 2002b).

Then, according to the presentations in *The Origin of Minds: Evolution, Uniqueness and the New Science of the Self* one may read that the two authors, Peggy La Cerra and Roger Bingham, having first been convinced of EP, are now opposed to that theory. This is not that surprising. In 1998 they had a paper published in the *Proceedings of the National Academy of Science of the U.S.A.* Their sponsor was none other than George Williams, one of the greatest current evolutionary theorists. The gist of that paper was clear – EP has got it wrong: The mind is not a set of inherited modules!

Why would Williams be interested in

supporting this view of the mind? I do not know. One possibility is this: Apart from the unease many feel about applying evolutionary theory to the mind, there is extra unease about specified modular mental adaptations. Daniel Dennett (also mentioned by the authors in their thanks) and another supporter of the book, V. S. Ramachandran, are opposed to specific adaptations versus general adaptation or feel like limiting the number or specificity of specific adaptations – often based on what they find silly or at odds with their own understanding of how evolution works (see Ramachandran & Blakeslee, 1998).

Kennair (2002b, p. 22) provides the following attempt at showing how the general may be quite specific:

“There is no such thing as a ‘general problem solver’ because there is no such thing as a general problem” claims Symons (1992, p. 142). Dennett (1995, p. 491) begs to differ; writing this off as a “luscious slogan”. He supports his opinion by referring to Williams’ (1966) classic work on adaptation, which is the major source of EP on the nature of adaptations, and this is not an unsubstantial challenge. Williams (1966, p. 86) writes “A precise adaptation might require more genetic information than one that would give a blanket coverage to a broad category of ecological demand.” It is thus more expensive, in evolutionary coinage, and therefore less likely to arise. Both Dennett and Williams use the example of wounds coming in all shapes and sizes, but the adaptation, whose function is wound healing, heals them all. Dennett (1995, p. 491) concludes that: “How general any cognitive mechanism is, or can be made to be through cultural enhancement, is always an open empirical question”, and of course this is true. One might draw attention to the fact that Symons is contrasting general problem

solvers to more specific problem solvers. In that respect Dennett’s criticism seems too stringent, as the healing mechanism he describes does not do anything but heal what is recognised as wounds and is therefore quite specific.

Whether the opposition to the genetically prescribed, specific model of the human mind leaves too little hope for a libertarian free will for some researchers, I do not know... But certain academic points obviously cause interesting constellations of allies and enemies.

2. A Song of Unsung Heroes

In a recent presentation of human evolution and evolutionary approaches to psychology the zoologist Iver Mysterud (2002) includes a chapter of voices critical to mainstream EP. Of course, every good book on the subject ought to include a presentation of detractors, too – in order to complete the picture. This is not the typical cast of anti-evolutionary writers, to be found in works such as *Alas Poor Darwin* (Rose & Rose, 2001), *Not in the Genes* (Rose, Lewontin & Kamin, 1984) and *Human Nature and the Limits of Science* (Dupré, 2001). It is neither the religious critics of all creatures great and small *not* being created by a good lord, like Behe (1996) or Dembski (1998).

Mysterud presents the following critics:

David S. Wilson (1994) criticises EP for not focusing on possible adaptive individual differences, which I believe is covered by the writings of Tooby and Cosmides (especially their 1990a paper) and Buss’ work on personality (see Buss & Greiling, 1999, for a recent paper). Further, Wilson’s (1999) critique of Buss’ (1999) introduction to EP is highlighted for not including all of psychology or all evolutionary approaches. In this case my stance is that all evolutionary approaches are not relevant, as EP is a specific and even rigorous theoretical approach. Like it or not, it is the most popular and sophisticated game within evolutionary approaches to behaviour or psychology, but it is not merely a collection of all evolution-inspired

research projects.

Irons (1998) concept of *Adaptively Relevant Environments* (ARE) is also presented. To me this specific formulation seems to offer little that a thorough reading of the strategic modelling of the EEA should not provide (Tooby & DeVore, 1987; Tooby and Cosmides, 1990b).

Panksepp and Panksepp (2000) describe what they find to be the seven sins of EP – which I find Ian Pitchford (2001), nicely, put to rest. Finally, La Cerra & Bingham's PNAS paper sets a discussion of modularity.

Personally I am not certain that I would have chosen to spend a chapter on this motley crew – their voices have in some cases (especially La Cerra & Bingham) been sounded loudly, but there has not been much in form of resulting research. They have not made it to the front of the attention within the field with their criticisms. That ruthless selection process is often a reasonable indicator of the fruitfulness of an idea.

3. Context

I read the current book after a year of reading different heretic and critical works, and unlike Eco's (1992) Benedictine monks of *The Name of the Rose*, I survived this inquisition with both my EP soul and life intact – I think. Dembski (1998) wanted to convince the world that his religion was true science – but it is not, and as philosophy... well, expletives are best avoided (But see Fitelson, Stephens & Sober, 1999; Kennair, submitted). Dupré (2001; see Kennair, 2002a), displayed a more heathen approach, suggesting with certainty that EP is not science, but not documenting any reasonable amount of reading or understanding of the most central theses. Miller (2000, see Kennair 2002c), more the heretic than the heathen, suggested that there are bio-aesthetic gods, not only functional deities. If he is a true prophet he will have to provide the empirical work next. Cartwright (2001, see also Kennair, 2002d) misconstrued most of the saints of EP in an otherwise fine, although a strange and some-

what false gospel. Thus, ending the year with the catechism and liturgy of Steven Pinker (2002) suited me well (Kennair, 2002e). It made me safe in my faith, gave me solace, and soothed my otherwise troubled doubts and cleansed me of all heresy. I am therefore strong enough to start the New Year by reviewing this new criticism of EP by La Cerra and Bingham... I hope.

An Alternative Paradigm After All?

Origins of Minds reads pleasantly, like a summer afternoon in a lush, green, warm garden – complete with honeybees, brightly coloured flowers and fluttering butterflies. This is the imagery the book applies, with an almost motherly touch that guides us through the new model, using the familiar to explain the novel. The odd surprise is that the model is actually not that new. Let me return to this after explaining what the supposedly new model claims.

A new, better and truer model of the mind?

The claim is this: The mind is not a set of inherited modules. The software of the mind is not heritable. Rather there are adaptive representational networks that are created and modified online, as the plastic neocortex processes information, sprouts new dendrites and changes levels of serotonin, dopamine, and noradrenaline. This happens through a process many behaviourally trained psychologists will recognise as the good old expanded stimulus-response model: The SORK-C model of learning psychology. Adaptive representational networks are formed associatively (through neurophysiological processes) coupling information about different environmental stimuli, internal state changes, emotional responses, behaviours and their outcomes.

The major driving force behind this model is the laws of thermodynamics – and minds or intelligence systems, a general description from the sensory systems of *E. Coli* to the human mind, are described as cost/benefit energy economising systems combating the threat of entropy. Of course, all evolutionists are aware of the need to consider energy – but

whether this needs to be reduced to physics is less obvious. I do not believe it explains anything new or more profound.

The model is claimed to be more in line with modern neuroscience – which EP is claimed to be both ignorant of and at odds with (although this does not seem to be a fair claim, see Duchaine, Cosmides & Tooby, 2001; Gazzaniga 1995, 2000). The book describes a rather modular view of the brain, and accepts explicitly that this is current orthodoxy – it is the mind and the neo-cortex that is more plastic, non-modular and consists functionally of adaptive representational networks. It is never quite clear just how this model, apart from describing basic neurology, is more compatible with modern neuroscience. Neither is the book explicit on where EP is in conflict with neuroscience. Rather there are statements and implicit suggestions that this is so. At the same time, it is fair enough to claim that there are neuroscientists who oppose EP, as there are social psychologists, cognitive psychologists, clinical psychologists, psychiatrists and general practitioners who oppose EP – but then again there are influential academics within all these areas who also support EP. A more explicit and substantial exposition is necessary to take these claims seriously. But, to be sure, any theory needs to be able to combine both neuroscience and functional adaptationist findings and hypotheses. And empirical certainties would of course have the upper hand. Thus mere advocacy is of little help.

Another part of the theory involves life history theory. This is a theory that claims that every age from foetus to old age has specific adaptive problems to solve and a specific evolutionary biology. This means that the mind, the brain, the hormones and all other features change through life in a co-ordinated and adaptive manner. There is a time for play, a time for courting, a time for parenting and so on – the minds of teenagers being different from those of their parents and young children. This is evolutionary, adaptationist developmental psychol-

ogy. In La Cerra and Bingham's presentation this is one of the limitations of plasticity and the development of certain adaptive representational networks.

The fact that this system is one of experience means that it, rather than what EP attempts, is one that describes why each individual is unique. An alternative approach, merging behavioural genetics and EP also attempts this, of course. Further, the focus on the more global, open problem solvers of the neural network minds not being inherited means that each individual has to manage to sort things out through cause and effect, learn rapidly, and that the more experience the more adapted one is. Thus development becomes standard psychology: a result of experience (empiricism), where early experience is most influential, and a neurologically based life history system (a set of modules?).

Thus taste preference is considered to be a result of the following process: Humans have general preferences for fat, sugar and salt. But these are not genetically transmitted information processing circuits, or modules or adaptations, typical of all members of all cultures. (At this point the culture that eats grass, or has a diet low on fat or sugar at religious festivals or other big occasions, would have come in handy.) Rather we have an intelligence system that consists of several neurological structures that assess the need of nutrients dependent on physiological state and life history status. How this differs from mental, sensory and neuroendocrinological modules or adaptations is left unmentioned. In the end we assemble adaptive representational networks of all tastes based on contingent conditioning as described above, creating unique experience-based taxonomies of taste. Thus we probably neither have sexual preferences, it just feels good to rub against certain people, and those society rewards us for having sex with and those we experience satisfying friction with, become our objects of lust. Call the neurological and sensory mechanisms adaptations – they are hardly anything but. Ac-

cept that behaviour that is associated with pleasurable states increases – nobody disagrees with the effects of classical conditioning or species restricted operant conditioning. What is left of a new model that differs greatly from EP? Not much – actually it is a step back from the development within modern learning theory. And it makes humans rather strange animals indeed. Other animals need innate taste preferences or they would starve to death or end up victims of severe malnutrition. Parts of this description are obviously true and would not be challenged by mainstream EP – the insistence that there should not be any specific genetic adaptations involved is inconsistent and quite perplexing.

The typical frame problem for the development of taste preference – in which something has to taste better than other things, differing due to the animal's genetically specified gastrointestinal system, is not addressed. This model claims on the other hand to have solved the frame problem. Not addressing something is one way of making it go away...

Also, the evolutionary theory, as far as it is explicitly stated in the book, smacks more of adaptiveness than the study of adaptation. That is probably due to the difficult manoeuvre of delineating the adaptive networks from adaptations or mental modules. The adaptive networks being online calibrated and created “adaptations” – thus they are highly “plastic” and “adaptive”. Therefore one actually does not need evolutionary theory. It cannot predict how the human nature will react in any given situation – because every human mind (and maybe brain) is as personal, unique as a fingerprint (the cover illustration). Every mind is contingent on the individual history of the specific individual. The only way to predict what a person will do is what they have experienced and what contingency there has been between internal states, external stimulus, behaviour and the outcomes of such. Thus this is a return to behaviourism – yet again it is a typical example of the SORK-C model of the psychology of learn-

ing. *Nota bene:* This *will* prove to be one of the reasons people find the model attractive – for years now all studies that can predict human nature from evolutionary theory have been seen as a threat to human dignity, libertarian free will and humane policies. This is false, of course, but still a potent ideological force within academia.

But – due to the above – the book's thesis is no longer a new, true or better model of the mind. It is old news, no longer tenable tenets, and a rather speculative stance altogether. Now, it may be that it fits the neuroscientific data better than the EP model – but it clashes with much that seems to be true within other areas of empirical psychological research. Research that has been tilting at the windmills of established academe (like Garcia, 1981, used to do when behaviourism was a practice of species naïve equipotentiality), but has eventually started toppling them.

The traditional paradigm psychology versus the new genetic and functional neuroscience

The biggest surprise of this book, therefore, is that it actually goes a far way in the direction of affirming a conventional paradigm of psychology. This might therefore actually exist, despite La Cerra's earlier statement to the contrary when she used to be an evolutionary psychologist. The established paradigm of psychology exists in the traditional myths of psychology. Such myths include the dualism, romanticism and empiricism of mainstream social science, including psychology, psychiatry, neurology, animal behaviour studies, etc., etc.

There are so many ideas within psychology that have no foundation in research, but have become a part of modern popular culture, and thus a part of the way even many psychologists think their science ought to be. These ideas pop up all over the place – from research that merely considers environmental factors when considering similarities between children and parents, to policy debates on whether one may prove the effect of something as ephemeral as psychotherapy, to Hollywood movies that

explain why someone acts the way they do due to their childhood experiences, and literary critics who believe Freud actually described human nature almost perfectly (which might be fair enough, because of the lack of imagination of modern authors writing copies of Freud who merely wrote a copy of Shakespeare...).

Today the model that Pinker presents in *The Blank Slate*, and which is to be found in the work of David Buss and Robert Plomin, an integration of behavioural genetics and adaptationist or functional cognitive neuroscience (which is what one might call EP), is a completely different approach to the psychology of yesteryear. To expect that all neuroscientists should be updated on behavioural genetics or that evolutionary theorists should be updated on functional cognitive neuroscience is therefore still a tall order – thus it does not come as a surprise to me that people outside psychology proper still believe in Freud and Skinner and Maslow – people within psychology proper still do!

Different Glimpses of the System

La Cerra and Bingham call one of their chapters “Glimpses of the system”. Following the suggested new paradigm above I hope one will see that the paradigmatic tenets of the old psychology are taken too much for granted. And that the whole foundation of old paradigm psychology is going to prove shaky and not the stuff of a new science. A neat fit with old – and false – ideas is nothing but a sign that something might be amiss with the thesis.

Instead of a return to Psychology 101 I believe one ought to consider more empirically based glimpses of the system. La Cerra and Bingham show us Freud (analysing Darwin, nonetheless), Skinner and Maslow. They point out the primacy of early experience, the effect of psychoanalysis, and several other paradigmatic examples of the mythological psychology that was. If Newton had stood on the shoulders of ye giants of psychology of yesteryear he might not have seen that far at all...

Here are a few challenges to the old

wisdom:

- The ideas of dualism (which La Cerra and Bingham avoid some of the time), romanticism (which perhaps is not obvious, but a focus on adaptiveness and uniqueness rings bells) and empiricism (which is the basis of the main thesis of La Cerra and Bingham) ought to be winnowed out of scientific psychology (Kennair, 2002e; Kennair et al., 2002; Pinker, 2002).
- The composite myth, a result of those above, of the primacy of early experience needs to be put down too (La Cerra & Bingham are explicit believers and proponents of this myth) (Paris, 2000; Plomin et al., 2000; Scarr, 1992; 1993).
- All psychological traits have a genetic basis (Kandel, 1998; Pinker, 2002; Plomin et al., 2000).

These glimpses are not reflected in La Cerra and Bingham’s thesis. Before they are, the thesis is probably not true.

The book’s treatment of psychopathology is also superficial, to the degree of being speculative or uninformed of developments within an evidence-based clinical psychology and psychiatry:

- One may make the following surprising announcement: One does not “learn” anxiety disorders. Even if the paradigm of psychology claims that anxieties are learnt via Mowrer’s Two-factor theory, this does not seem true (Collier, 2002; Kendler, Myers & Prescott, 2002). Interestingly, these anxieties may be “extinguished” in a behavioural or “reattributed” in a cognitive behavioural therapy – but the mechanisms are no longer as simple or obvious as when the simple *shibboleth* of “learning” was simply invoked.
- Psycho-analysis is not a scientifically proven theory – it has neither been documented efficient (in some cases it is docu-

mented less efficient than waiting lists,

Fisher and Durham, 1999), and is not a recommended treatment for any diagnostically defined mental disorder (see Nathan & Gorman, 1998; The Division 12 Task Force, 1995). Thus basing their model's likely efficiency on the similarities with and supposed efficiency of psycho-analysis is uninformed and probably more likely pure speculation rather than indicative of whether their model is correct or not.

- There is a genetic component in all personality disorders. The typical effect of genetic influences on psychological phenomena is not that more experience or more specifically longer time living one's life makes for more influence on traits by the environment. Rather the opposite – as one grows older one resembles one's parents more.
- Mental disorders do not arise due to the process of learning that La Cerra and Bingham describe or trauma or early childhood experiences (Paris, 2000) – this holds for all the many disorders speculated on in the book.
- Beyond the evolutionary psychopathology research and comparative research on depression (see Sloman & Gilbert, 2000, for a broad exposition of current approaches; see Kennair, 2001, for a review of the book) – there is a limit to how many types of depression this model probably describes. Also, in my humble opinion, it fits more neatly with specific cues being perceived; specific state governed behaviour being displayed and a modular, adaptation rather than adaptiveness, type process. Some people do not get depressed. Those depressed a rather syndromatic – that is they show the same symptoms when depressed, and do not show these symptoms when not depressed.
- Manic episodes usually demand the genes necessary for such a disorder. It is not something any individual can experience, and it is not learned contingently. The

mono-zygotic twin of a patient with bipolar disorder has circa 50 percent chance of having the disorder, too. We do not know whether the other 50 percent are due to environmental factors that prevent bipolar disorder or environmental factors that elicit the disorder – or, as Steven Pinker (2002) points out in his discussion of hairs of genetically similar flies, it could be chance.

- There is disorder specific information processing and attention processes involved in the different anxiety disorders (Wells, 1997).

Further, there are a few inconsistencies in the thesis being presented, and these most often arise in when the authors are attempting to refute EP. These include the bypass of how one behaviour is considered positive and another negative, how specific cues in the environment regulate the organism and mind through predictable specific and structured processes, and how and when specific heritable modules fit into their system, if at all. And if they do not exist – how does one solve all the problems that usually arise – such as the extreme plasticity perspective, frame problems and the non-heritability of known heritable features. Also, the research that is criticised needs to be foundational when launching a critique of a complete research program.

- The three most important empirical findings within Cosmides' (1989) cheater detection research, Buss et al.'s (1990) study of mate choice in 37 cultures and Daly and Wilsons (1988) research on homicide. I claim that these are what need to be addressed critically in any attempt at disqualifying EP (Kennair, submitted).
- The demise of the idea of equipotentiality of learning has changed the field of learning. Innate predispositions govern – these solve many of the frame problems suggested solved by network learning in the book. Sugar is important for fruitedeaters, fat

for meateaters – omnivores like humans will respond positively to both. You cannot start the SORK-C learning process without the predisposed reward circuits in place.

- Neuropsychopathological states such as hemispheric neglect, actual colour blindness, “face blindness” etc. all point toward a modular mind. So do other states such as empathy and emotions, also executive functions, and not least normal personality traits. In all of these cases a single ability or faculty or mental mechanism may be missing, malfunctioning or be over sensitive, leaving most of the rest of the system functioning quite normally.
- The most obvious of the cases above would be Simon Baron-Cohen’s (1995) research on autism. It is a major problem for the authors theses that they do not consider the case of ToM (or ToOM) and autism. The fact that ToM and thereby autism seems to be dependent on a module, plus the heritability of autism, seems to be a serious problem for the current model.

It is my firm belief that any New Science of the evolution of the mind, self or psychopathology would need to incorporate or at least discuss the points above.

A Few Important Positive Points

Despite the many problems I have with this book, there are a few important positive points that are worth mentioning. First, this book is based on an evolutionary approach, albeit not in a manner that will make it fruitful (as one cannot make evolutionary predictions of the contents of the human mind), but still this is a foundation for any good study of human nature – one cannot study human nature in an evolutionary void. Second, it is written within a biopsychosocial frame – even if the neo-cortex becomes “wonder tissue” (p. 177) and the authors seem to be a little uncomfortable handling the mind as they do the brain. The attempts at reducing dualism need to be extended to the part of the mind that is a product of the neo-

cortex, of course, but as long as they are considering the brain they manage to present an important integrated picture of body, social context and psychological processes.

Last, their focus on life history theory does them credit. It is not that EP is not aware and acknowledges life history theory as valid; the issue is that the nature of the research program is focused on universal human nature. The problem arises due to all psychological adaptations needing to be addressed in a matrix that addresses age, gender, status and probably a few other contextual cues. La Cerra and Bingham lose the ability to make predictions if they keep their “wonder tissue” model. EP needs to be specific about what mental mechanisms are likely to be found to be developing and functioning at what age, for what gender, in which specific contexts etc. I agree that more focus needs to be placed on this. A cheater detection mechanism is probably necessary in family life from one is able and in need to fight for one’s fair share of food – and probably typical for both genders and in all other social contexts. Not all mechanisms are as universal through age, gender and other contexts. Further, this approach will also need to be adopted by behavioural genetics – traits need to be viewed at same age and similar life history context according to the matrix described above. Life history theory is the best explanation of developmental discontinuity – more so than learning theory. This of course is one of the limits of plasticity of La Cerra and Bingham’s model.

Conclusions

The Origins of Minds is a book worth reading. It is a window into the anti-EP camp within neuroscience and evolutionary theory, and it is important to know what those who have a non-adaptation approach to mind consider relevant. It presents a thesis that has been supported by influential names such as the evolutionist George Williams and the neuroscientist V. S. Ramachandran. This alone suggests that one ought to be familiar with the arguments.

But it is not necessarily convincing. The outdated myths of the old paradigm and internal inconsistencies are too noticeable from an informed EP, clinical psychology and behavioural genetics point of view. In a world where behavioural genetics is providing reasons to believe that there are heritable (i.e., specific genetic) causes for the behaviours and disorders that are described, omitting a discussion of this evidence makes for a weaker case than necessary. Further, modularity of mind is probably still the major research model – if nothing else because legal reductionism and some sort of atomism is the only way to research anything properly. It also does seem to be true of the mind, whether it is genetically prescribed or emergent, through some mystical process called development. Finally, the idea that these modules are possibly predicted from evolutionary theory and that these modules exist due to genes “for” these modules has so far inspired fruitful and creative empirical life science.

Still, I am going to recommend that one follow the development of La Cerra and Bingham’s work closely. There are many scientists who are backing this approach – either in part or hook, line and sinker. The ideas of Neural Plasticity, Learning Networks, Behaviourism, the Non-Modularity of Mind, the Non-Innate Nature of Modules or Developmental Emergent Modules, Adaptiveness and General-Purpose Neo-Cortex, to name a few, are still influential within the behavioural or psychological research communities. Surely the only measure of whether one approach is better than another is the ability to formulate research hypothesis, generate research and make empirical discoveries that expands and improves our understanding of Human Nature. So far I believe this has been EP’s strongest influence. When the other approaches become as fruitful they will deserve more attention. Until then all they have is advocacy. I have to disagree with Dawkins (in the blurb of Miller, 2000 – and quite out of character with most Dawkins has said on science and faith) – advocacy is not fair enough. La Cerra

and Bingham may be able to communicate a sound biopsychosocial approach to the general-purpose community. Further, their skill at communication may be able to influence the founding of a somewhat more coherent research programme – and even an evolutionary approach to the study of the mind. I will be waiting to see the empirical results.

Postscript

It did not slip my attention that my own list of references below has grown ridiculously expansive. At the same time it reflects a last point of criticism. I am familiar with parts of the literature that La Cerra and Bingham peruse in their book, but not all of it, of course. As they claim this is a science book (is it?), one would expect them to have a list of references, not merely a handful of books suggested for further reading. It would make checking up on their claims and interpretations not only easier, but indeed possible.

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