My Experience in Science

Steven Lehar

How I Got Into Biological Vision

My interest in biological vision began with my work in computer image processing and automatic target recognition, when I was employed with a defense contractor Avco/Textron in the 1980's. There is no better introduction to the problems of natural vision than attempting to solve the problem with computers. For the computer has, in the digital image, all of the information in that image in the form of explicit numerical data. And yet the problem of extracting useful information from that data turns out to be extraordinarily difficult. For although the computer can detect simple features easily enough, such as image edges, an edge detection algorithm tends to find thousands of edges in a typical natural scene, most of which are spurious, either texture lines, or shadows, or irregular fragmented surfaces which are hopelessly confused. Furthermore, many of the most significant edges are often missing, being occluded by foreground objects, or having insufficient contrast with the background, and many significant edges contain gaps, kinks, multiple contours, contrast reversals, etc. The next step of making sense of configurations of edges remains largely an unsolved problem except in the most controlled visual environments.

The Difficulties of Computer Pattern Recognition

In my experience with image processing I began to get the impression that the farther we progress with complex algorithms designed to analyze the image data with ever more sophisticated strategies, the more brittle and rigid and cantankerous our algorithms seem to become. I began to see that there is a fundamental difference between the properties of natural vision, as exhibited even by the lowly house fly, and the rigid deterministic approach to vision represented by the digital computer. The little fly, with its tiny pinpoint of a brain, dodging effortlessly between the tangled branches of a shrub in dappled sunlight and in gusty cross-winds, seems to thumb its nose at our lofty algorithms and expensive hardware that can, at best, guide a van loaded with the latest in computer equipment at a snail's pace down a clearly demarcated road, even then occasionally running astray. It became clear to me that nature was hiding some very simple elegant secret in biological vision, whose operational principles are entirely different from digital computation.

Stephen Grossberg's Approach

While I was employed doing image processing and artificial intelligence, I happened into a talk by Stephen Grossberg in which he presented an interesting approach to investigating biological vision. Grossberg's approach was to study the properties of visual illusions, and attempt to replicate those illusions with neural network models in computer simulations. For if we can replicate the properties of the visual illusions, this surely will offer insights into the nature of early visual processing in biological vision. This approach is particularly appealing because some illusions seem simple in principle, and therefore offer a good starting point for modeling visual perception, and yet other illusions exhibit an exquisite subtlety and complexity, and therefore those illusions promise a glimpse into those most mysterious and enigmatic aspects of visual processing whose operational principles remain to be discovered. I was so enamored of this approach that I quit my job in image processing and joined the Ph.D. program at Boston University in Grossberg's department.

Gestalt Theory

The study of visual processing by way of visual illusions was an approach championed by the Gestalt movement. Gestalt theory, I discovered, seemed to capture the essence of that elusive principle of computation that is so difficult to express in computational terms. In fact the early Gestaltists had made a concerted effort to characterize exactly those kinds of phenomena that are impossible to express in terms of local or atomistic computational strategies as in the digital computer. There has been considerable cross-fertilization of ideas in recent decades between theories of artificial and biological computation, and many of the concepts in computer image processing, such as spatial convolutions with spatial kernels, have found parallels in neural network theory, in the form of patterned receptive fields. The limitations of computer image processing algorithms therefore reflect corresponding problems in neural network theory. For the spatial receptive field is no different in principle from a template matching scheme, a concept whose limitations are well known. It seemed to me therefore that many of the limitations of artificial vision system were also problems for neural network theory.

The principal difficulty involves the most central element in neural network theory, i.e. the concept that neurons behave as quasi-independent processors with strictly segregated input and output channels. This atomistic concept of local processors, sometimes known as the Neuron Doctrine, is the antithesis of the holistic global computational paradigm suggested by Gestalt theory. I discovered however that my concerns with the neuron doctrine were not generally shared by others in the field, most researchers seeming to believe that the properties of the neuron are so well established experimentally that all that remains to be discovered is the proper arrangement of these elemental processors to account for the observed properties of perception. I felt that I was virtually alone in my conviction that the fundamental principles of neural function remain to be discovered.

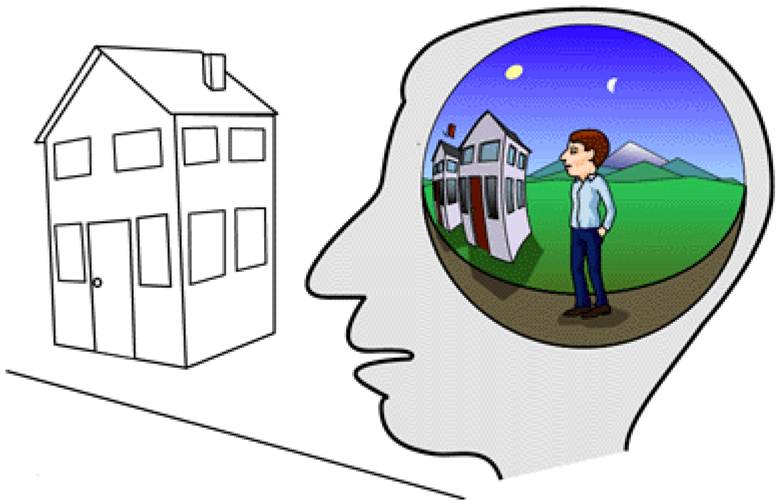
Harmonic Resonance Theory

In my own Ph.D. thesis work I proposed a Harmonic Resonance theory to explain a number of visual illusions which were difficult to account for in conventional neural terms. I thought I had made a very significant discovery of a completely new principle of neurocomputation that promised an answer to those troublesome Gestalt aspects of perception. While I was permitted to graduate from Boston University with my Harmonic Resonance thesis, Grossberg and others remained unconvinced, seeing no need to abandon the well established concepts of neuroscience. Furthermore, to my surprise, I found that all my attempts to get my thesis work published were rebuffed. It seems that the conventional notion of neurocomputation by way of spatial receptive fields has been accepted for so long that it would take extraordinary evidence to convince neuroscientists of the extraordinary hypothesis that the neuron doctrine is not sufficient to account for the phenomena of visual experience. That extraordinary evidence was available however, and was plain for all to see. The problem was that people, including myself at the time, were looking right at it without ever seeing it. It was a classic case of not seeing the forest for the trees, for the extraordinary evidence of visual processing is plainly evident in the world of conscious experience.

The World-In-Your-Head, A Flash of Insight!

I remember very vividly the first time I came to realize the truth of indirect realism. I was sitting in my armchair at home, practicing the exercise I now call introspective retrogression, trying to see where in the world that I see around me could I find evidence of the properties of my visual cortex. I knew that without my cortex I could see nothing at all, and that therefore in some sense this image of the world around me was itself somehow produced by my cortex, but while in my cortex, it was also at the same time out in the world around me. It seemed that the world around me had a dual character, it was both the real world, and a perceptual world, and that the two appeared to be somehow superimposed. There was a curious paradox wrapped up in this idea of perception that I just could not seem to get straight, for how could the world of perception escape the confines of my head to appear in the world around me?

Then one day it hit me all of a sudden like a lightning bolt, in the form of a vivid mental image. Suddenly I could see in my mind's eye that the world I saw around me, including the picture of myself sitting in my chair, was merely an image generated inside my head, and therefore it could not be out in the world. In other words, out beyond the walls and floor and ceiling of the room I saw around me, was the inner surface of my true physical skull, and beyond that skull was an inconceivably immense remote external world, of which this world that was in my experience was merely a miniature virtual-reality replica.

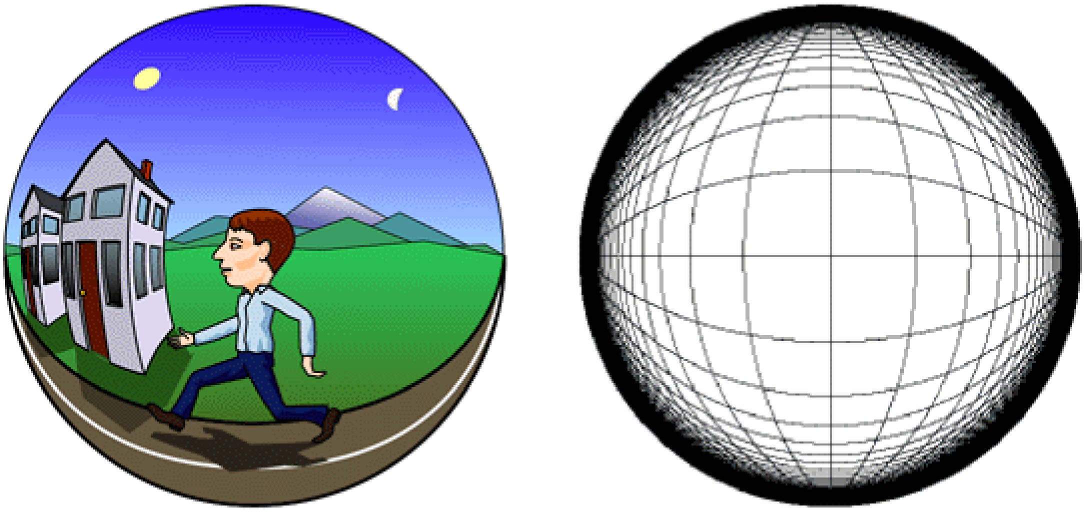


It was no new fact that I had suddenly learned, for my answers to most questions about perception would have been about the same as before that insight. And yet there was a fundamental shift in my perspective that has colored all of my subsequent thinking on perception. For what I could now see was that the brain is capable of generating vivid three-dimensional images of a fully spatial world, like the one I see around me right now. There is no way that a hierarchy of independent neural processors, however they might be arranged, could possibly account for this world of internal reality as I experience it.

The World is Unimpressed

I came running into school the day after my great introspective discovery, only to find that nobody knew what the hell I was talking about. The idea of an enormous world out there above the dome of the sky, they said, was just plain absurd. I had endless debates with colleagues on this issue, to the point where I was forbidden to bring up the topic any more in social settings, because I was getting to be a bore. I found it incredible that I should be the only person to have seen into the illusion of conscious experience, and incredible that others could not see it as clearly as I did, now that I was there to point it out to them. For as incredible as my hypothesis might seem, the alternative was even more incredible, for it suggests that we can somehow be aware of the world directly, as if bypassing the representational machinery in the brain, in violation of everything we know about the laws of physics.

Phenomenal Perspective



One of the most interesting and significant observations I have made is on the nature of phenomenal perspective. I spent many hours pondering whether the walls, floor, and ceiling, of a long straight corridor appears to converge with perspective, or whether they appear straight and parallel and equidistant, as we "know" them to be. It turns out that ***both*** are true! Things in the distance appear both smaller, and at the same time they also appear undiminished in size. The sides of the corridor appear to converge, and at the same time they appear to be straight and parallel. This in turn is direct evidence that the scale of our perceptual representation diminishes with distance from our egocentric point, such that objects in the distance appear smaller, but we measure them against a shrinking reference grid whose scale also shrinks with distance, thus providing an "objective" measure of the actual size of distant objects, which are at the same time represented at reduced scale. See the Hallway Experiment, and the Cartoon Epistemology. It will take decades for the full implications of this observation to be realized by the community at large.

Post Graduate Work

In the year after my Ph.D. I went back to the library and began to read the original Gestalt texts in the words of the Gestalt masters themselves. I discovered to my surprise and relief that I was not alone, but that the Gestaltists themselves had made that same discovery decades ago, and had embodied it in the Gestalt principle of isomorphism. But in the intervening decades, this great secret of vision had somehow been forgotten! How could something of such significance have vanished from contemporary psychology and neuroscience almost without a trace? For despite the strong emphasis on Gestalt theory in Grossberg's department at Boston University, I had never once heard mention of the principle of isomorphism, or that the world of experience is all contained inside your head. These aspects of Gestalt theory have been largely forgotten even by those who consider themselves proponents of Gestalt theory. The issue of indirect realism is not only rejected by contemporary neuroscience, it is no longer even considered a valid topic of discussion. In the rare texts where the issue is mentioned at all, it is usually passed off as a pseudoproblem that had been resolved long ago.

The Origins of Representationalism

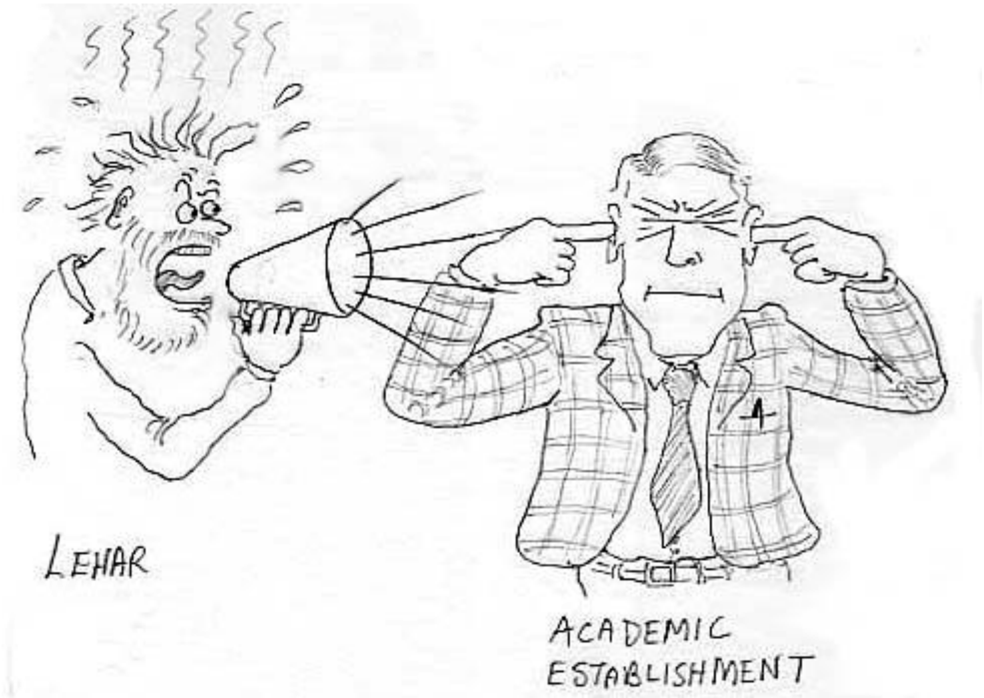
Further searching through the library turned up some writings by Bertrand Russell, who made exactly the same argument for isomorphism, but without any mention of Gestalt theory, since Russell argued the point from first principles. Finally I discovered the writings of Immanuel Kant, and found that it was he who had first articulated this idea over two centuries ago. So it was an old idea that I was dealing with after all, although curiously it seems to be an idea that has had to be rediscovered again and again by different generations of thinkers, because the idea has never taken hold to become a part of the established body of scientific knowledge. It was a great relief for me to see that this idea had such a noble and ancient heritage, for I was so convinced of its irrefutable truth, that should I be mistaken, I would also necessarily be completely mad, as some of my colleagues were beginning to believe. It is an idea which is both very obvious at some level, and yet at the same time almost impossible to conceive, and both Wolfgang Köhler Bertrand Russell had expressed exasperation in their attempts to convince others of its irrefutable truth. And yet, there are other ideas that are equally difficult to conceive which have made their way into accepted science. For example the idea that the world is round, and that people on the underside do not fall off it, or that solid matter is composed mostly of empty space, and the dimensions of our universe, both at the micro and the macro level are truly beyond the ability of anyone to fully comprehend. It is difficult for us to realize retrospectively how absurd these ideas must have seemed to others when they were first proposed. Nevertheless these ideas have entered into the mainstream of science, and are even taught to high school students as properties of the physical world. I believe therefore that the idea that there are two worlds of reality is an idea that will one day be taught to children in school, as one of the essential facts that make sense of our experience of this world. My objective is to make that day come sooner rather than later. For as long as we maintain the direct realist view that the world we see around us is the world itself, we can never make any significant progress in understanding the mechanism of conscious experience.

At the same time I was also working on refinements and extensions to my harmonic resonance theory. I discovered that nature already uses spatial standing waves in embryological morphogenesis as a spatial template for the patterns of plant and animal bodies, and the Chladni figures demonstrate how those spatial patterns can be encoded in an abstract symbolic code that can serve both for recognition, and for reification, regenerating explicit spatial patterns from the symbolic code. And this theory also explained the human aesthetic preference for symmetrical and periodic patterns in ornamental art, and in music, rhythm, and dance, which are all manifestations of resonance in the nervous system. I was beginning to realize that I had stumbled on something of considerable interest and significance. But none of my former "advisors" expressed any interest in my work, and most of my colleagues kept their distance, thinking I was some kind of a kook.

But that was nothing compared to the disappointments I ran into trying to get my ideas published in peer reviewed journals. Little did I know how much the system is stacked against ousiders with a truly novel approach. In my naivite I submitted my papers on my own, instead of co-authoring with a respectable authority in the field, the usual route to successful publication. And I did not list any academic affiliation, but gave my name and home address as an independent researcher. Little did I know that these are like red flags to reviewers, who are more impressed by a person's acknowledged academic reputation and affiliation with a prestegious institution, than by any actual ideas expressed in the paper. It is like a "good old boys" network in which only members are usually published, no matter what absurd nonsense they might write, while outsiders are routinely rejected without reasonable explanation just because they are unknown. This I discovered through the school of hard knocks, as my papers got rejected again and again.

Eventually I developed a persecution complex, that I was fighting a monster bureaucracy of entrenched interests and willful ignorance, and I was fighting it all by myself without any help from anyone. It was a daunting undertaking. After enough rejections which were completely unreasonable and unjustified, I developed a pessimistic attitude about ever getting anything published, and I began instead to cherish my journal rejections, and to post them on my web site for all the world to see the absurdity of the whole peer review process! Instead of bowing and scraping humbly before my anonymous reviewers, I began to fight back and argue with them, and challenge them to justify their rejections with reasoned arguments. And when they failed to provide any reasonable justification for their rejections, I posted the whole ugly business on my web site. It was a suicidal move in terms of prospects for any kind of academic career, but I figured that door was closed to me anyway, so it would not do more harm by throwing rocks at that door and causing a fuss. I may not ever get my academic career, but I will have my fun slinging stones at the ediface of academic orthodoxy.

I used to be young and fresh, curious and enthusiastic about the great enterprise of science. Now I am old and bitter and cynical, and I see the academic establishment as an obstacle to real discussion of interesting ideas. The anonymous reviewers of the peer review journals play the role of the gatekeepers of science, filtering out not only ideas which are unsound, but also rejecting any challenge to the paradigmatic assumptions of academic orthodoxy. Their status of anonymity protects them against any challenge to their judgment. In science we are not afforded the right to face our accusers, but must stand before a kangaroo court of masked judges and accept their judgment as final with neither criticism nor complaint. It is time to unmask those anonymous reviewers and make them stand and be accountable for their judgments!



Summary of my Major Submissions and Rejections

The full text of the reviewers' comments and my rebuttals can be seen by clicking on the paper and following the link "Rejected".

Lehar S. (1994) "Harmonic Resonance in Visual Perception Suggests a Novel Form of Neural Communication."

Editor

There is a consensus among all readers that the topic is interesting and the work contains some novel ideas. There is also agreement on several serious problems. One problem, mentioned by all three reviewers, concerns the writing and organization of the manuscript. This stylistic issue may interact with more substantive ones. ... the speculation about a new form of neural coding is fascinating but mainly a conjecture at this point. ... Accordingly, I must decline the paper for publication in Perception & Psychophysics.

Reviewer A

Maybe I'm not the right person to evaluate this MS: I found it irritating because of the many adhockeries and nonsequiturs. The MS is of a purely theoretical (to my taste: speculative) nature. The theory isn't really firmly grounded in empirical fact and doesn't lead to any hard, testable predictions. Thus in my opinion it doesn't fit very well in this journal: there exists journals expressly aimed at contributions like this (I expect that speculation has a valid place in science)

Reviewer B

...While the problem is extremely important and the approach is very attractive, the outcome is somewhat confusing, maybe because of the style of writing. I guess a more carefully written full length paper has a better chance for making the theory clear.

Reviewer C

...The idea of harmonic interactions among groups of cooperative cells is intriguing, and explanations of visual phenomena in terms of dynamic interactions are certainly topical. Furthermore, as current theoretical accounts of the perception of illusory corners are lacking, any contribution to explaining such phenomena would be welcome. ... However, this manuscript is simply not ready for publication.

Lehar S. (1996) "A Gestalt Bubble Model of the Interaction of Lightness, Brightness, and Form Perception"

Editor

The general feeling is that your work is interesting and imaginative, but not mature for appearing in print. Therefore, I regret that I cannot accept your paper for publication in the special issue of Perception.

...two of the referees were in fact quite favorably impressed by your work, and showed their interest and appreciation by taking the time to read it carefully and offer thoughtful and detailed comments. The reason why your paper was ultimately considered inappropriate for publication, I am glad to notice, is not that your model is implausible or too radical or simply unconventional, but that in its present version it runs into a number of problems.

...I believe that your paper presents a fresh and exciting approach to very complex issues, and that an ammended version of the model well deserves to be made available to the scientific community.

Reviewer 1

There are a number of aspects of this paper that I like. They include the stress on lower-level processes, the principle of isomorphism and the importance of explaining appearance, the need for a full spatial representation of 3-D forms, and the interaction of local and global processes. Also, the paper contains a number of interesting observations and creative insights. However I cannot recommend this paper for publication in its present form.

...In conclusion, although I see this as an interesting piece of work, in the current form the deficiencies still outweigh the positive aspects. The author tries to deal with a variety of classical percepual problems, issues on which there are a lot of empirical data and with which a great number of excellent thinkers have grappled for a long time. It is a bit unrealistic to hope that these issues can all be solved in one fell swoop. ...Finally, it is, let's say, unproductive to claim of one's own model that it 'represents so great a departure from the conventional approach to modeling perception', when there are others, starting with

Grossberg and associates, but also other schools, who have done similar work, but both mathematically specified and simulated.

Reviewer 2

On Page 7, Line 5, the Author says: "Since the scope of the model, i.e. the breadth of data that it is designed to explain, is considerably greater than that normally addressed by such models, this model will of necessity be somewhat sketchy, and many details will remain to be defined." Then, the title should read "A Gestalt bubble sketch..." rather than "A Gestalt bubble model..."

On Page 17, line 17 from below, he says: "In other words, this model represents a hypothesis which remains to be tested for feasibility." Then, why did not the Author test it?

The manuscript is excessively long. It takes 13 pages before the "model" is presented. In my opinion, these pages should be reduced to 1, or 2 at most. The "model," being a sketch, should also be presented using a smaller number of pages, let us say 3, or 4 at most. Final comments should also be short, 1 or 2 pages. ... After the Author has done this, he should resubmit his new manuscript for further review.

Reviewer 3

The idea of reification is very interesting; as for the model, I admit that probably I failed to understand it fully, but nevertheless I reckon it contains trivial mistakes. Perhaps the author should first write an article explaining in full details the thesis of reification only, since this deserves to be evaluated separately from the model ...the model should be presented, in a modified version, in a second article, since one cannot introduce too many new ideas in a single work.

P. 28: "If the model accurately reflects the nature of the subjective experience of the percept, and offers a quantitative isomorphic representation which corresponds to the subjective experience, then the model is unassailable as a perceptual model, even if its neurophysiological correlate remains to be identified"; I agree: such a model would be unassailable, because it would be an exact copy of the perceptual world, but one wonders what its utility would be.

...It is clear that the ideas are extremely clever, but it is also clear that the author has a theoretical, more than an experimental inclination. This is not a criticism, of course, but the author must be aware that a referee with a strong experimental mentality can be very annoyed by the kind of errors that this work contains, and end up rejecting an article that has other good points; that may seem unfair to the author, but it happens, and I think he should keep this factor in due account if he wants to see his work published. ... I would also urge the author to take care of details, such as the incomplete quotations, that perhaps he considers unworthy of his attention when proposing a new theory of perception, but that are important, even though of no scientific relevance at all, because the referees can found them irritating.

[Lehar S. & McLoughlin N. (1995) "Gestalt Isomorphism I: Emergence and Feedback in the Perception of Lightness, Brightness, and Illuminance"]

[Lehar S. & McLoughlin N. (1995) "Gestalt Isomorphism II: The Interaction Between Brightness Perception and Three-Dimensional Form."]

Editor

Although the reviewers still have criticisms of it, it seems to me that Paper 1 may be close to being acceptable for publication. ... Paper 2 ... is still open to a range of objections. Reviewer 1 lists a number of problems which can be summarised by saying that your approach may work for particular examples of simple scenes, but that one can easily think of equally likely scenes which would be wrongly perceived.

When I read the reviews of these latest revisions, I confess that I was disappointed: it did not seem to me that much progress had been made in producing acceptable versions of the papers. What the reviewers and I like about the work is that it tries to identify (sometimes for the first time) serious problems which visual systems need to solve. Unfortunately, the work is less successful in producing robust solutions to these problems. There is no shame in this (they are just very hard problems) but the worrying thing from my point of view is that successive versions of the papers seem to contain increasingly ad hoc and unsuccessful postulates. Although the review process is a negotiation between authors, reviewers, and editors, it does not seem to me that progress is being made: the new problems seem just as severe as the old ones. Although you (I think) and I (certainly) appreciate the efforts of the reviewers, I think that it would not be fair to ask more of them.

Reviewer 1

As I have noted in previous reviews of this paper, it contains many interesting and innovative ideas that deserve to be presented to the perception community. It shows an advance with respect to the earlier versions. ... However, in my judgement the paper still shows a number of weaknesses that I will document below. My main complaints are, first, that in a number of cases the authors accounts may apply for certain select cases but may not apply for other, equally legitimate ones, which they fail to discuss, second, that some aspects of the working of the model are unclear, and third, that some modeling solutions lack psychological credibility.

The notion that I find most troublesome in this work is the new section containing the reverse ray-tracing algorithm, as far as I understand it. It is obviously of great importance for the authors, because they say that it is this aspect of their approach that offers a solution to 'many troublesome issues in visual perception' such as transparency, specularity, mutual illumination, various types of shadows etc. The main problem that I see with this idea is that it appears to be a computational algorithm with no shred of evidence for its psychophysical reality, and therefore, in my judgement, would be poorly received by perception researchers. The very idea that the perceptual system incorporates a 'model of physical propagation of light through space' sounds very far fetched and does not have any phenomenological support. In everyday perception we just don't see any light rays, we see objects in space. ... In sum, it is not clear to me how a model which would incorporate such an extensive, detailed, and essentially correct knowledge of the external physical situation would ever fall prey to a perceptual illusion.

Finally, in their closing statement the authors talk about the 'illusion of consciousness' as being the key inspiration of the Gestalt movement, 'from which all of their other ideas were developed'. If they really believe this to be a historical fact, they should at least make a list of all the gestaltists 'other ideas' and document for each of them exactly how it was developed from the 'illusion of consciousness', otherwise this is just a piece of rhetorics that might sound like a nice way to conclude the paper. Furthermore, I do not see in Köhler's intricate discussions the basis for any such blunt statement as the author's claim that 'the world we perceive around us is an illusion'.

Reviewer 2

I think that the first paper should be published. It addresses many crucial topics of current perceptual research. Of course, the model is still vague, as authors themselves admit, but I assume that it will be elaborated in further writings. As to the second paper, I think there is a major problem that should be solved before its publication: its length.

Lehar (1999) "Harmonic Resonance Theory: an Alternative to the "Neuron Doctrine" Paradigm of Neurocomputation to Address Gestalt properties of perception"

Editor

The reviewers agree that you have tackled an important and very interesting issue and that your approach has novelty and may have promise. However, both of them criticize the manuscript on the same ground, that you have not developed your approach beyond the metaphor stage.

Reviewer A

The manuscript addresses an issue of considerable theoretical significance. On a general level, the reviewer is sympathetic to the view developed in it. The paper presents material which is not universally available and would thus be of interest to a broader public. Also, it is written in a fine stile, though a bit too essayistic relative to its subject. Unfortunately, most of the article is designed as if it were going to offer a solution of the exposed problem while actually it is at best diagnosing the deficiencies of a widely supported view and providing an alternative by way of example, presenting analogies which point out some features of a possible solution.

Despite of these weaknesses, I would like to encourage the author to submit a revised paper which, while maintaining the general viewpoint, is less ambitious in its goals and, providing a detailed critical review, more analytic and informative.

Reviewer B

This is a very ambitious but ultimately unsatisfactory paper. It addresses one of the most important unsolved problems in perceptual theory: the mechanism by which certain holistic (gestalt) properties of perception arise from neural activity. The author proposes the (somewhat) novel metaphor of some sort of neural harmonic resonance to replace the "neuron doctrine" that dominates current theories of neural computation.

The big problem with the present paper is that the author never gets beyond the metaphor stage. Yes, harmonic resonance and standing waves on vibrating plates appear to bear some interesting relations to Gestalt phenomena of perception, but this observation leads to no useful theoretical work. There are some very sketchy ideas about how neural activity might support resonance phenomena, but even these are mostly handwaving. No percptual phenomena are explained and no predictions are derived or tested.

Lehar (2000) "Computational Implications of Gestalt Theory: The Role of Feedback in Visual Processing."

Editor

I'm enclosing two reviews ... As you'll see, both find much to admire in the manuscript but, in the end, neither considers it to be appropriate for publication in Cognitive Psychology.

Following my independent reading of the manuscript, I must agree, and conclude that I can't accept the manuscript for Cognitive Psychology. This actually was not an easy decision for me. I have to admit that I am swayed (probably overly swayed) by good clear writing and, as was noted by Reviewer A, your writing in this manuscript was exemplary. It was a pleasure to be able to pretty much understand this quite technical material on the first pass.

I should say that the reviewers, unlike me, have a great deal of expertise in the subject matter of your manuscript. While I could, as I indicated follow your logic pretty well, I wasn't really in a position to evaluate the bases of your theory from the perspective of how it fits into the literature. But both reviewers, alas, conclude that the fit is wanting.

Reviewer 1

This is a very well written paper. The "tutorial" on edge processing is excellent; and much better than what is usually written in textbooks on image processing. The simulations are very nice (although I have a few quibbles).

My problem with the paper is with regard to the core ideas. The key characteristics of the resulting system end up being very similar to Grossberg & Mingolla's (1985) neural network model. The author argues that his system is only an example of the more general approach. Maybe so, but then he would do better to indicate the power of his approach by finding a conclusion that has not already been reached by other means. ... The net result is that I don't feel the paper fully lives up to its goal of demonstrating the MLRF approach.

Gestalt theory: In the Abstract, Introduction and elsewhere, the author seems to treat Gestalt theory as something that needs to be explained. This, I think, is a mistake. Gestalt theory is a collection of ideas and (partial) explanations. It is not experimental data that needs to be explained by models (neural network or perceptual).

Justification for processes: After reading the paper I am left wondering ***why*** the visual system would be built with the levels proposed by the MLRF approach. The current model seems to be built to account for the percepts of illusory contours. But the visual system surely did not evolve to allow it to see Kanisza triangles!

Reviewer 2

This model clearly borrows many of properties of the BCS/FCS model Grossberg and colleagues. It differs in terms of some of the specific mechanisms that it proposes in order to account for many of the same phenomena, but it is generally quite similar to the BCS/FCS model both in spirit and in terms of presumed computational stages. The author's most significant point of departure from Grossberg's approach is his disavowal of the research program of attempting to identify processing stages in the model with actual neural processes in the brain.

On the positive side, the model introduced here suggests some novel computational strategies for performing some of the same functions that are performed by the BCS/FCS model. In particular, the current model utilizes feedback in different ways than it is utilized in the BCS/FCS model. If an argument were given for the superiority of the current model in accounting for perceptual data, then a paper describing the model and its novel properties would make a significant contribution to the cognitive psychology literature. But that would be a different paper.

Lehar S. (2000) The Dimensions of Conscious Experience: A quantitative phenomenology.
Submitted to Journal of Consciousness Studies.

Dear Steve,

I have heard from my two readers of your paper and I sorry to say that neither felt able to recommend its publication in JCS.

Since neither submitted a detailed report, I cannot give you the reasons for their decision. This is annoying for you as an author and awkward for me as an editor. But if a busy scholar reads through a paper, judges it unsuitable for publication, and decides not to spend further time on it, we have to accept that. At least they both responded fairly quickly.

Author's Response

Dear Dr. Freeman,

I have had my share of rejections from journals, but this is really the most insulting and unprofessional rejection I have ever received! We are all busy scholars, and nobody has time to waste. However if the reviewers did not even have time to jot down a quick email report of *WHY* they considered the paper unfit for publication, then the paper cannot really be said to have been reviewed at all!

The standard peer review process is already stacked against outsiders with a really new perspective, as was explained by Kuhn. The one-way anonymitiy accorded to the reviewers essentially releases them from any accountability for their judgements. Furthermore, reviewers are necessarily selected from those whose whole career is committed to the older paradigm. To stack the odds even more by not even requiring an explanation for their rejection is really going too far!

In the name of the reputation of your journal as a serious and professional academic institution, it only seems fair that either I receive a detailed report from the reviewers to which I am given the opportunity to respond, or the journal should seek out alternate reviewers who take their responsibiliy a little more seriously!

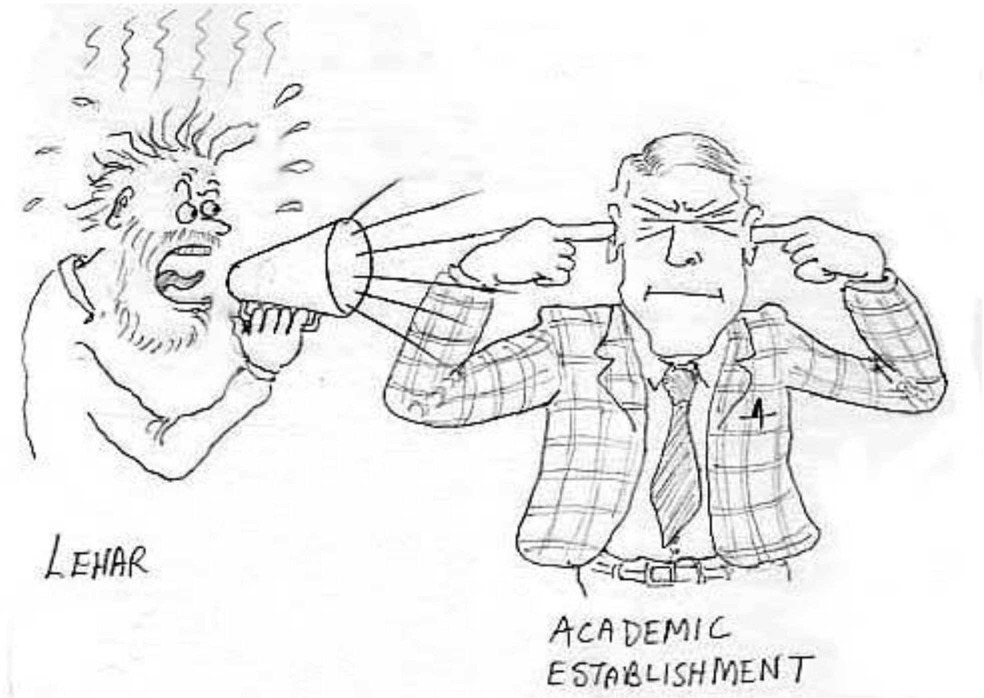
(see full exchange and two absurd nonsensical reviews)

Author's Final Comment

I've had my share of rejections in science, in fact I 've had ***more*** than my share of them. But this one is by far the most shameful and disgraceful rejection yet! This "review", if it can be called such, is a vivid demonstration of exactly what is wrong with the anonymous peer review process! There is ***no accountability*** on the part of the anonymous reviewers to stand behind their statements, some of which are so absurd and indefensible as to be ***laughable!***

The review process should be an exchange between the authors and the reviewers, with the editor acting as the final arbiter who takes into account the arguments of both sides. In this case however the editor, the Reverend Anthony Freeman, exhibited the most gross and egregious dereliction of his responsibilities as editor. There was never any kind of exchange between the author and the reviewers. Instead, the reviewer's word was accepted as the final judgement, even when the reviewers ***couldn't be bothered*** to actually ***write a review!*** As for the one reviewer who did (after much prodding and protestation) actually deign to write a review, his statements in that review are so ***absurd*** and ***indefensible*** that Reverend Anthony Freeman brings ***shame*** on himself and on the Journal of Consciousness Studies for allowing them to pass unchallenged!

In fact the real reason why this paper was not really reviewed at all was because the editor and the reviewers are all ***naive realists,*** and they feel ***threatened*** by this challenge to their cherished beliefs. But instead of standing up to the challenge and addressing the issues raised in this paper, they choose the ***cowards way out*** by hiding behind their anonymity and rejecting the paper outright ***without any kind of meaningful discussion.*** And the reason why they choose this cowards way out is because they have ***NO ANSWER*** to my challenge of their naive realism! And Reverend Anthony Freeman lets them ***get away*** with this perversion of the review process by not holding them accountable to provide a reasoned response, or ***any response at all*** for that matter, if they don't feel like providing one!



Steve Lehar

Reverend Freeman replies

Lehar, S. (2000) The Function of Conscious Experience: An analogical paradigm of perception and behavior. **Submitted to Consciousness & Cognition.**

Summary of absurd and unconscionable review process.

Editor's Rejection:

"the changes do not address my previous editorial comments."

Author's Response:

What? That's ***IT?*** Thats all you have to say?

No discussion? No argument? Just rejected? Simple as that?

After **ALL THIS???**

HAHAHAHAHAHAHAHAHAHAHA!!!!

You, Dr. Baars, are a **Donkey's Ass** of the ***highest caliber!***

Your journal would be more appropriately named:

"Unconsciousness and Incogitance"

There ought to be a disclaimer in the mission statement of the journal:

"Only ***Naive Realists*** need apply!"

"We will not publish papers that challenge our cherished ***Naive Realist*** assumptions!"

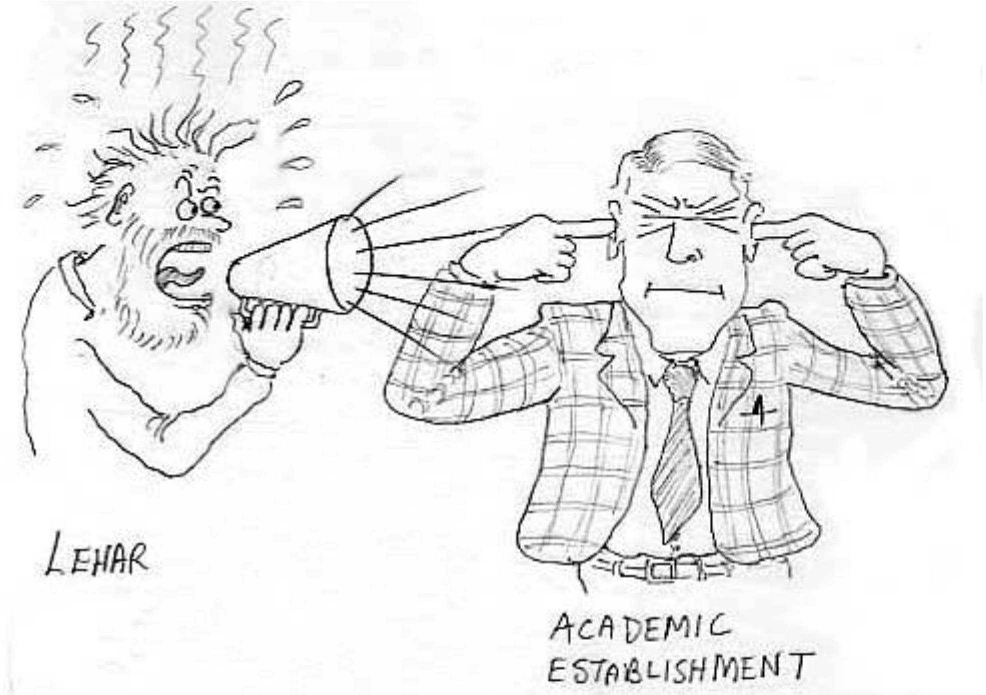
Imagine! Professional philosophers, and they're ALL **Naive Realists!** Who would have thunk it?

You won't get away with it Baars! You can't suppress this idea forever! The truth will come out in the end! And when it does, you and your miserable journal will be a **laughing stock!** This whole shameful process is fully documented on my web site for all the world to see!

You can go ahead and ignore me now, Baars. But **you haven't heard the last of my theories!**

HAHAHAHAHAHAHAHAHAHAHA!!!!

Do you see yourself in this picture Dr. Baars?



Steve Lehar

Dr. Baars Responds

[**Lehar S. (2003) Gestalt Isomorphism and the Primacy of the Subjective Conscious Experience: A Gestalt Bubble Model**](Lehar S. (2003) Gestalt Isomorphism and the Primacy of the Subjective Conscious Experience: A Gestalt Bubble Model)

Submitted September 1999.

See **extraordinarily** long and acrimonious exchange with 6 reviewers over 4 years!

Finally published in The Behavioral and Brain Sciences, (2003), 26(4) 375-444.

Return to Steve Lehar

Lehar S. (2008) The Constructive Aspect of Visual Perception: A Gestalt Field Theory Principle of Visual Reification Suggests a Phase Conjugate Mirror Principle of Perceptual Computation

Submitted August 2008.

Editor

Two reviewers have seen your manuscript "The Constructive Aspects of Visual Perception..." and while both found it interesting and potentially significant, they also both though it premature for peer commentary at this time. One suggested "his creative but speculative ideas might be tested in a more specialized journal".

Reviewer 1

...some mention of nonlinear wave phenomena in the brain towards the end, which if elaborated in a plausible way (including how it might implement nonlinear phase conjugation), which won't be a trivial enterprise, could have formed a basis for a new, though speculative framework for understanding perception. In its present form, I cannot recommend the paper for a full review.

Reviewer 2

I can assure that (the Lehar ms) is serious science. ... I think he needs to tighten things up before (it is ready for commentary)

Return to Steve Lehar

I have given up on the peer review process as an exercise in frustrating futility! I have lost all confidence in the fairness of the process. I am constantly amazed by the profound depths of ignorance displayed by the parade of anonymous reviewers who have reviewed my work.

We all know how the peer review system *really* works. Scientific unknowns with big new ideas are automatically rejected on the assumption that only people with established academic credentials are permitted to propose big new ideas. But of course they never do, having built their whole career on theories that were deemed acceptable to the prior generation of orthodox academicians. The whole system is set up to preserve scientific orthodoxy and to delay and postpone challenging new theories at least until the older generation of rigid non-thinkers have retired from their comfortable tenured positions.

One day the truth will come out, and the true significance of my theories will finally be generally recognized. When that happens, then I will be able to get virtually ***anything*** published without any kind of serious review! But by that time it will be too late for those publications to do me any good!

It is high time to change the system, and to hold anonymous reviewers accountable for their judgments. Any man who will not stand by their own words in a review, does not deserve to have those words considered for the review process.