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Authenticity in Art

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Abstract:
Works of art possess what we may call *nominal authenticity*, defined simply as the correct identification of the origins, authorship, or provenance of an object, ensuring that an object of aesthetic experience is properly named. However, the concept of authenticity often connotes something else, having to do with an object's character as a true expression of an individual's or a society's values and beliefs. This second sense of authenticity can be called *expressive authenticity*. The following discussion will summarize some of the problems surrounding nominal authenticity and will conclude with a general examination of expressive authenticity. This paper is excerpted from a longer version published in the Oxford Handbook of Aesthetics.

Introduction

"Authentic," like its near-relations, "real," "genuine," and "true," is what J.L. Austin called a "dimension word," a term whose meaning remains uncertain until we know what dimension of its referent is being talked about. A forged painting, for example, will not be inauthentic in every respect: a Han van Meegeren forgery of a Vermeer is at one and the same time both a fake Vermeer and an authentic van Meegeren, just as a counterfeit bill may be both a fraudulent token of legal tender but at the same time a genuine piece of paper. The way the authentic/inauthentic distinction sorts out is thus context-dependent to a high degree. Mozart played on a modern grand piano might be termed inauthentic, as opposed to being played on an eighteenth-century forte-piano, even though the notes played are authentically Mozart's. A performance of Shakespeare that is at pains to recreate Elizabethan production practices, values, and accents would be to that extent authentic, but may still be inauthentic with respect to the fact that it uses actresses for the female parts instead of boys, as would have been the case on Shakespeare's stage. Authenticity of presentation is relevant not only to performing arts. Modern museums, for example, have been criticized for presenting old master paintings in strong lighting conditions which reveal detail, but at the same time give an overall effect that is at odds with how works would have been enjoyed in domestic spaces by their original audiences; cleaning, revarnishing, and strong illumination arguably amount to inauthentic presentation. Religious sculptures created for altars have been said to be inauthentically displayed when presented in a bare space of a modern art gallery (see Feagin 1995).

Whenever the term "authentic" is used in aesthetics, a good first question to ask is, *Authentic as opposed to what?* Despite the widely different contexts in which the authentic/inauthentic is applied in aesthetics, the distinction nevertheless tends to form around two broad categories of sense. First, works of art can be possess what we may call *nominal authenticity*, defined simply as the correct identification of the origins, authorship, or provenance of an object, ensuring, as the term implies, that an object of aesthetic experience is properly named. However, the concept of authenticity often connotes something else, having to do with an object's character as a true expression of an individual's or a society's values and beliefs. This second sense of authenticity can be called *expressive authenticity*. The following discussion will summarize some of the problems surrounding nominal authenticity and will conclude with a general examination of expressive authenticity.

2. Nominal Authenticity

2.1 Forgery and Plagiarism

Many of the most often-discussed issues of authenticity have centred around art forgery and plagiarism. A forgery is defined as a work of art whose history of production is misrepresented by someone (not necessarily the artist) to an audience (possibly to a potential buyer of the work), normally for financial gain. A forging artist paints or sculpts a work in the style of a famous artist in order to market the result as having been created by the famous artist. Exact copies of existing works are seldom forged, as they will be difficult to sell to knowledgeable buyers. The concept of forgery

necessarily involves *deceptive intentions* on the part of the forger or the seller of the work: this distinguishes forgeries from innocent copies or merely erroneous attributions. Common parlance also allows that an honest copy can later be used as a forgery, even though it was not originally intended as such, and can come to be called a "forgery." In such cases a defrauding seller acts on an unknowing buyer by misrepresenting the provenance of a work, perhaps even with the additions of a false signature or certificate of authenticity. The line between innocent copy and overt forgery can be, as we shall see, difficult to discern.

Plagiarism is a related but logically distinct kind of fraud. It involves the passing off as one's own of the words or ideas of another. The most obvious cases of plagiarism have an author publishing in his own name a text that was written by someone else. If the original has already been published, the plagiarist is at risk of being discovered, although plagiarism may be impossible to prove if the original work, or all copies of it, is hidden or destroyed. Since publication of plagiarized work invites wide scrutiny, plagiarism is, unlike forgery, a difficult fraud to accomplish as a public act without detection. In fact, the most common acts of plagiarism occur not in public, but in the private sphere of work that students submit to their teachers.

2.2 Honest Misidentification

Authenticity is contrasted with "falsity" or "fakery" in ordinary discourse, but, as noted, falsity need not imply fraud at every stage of the production of a fake. Blatant forgery and the intentional misrepresentation of art objects has probably been around as long as there has been an art market — it was rife even in ancient Rome. However, many works of art that are called "inauthentic" are merely misidentified. There is nothing fraudulent about wrongly guessing the origins of an apparently old New Guineamask or an apparently eighteenth-century Italian painting. Fraudulence is approached only when what is merely an optimistic guess is presented as well-established knowledge, or when the person making the guess uses position or authority to give it a weight exceeding what it deserves. The line, however, that divides unwarranted optimism from fraudulence is hazy at best. (Any worldly person who has ever heard from an antique dealer the phrase "It's probably a hundred and fifty years old" will understand this point: it's probably not that old, and perhaps not even the dealer himself could be sure if he's merely being hopeful or playing fast and loose with the truth.)

Authenticity, therefore, is a much broader issue than one of simply spotting and rooting out fakery in the arts. The will to establish the nominal authenticity of a work of art, identifying its maker and provenance — in a phrase, determining how the work came to be — comes from a general desire to understand a work of art according to its original canon of criticism: what did it mean to its creator? How was it related to the cultural context of its creation? To what established genre did it belong? What could its original audience have been expected to make of it? What would they have found engaging or important about it? These questions are often framed in terms of artists' intentions, which will in part determine and constitute the identity of a work; and intentions can arise and be understood only in a social context and at a historical time. External context and artistic intention are thus intrinsically related. We should resist, however, the temptation to imagine that ascertaining nominal authenticity will inevitably favour some "old" or "original" object over a later artefact. There may be Roman sculptures, copies of older Greek originals, which are in some respects aesthetically better than their older prototypes, as there may be copies by Rembrandt of other Dutch artists that are aesthetically more pleasing than the originals. But in all such cases, value and meaning can be rightly assessed only against a background of correctly determined nominal authenticity (for further discussion see Dutton 1983; Goodman 1976; Currie 1989; Levinson 1990).

2. 3 The Igorot of Luzon

Forgery episodes such as van Meegeren's Vermeers are unproblematic in terms of nominal authenticity: there is a perfectly clear divide between the authentic Vermeers and the van Meegeren fakes. But there are areas where determining nominal authenticity can be extremely fraught. Consider the complexities of the following example. The Igorot of northern Luzon traditionally carved a rice granary guardian figure, a *bulul*, which is ceremonially treated with blood, producing over years a deep red patina which is partially covered with a black deposit of grease from food offerings. These objects were already being made for tourists and for sale at international exhibitions in the 1920s, and one famous virtuoso Igorot carver, Tagiling, was by then producing figures on commission by local families

and at the same time for the tourist trade. *Bululs* are still in traditional use, but specialized production of them ceased after the Second World War. Today, if a local wants a *bulul*, it is purchased from a souvenir stand and then rendered sacred by subjecting it to the appropriate ceremony. “The result,” Alaine Schoffel has explained, “is that in the rice granaries one now finds shoddy sculptures slowly becoming covered with a coating of sacrificial blood. They are authentic because they are used in the traditional fashion, but this renders them no less devoid of aesthetic value.” We do not necessarily have to agree with Schoffel’s aesthetic verdict on “shoddy” souvenirs to recognize that he is legitimately invoking one of the many possible senses of “authenticity”: the authentically traditional.

2. 4 Authenticity in Music

Arguments over the use and presentation of art are nowhere more prominent than in music performance. This is owing to the general structure of Western, notated music, in which the creation of the work of art is a two-stage process, unlike painting and other plastic arts. Stand in front of Leonardo’s *Ginevra de’ Benci* in the National Gallery in Washington, and you have before you Leonardo’s own handiwork. However much the paint may have been altered by time and the degenerative chemistry of pigments, however different the surroundings of the museum are from the painting’s originally intended place of presentation, at least, beneath the shatterproof non-reflective glass you gaze at the very artefact itself, in its faded, singular glory. No such direct encounter is available with a performance of an old musical work. The original work is specified by a score, essentially a set of instructions, which are realized aurally by performers, normally for the pleasure of audiences. Because a score underdetermines the exact sound of any particular realization, correct performances may differ markedly (Davies 1987).

With a painting, therefore, there normally exists an original, nominally authentic object that can be identified as “the” original; nothing corresponds to this in music. Even a composer’s own performance of an instrumental score — say, Rachmaninoff playing his piano concertos — cannot fully constrain the interpretive choices of other performers or define for ever “the” authentic performance. Stephen Davies argues that a striving towards authenticity in musical performance does not entail that there is one authentic ideal of performance, still less that this would be a work’s first performance or whatever a composer might have heard in his head while composing the piece. The very idea of a performance art permits performers a degree of interpretive freedom consistent with conventions that govern what counts as properly following the score (Davies 2001; see also Godlovitch 1998; Thom 1993).

Nevertheless, the twentieth-century witnessed the development of an active movement to try to understand better the original sounds especially of seventeenth-and eighteenth-century European music. This has encouraged attempts to perform such music on instruments characteristic of the time, in line with reconstructions of the past conventions that governed musical notation and performance (Taruskin 1995). This concern with authenticity can be justified by the general, though not inviolable,

principle which holds that “a commitment to authenticity is integral to the enterprise that takes delivery of the composer’s work as its goal. If we are interested in performances as of the works they are of, then authenticity must be valued for its own sake” (Davies 2001). This interest can take many forms — playing Scarlatti sonatas on harpsichords of a kind Scarlatti would have played, instead of the modern piano; using a Baroque bow over the flatter bridge of a Baroque violin to achieve more easily the double-stopping required of the Bach solo violin works; performing Haydn symphonies with orchestras cut down from the late Romantic, 100-player ensembles used by Brahms or Mahler. These practices are justified by taking us back in time to an earlier performing tradition and, in theory, closer to the work itself.

In this way of thinking, the purpose of reconstructing a historically authentic performance is to create an occasion in which it sounds roughly as it would have sounded to the composer, had the composer had expert, well equipped musicians at his disposal. Enthusiasm for this idea has led some exponents of the early music movement to imagine that they have a kind of moral or intellectual monopoly on the correct way to play music of the past. In one famous put-down, the harpsichordist Wanda Landowska is said to have told a pianist, “You play Bach your way, I’ll play him his way.” The question for aesthetic theory remains: *What is Bach’s way?* If the question is framed as purely about instrumentation, then the answer is trivially easy: the Bach keyboard *Partitas* are authentically played in public only on a harpsichord of a kind Bach might have used. But there are other ways in which the music of Bach can be authentically rendered. For instance, Bach’s keyboard writing includes interweaved musical voices which, under the hands of a skilled pianist such as Glenn Gould, can often be revealed more clearly on a modern concert grand than on a harpsichord (Payzant 1978; Bazzana 1997).

However, if an authentic performance of a piece of music is understood as one in which the aesthetic potential of the score is most fully realized, historic authenticity may not be the best way to achieve it. We would not go back to productions of Shakespeare plays with boys taking the female roles simply because that was the way it was done in Shakespeare’s time. We regard the dramatic potential of those roles as ideally requiring the mature talents of actresses, and write off the Elizabethan practice of boy actors as an historic accident of the moral climate of Shakespeare’s age. We assume, in other words, that Shakespeare would have chosen women to play these parts had he had the option. Similarly, the Beethoven piano sonatas were written for the biggest, loudest pianos Beethoven could find; there is little doubt that he would have favored the modern concert grand, if he had had a choice. The best attitude towards authenticity in music performance is that in which careful attention is paid to the historic conventions and limitations of a composer’s age, but where one also tries to determine the larger artistic potential of a musical work, including implicit meanings that go beyond the understanding that the composer’s age might have derived from it. In this respect, understanding music historically is not in principle different from an historically informed critical understanding of other arts, such as literature or painting.

3. Expressive Authenticity

In contrast to nominal authenticity, there is another fundamental sense of the concept indicated by two definitions of “authenticity” mentioned in the *Oxford English Dictionary*: “possessing original or inherent authority,” and, connected to this, “acting of itself, self-originated.” This is the meaning of “authenticity” as the word shows up in existential philosophy, where an authentic life is one lived with critical and independent sovereignty over one’s choices and values; the word is often used in a similar sense in aesthetic and critical discourse. In his discussion of authenticity of musical performance, Peter Kivy points out that, while the term usually refers to historical authenticity, there is another current sense of the term: performance authenticity as “faithfulness to the performer’s own self, original, not derivative or aping of someone else’s way of playing” (Kivy 1995). Here authenticity is seen as committed, personal expression, being true musically to one’s artistic self, rather than true to an historical tradition. From nominal authenticity, which refers to the empirical facts concerning the origins of an art object — what is usually referred to as provenance — we come now to another sense of the concept, which refers less to cut-and-dried fact and more to an emergent value possessed by works of art. I refer to this second, problematic sense of authenticity as *expressive authenticity*.

3.1 Authenticity and Audiences

Too often discussions of authenticity ignore the role of the audience in establishing a context for creative or performing art. To throw light on the importance of an audience in contributing to meaning in art, consider the following thought-experiment. Imagine the complicated and interlocking talents, abilities, stores of knowledge, techniques, experience, habits, and traditions that make up the art of opera — for example as it is presented, or embodied, by a great opera company, such as La Scala. There is the music and its history, the dramatic stories, the staging traditions, the singers, from the chorus to the international stars, along with the distribution channels for productions — broadcasts, videos, and CDs. In addition, surrounding opera there is a whole universe of criticism and scholarship: historical books are written, academic departments study the music and the art and technique of singing, reviews for new casts and productions appear in magazines and daily newspapers. When the lights go down for a La Scala performance, the curtain rises not on an isolated artistic spectacle, but on an occasion that brings together the accrued work of countless lifetimes of talent, knowledge, tradition, and creative genius.

Now imagine the following: one day La Scala entirely loses its natural, indigenous audience. Local Italians and other Europeans stop attending, and local newspapers cease to run reviews of performances. Nevertheless, La Scala remains a famous attraction for visitors, and manages to fill the hall every night with busloads of tourists. Further, imagine that, although these nightly capacity crowds — consisting of people from as far away as Seoul, Durban, Yokohama, Perth, Quito, and Des Moines— are polite and seem to enjoy themselves, nevertheless, for nearly all of them their La Scala experience is the first and last opera they will ever see. They are not sure when to applaud, and although they are impressed by the opulent costumes, dazzling stage-settings, massed chorus scenes, and sopranos who can sing very high, they cannot make the sophisticated artistic discriminations that we would associate with traditional La Scala audiences of the nineteenth and twentieth centuries.

How would we expect the demise of the traditional audience to affect the art of opera as practised at this imaginary La Scala? The problem here is not necessarily the loss of good singers or orchestral pit musicians: it is rather the loss of a *living critical tradition* that an indigenous audience supplies for any vital artform. It is impossible to engage in this thought-experiment without concluding that in the long term operatic art as practised at such a La Scala would steeply decline. A PacificIslanddancer was once asked about his native culture. “Culture?” he responded. “That’s what we do for the tourists.” But if it is only for the tourists, who have neither the knowledge nor the time to learn and apply a probing canon of criticism to an artform, there can be no reason to expect that the artform will develop the complex expressive possibilities we observe in the great established art traditions of the world (Dutton 1993).

Why, then do critics and historians of art, music, and literature, private collectors, curators, and enthusiasts of every stripe invest so much time and effort in trying to establish the provenance, origins, and proper identity — the nominal authenticity — of artistic objects? It is sometimes cynically suggested that the reason is nothing more than money, collectors’ investment values — forms of fetishizing, commodification — that drives these interests. Such cynicism is not justified by facts. The nominal authenticity of a purported Rembrandt or a supposedly old Easter Island carving may be keenly defended by its owners (collectors, museum directors), but the vast majority of articles and books that investigate the provenance of art works are written by people with no personal stake in the genuineness of individual objects. Moreover, when this comes into question, issues of nominal authenticity are as hotly debated for novels and musical works in the public domain as they are for physical art objects with a specific commodity value.

Establishing nominal authenticity serves purposes more important than maintaining the market value of an art object: it enables us to understand the practice and history of art as an intelligible history of the expression of values, beliefs, and ideas, both for artists and their audiences — and herein lies its link to expressive authenticity. Works of art, besides often being formally attractive to us, are manifestations of both individual and collective values, in virtually every conceivable relative weighting and combination. Clifford Geertz remarks that “to study an art-form is to explore a sensibility,” and that “such a sensibility is essentially a collective formation” whose foundations “are as wide as social existence and as deep” (Geertz 1983). Geertz is only partially right to claim that the sensibility

expressed in an art object is in every case *essentially* social: even close-knit tribal cultures produce idiosyncratic artists who pursue unexpectedly personal visions within a socially determined aesthetic language. Still, his broader description of works of art, tribal or European, is generally apt, along with its corollary is that the study of art is largely a matter of marking and tracing relationships and influences.

This explains why aesthetic theories that hold that works of art are just aesthetically appealing objects — to be enjoyed without regard to any notion of their origins — are unsatisfactory. If works of art appealed only to our formal or decorative aesthetic sense, there would indeed be little point in establishing their human contexts by tracing their development, or even in distinguishing them from similarly appealing natural objects — flowers or seashells. But works of art of all societies express and embody both cultural beliefs general to a people and personal character and feeling specific to an individual. Moreover, this fact accounts for a large part, though not all, of our interest in works of art. To deny this would be implicitly to endorse precisely the concept of the eighteenth-century curiosity cabinet, in which Assyrian shards, tropical seashells, a piece of Olmec jade, geodes, netsuke, an Attic oil lamp, bird of paradise feathers, and a Maori patu might lay side by side in indifferent splendour. The propriety of the curiosity cabinet approach to art has been rejected in contemporary thought in favour of a desire to establish provenance and cultural meaning precisely because intra- and inter-cultural relationships among artworks help to constitute their meaning and identity.

4. Conclusions

Leo Tolstoy's *What Is Art?*, which was published near the end of his life in 1896, is the work of a genius nearly gone off the rails. It is famous for its fulminations not only against Beethoven, Shakespeare, and Wagner, but also even against Tolstoy's own great early novels (Tolstoy 1960). It continues, however, to be read for its vivid elaboration of a thesis that has a permanent place in the history of aesthetics: artistic value is achieved only when an artwork expresses the authentic values of its maker, especially when those values are shared by the artist's immediate community.

Tolstoy claimed that cosmopolitan European art of his time had given up trying to communicate anything meaningful to its audience in favour of amusement and careerist manipulation. Where and how Tolstoy drew the line between art that is falsely sentimental and manipulative on the one hand, and sincerely expressive on the other, has been hotly disputed (Diffey 1985). But it is impossible that these categories could be entirely dispensed with, at least in the critical and conceptual vocabulary we apply to Western art. It is more than just formal quality that distinguishes the latest multimillion-dollar Hollywoodsex-and-violence blockbuster or manipulative tearjerker from the dark depths of the Beethoven Opus 131 String Quartet or the passionate intensity of *The Brothers Karamazov*. These latter are *meant* in a way that many examples of the former cannot possibly be: they embody an element of personal commitment normally missing from much popular entertainment art and virtually all commercial advertising. Expressive authenticity is a permanent part of the conceptual topography of our understanding of art.

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Discussion

Should we care about nominal authenticity?
Dan Sperber
Sep 15, 2004 14:20 UT

Denis Dutton writes:

"*The will to establish the nominal authenticity of a work of art, identifying its maker and provenance — in a phrase, determining how the work came to be — comes from a general desire to understand a work of art according to its original canon of criticism: what did it mean to its creator? How was it related to the cultural context of its creation? To what established genre did it belong? What could its original audience have been expected to make of it? What would they have found engaging or important about it?*"

In other terms, Dutton seems to suggest that (leaving aside financial value) we care about nominal authenticity because it is correlated to expressive authenticity. If this were so, how come that most of us would much prefer being given the opportunity to see an unknown drawing by Matisse, than an indiscernible copy of it? Both impinge on our senses in exactly the same way. Both owe all their distinctive perceptible properties to the labour of Matisse. There is nothing we could learn from one about authorial intentions, cultural context of creation, and so on, that we could not learn from the other.

Nominal authenticity is a causally inert property – what philosophers call a merely Cambridge property. Expressive authenticity is a causally potent property: expressively authentic works of art have a different (and more desirable) effect on discerning viewers than expressively inauthentic ones, whether or not the viewers are independently aware of the work's expressive authenticity. In fact, the discerning viewers' reactions to a work of art are evidence (fallible, of course) of its expressive authenticity. So, it seems we should care about expressive authenticity, and not about nominal authenticity except as an indicator of probable expressive authenticity. But, as I hope my example illustrate, in our aesthetic experiences, we do care about nominal authenticity. Why?

> **Feeling the Vibes**
> Richard Minsky
> Sep 15, 2004 15:20 UT
>
> Dan Sperber asks why we care about nominal authenticity. Perhaps it is a tool that helps validate our feelings. I do not agree that both the real thing and a good fake "impinge on our senses in exactly the same way."
>
> If you don't believe that objects can preserve vibes, then you can dismiss the following. But if, like me, you collect things because of spiritual connections that you feel through them, then nominal authenticity may have some use. It's like a spiritualist finding someone through holding an item they had touched.
>
> I wrote about that in the text-e discussion a couple of years ago, in an intervention titled "The Book as Totem, Icon, Memory Object and Spiritual Channel." It was in response to Jacob Epstein's "Reading: The Digital Future." You can read it by going to http://text-e.org/debats/index.cfm?ConfText_ID=13
>
> No matter how good the fake, Matisse didn't touch it.

Pas d'esthétique sans ontologie !
Roger Pouivet
Sep 15, 2004 16:51 UT

L'article de Denis Dutton peut donner l'impression que l'authenticité est un genre comprenant deux espèces : l'authenticité nominale et l'authenticité expressive. La question de l'authenticité nominale est ontologique : la chose est-elle bien ce qu'elle est supposée être, p.e. un Rembrandt ? La question de l'authenticité expressive est (a) sémantique : la chose veut-elle bien dire ce qu'elle est supposée exprimer ? (b) "éthique" : l'auteur a-t-il ressenti ce que l'oeuvre exprime ? Cette dernière question repose sur la thèse contestable que l'expression par une oeuvre d'une propriété comme la tristesse dépend de la tristesse de son auteur.

Quelque chose gagne ou perd une propriété de Cambridge sans avoir véritablement changé : je suis maintenant plus petit que mon fils ainé sans avoir changé de taille, simplement parce qu'il a grandi. L'authenticité nominale n'est pas une propriété de Cambridge. Si on découvre qu'un Rembrandt est un faux, le tableau a changé, même si ses propriétés manifestes restent les mêmes. Car une oeuvre d'art (comme bien d'autres choses) possède des propriétés non manifestes qui sont indispensables à son appréhension et à son appréciation : être une oeuvre d'art, appartenir à un certain contexte artistique, être de la même main que d'autres oeuvres, etc. Or, toutes ces propriétés dépendent de son authenticité nominale.

Dire que nous ne devrions nous oocuper que de l'authenticité expressive revient à adopter une "théorie empiriste de l'art" (c'est maintenant l'expression consacrée) : pour apprécier une oeuvre d'art, il faut et il suffit d'avoir une expérience directe de l'oeuvre. Dès lors, la réponse à la question finale de Dan Sperber pourrait être : parce que la théorie empiriste est fausse. Une appréciation de l'oeuvre suppose son identification correcte. Pas d'esthétique sans ontologie !

Should we care about nominal authenticity?:
Jose Luis Guijarro
Sep 15, 2004 17:56 UT

In the mid fifties, the most well known Madrid literary café (the still existent Café Gijón) was the meeting place of artists and would-be artists of the time. An amateur poet very much influenced by Federico García Lorca used to show his lorquian poems to an already famous poet, who after reading them, invariably returned them back to him with the comment: “It is very bad”. This lasted for some time, until the young amateur found an unknown and unpublished poem of García Lorca and showed him to the famous poet. He read it and repeated: “It is very bad”. The young chap beamed and said: “Is that so? Well, this is a poem by García Lorca himself!”. To which the other one, without a change of tone, answered: “ Then, it is very good”.

The famous poet was perhaps the Nobel Prize winner Vicente Aleixandre, or maybe the famous literary critic and also poet, Dámaso Alonso, but in this instance it is immaterial for anybody to know the REAL author of the response, whereas in the case of García Lorca, it seemed to be crucial. There are lots of things happening here. One is naturally that the values one attaches to things or events are basically context dependent. Cultural and historical trends colour our evaluations, one of them being that which conceptualises something as a fake. Nominal authenticity, like the one in my story, or expressive authenticity, and so on, are therefore individual values that happen to be operating at one time or another. For me, they don’t have any explanatory power whatsoever.

When I say this, I am supposing everybody understands the basic explanatory requirement as Noam Chomsky proposed, namely, as something that explains WHY something is (or came to

be) the case –which in our human environment means an evolutionary understandable account. In another section, I will try to schematise my explanation on that matter.

Reply to Richard Minsky
Dan Sperber
Sep 15, 2004 22:53 UT

Are you saying that my Gedankenexperiment is impossible and that *there could not be* a drawing by Matisse and as perfect as possible a copy of it such that, in the absence of information about which is which, you would experience both in exactly the same way? If you are, I am afraid I am getting fewer vibes than you are. A pity! If you are not, if you agree that it is conceivable, then my question to Dutton stand: why do we care?

Réponse à Roger Pouivet
Dan Sperber
Sep 15, 2004 23:33 UT

Roger Pouivet aurait pu m'objecter que j'employais « propriété de Cambridge » de façon inexacte puisque la propriété d'avoir été dessiné par Matisse ne peut pas changer et donc ne peut pas faire l'objet d'un « changement de Cambridge ». J'aurais répondu qu'en effet j'employais l'expression de façon sans doute trop élargie pour désigner les propriétés causalement inertes. Dans l'hypothèse où je me situais, la propriété d'être un authentique dessin de Matisse ou celle d'être une copie si parfaite que l'observateur, qui ne dispose d'aucune information supplémentaire, est incapable de la distinguer de l'original sont sans effet sur l'appréciation de l'œuvre. Ce sont des propriétés causalement inertes. On a, si on me permet l'expression, sinon un changement de Cambridge, du moins une « différence de Cambridge ». L'observateur néanmoins préférerait que le dessin qu'il voit soit l'original plutôt que la copie, et je demandais pourquoi. (J'ai bien mon idée là dessus, mais c'est la réponse de Dutton qui m'intéresse.)

Maintenant, quand Pouivet écrit : « *Si on découvre qu'un Rembrandt est un faux, le tableau a changé* », je ne suis pas d'accord. Les croyances ou les savoirs dont fait l'objet un tableau (par exemple qu'il est ou n'est pas de la main de Rembrandt) sont, cette fois-ci, l'exemple même d'une propriété de Cambridge et un changement dans de telles croyances est l'exemple même d'un changement de Cambridge. Les changements de Cambridge peuvent avoir des conséquences : maintenant que Pouivet (qui est resté de la même taille)est plus petit que son fils aîné, il ne peut plus lui prêter ses chemises. Quand on se met à croire qu'un tableau est un faux, on lui donne moins de valeur (ou qu'un poème est vraiment de Lorca on lui en donne plus, comme dans l'anecdote que rapporte José Luis Guijarro). Mais non, le tableau (ou le poème) est resté le même, c'est nous seulement qui avons changé. « *Pas d'esthétique sans ontologie* » écrit Pouivet. Je veux bien, mais – et c'est une autre formulation de ma question originale – pourquoi et comment les propriétés de Cambridge, et plus généralement les propriétés causalement inertes, jouent-elles un rôle esthétique ?

That's right…
Richard Minsky
Sep 16, 2004 1:19 UT

I was saying that the experiment is impossible. That's the key point about authenticity. I thought you might say that those vibes I'm feeling are a characteristic of "Expressive" authenticity, and that doesn't answer your question, "why do we care?" It leaves nominal authenticity "as an indicator of probable expressive authenticity." But just because it is an indicator doesn't mean it lacks value. We care because we value it as an indicator. The key word in my previous comment was "validate."

For someone who never quite felt that Roger Pouivet's Rembrandt had the Rembrandt vibes, the revelation that it was a forgery validated the suspicion that something was lacking.

One can have an aesthetic experience with a forgery. The Cambridge properties need not affect that, though given the roles of business, snobbery, aesthetic theories, etc. in the lives of artists and audiences, one can see how someone's aesthetic experience can be a Mackay bubble.

To expand on Denis Dutton's musical analogy, let's add one of my favorite experiences of the 1960's- Peter Schickele's PDQ Bach concerts. This is tongue-in-cheek forgery, perhaps satire, not attempting to be hidden or passed off as an original of another author's work, but as a work created by a fictional author. It is simultaneously an authentic original work and a forgery, but not in the sense of van Meegeren's Vermeers.

Réponse à Dan Sperber
Roger Pouivet
Sep 16, 2004 10:48 UT

Je voulais dire que le tableau a changé pour nous, même s'il n'a pas matériellement changé. Il me semble qu'entre le statut ontologique des propriétés de Cambridge et celui des propriétés intrinsèques (que les choses ont indépendamment de tout autre chose), on peut admettre un statut ontologique pour des propriétés extrinsèques (qui dépendent de l'existence d'autre chose que de ce à quoi on les attribue) dont l'appréhension, particulièrement dans le cas des oeuvres, joue un rôle esthétique décisif (voir ma contribution à venir au colloque). Elles ne sont pas intrinsèques, elles ne sont pas non plus causalement inertes. Pourquoi ? Parce que toutes les propriétés constitutives d'une oeuvre d'art ne sont pas manifestes. Comment ? Dan Sperber a raison de penser que c'est toute la question. Il n'est pas facile d'y répondre. Je dirais que certaines propriétés extrinsèques des oeuvres d'art ont une efficacité causale, du genre de celles que peuvent avoir certaines propriétés dispositionnelles (elles ne se réalisent qu'en certaines circonstances). Celui qui sait qu'un tableau n'est pas de Vermeer devient attentif, voire sensible, à certaines de ses caractéristiques auxquelles, auparavant, il était aveugle. L'appréhension de ces propriétés dispositionnelles (et à mon sens réelles) des oeuvres supposent des croyances correctes du spectateur, par exemple au sujet de son auteur, de son contexte artistique, historico-esthétique, etc.

Propriétés causalement inertes et fétichisme
Sébastien Réhault
Sep 16, 2004 11:03 UT

Si les propriétés causalement inertes jouent un rôle esthétique, c'est certainement en cas de changement d'attribution. Apprendre qu'une œuvre qu'on croyait de Turner est en réalité un faux (et non pas une copie qui tente de se faire passer pour un original) produit au 20ème

siècle, ôte à cette œuvre tout l'arrière-plan constitué par l'ensemble de l'œuvre de Turner et par le contexte de création qui lui est attaché. La contrefaçon perd certaines propriétés relationnelles qu'elle entretenait, en tant que Turner authentique, avec d'autres œuvres. Or ces propriétés relationnelles jouent un rôle dans l'appréciation esthétique de l'œuvre : une même œuvre X peut être belle en tant qu'œuvre de jeunesse de Turner (l'utilisation académique des couleurs pour peindre le paysage dans X contient déjà en germe l'audace des paysages ultérieurs) et moins belle en tant que peinture d'un faiseur talentueux du 20ème siècle (le paysage dans X est toujours aussi académique, mais il n'annonce aucune évolution esthétiquement intéressante). Cependant, il est clair (et la perplexité de Dan Sperber est ici légitime) que cet impact esthétique de propriétés causalement inertes ne devrait pas jouer quand l'observateur sait qu'il a en face de lui, non pas un faux, mais une copie de très bonne qualité, disons même parfaite, d'une œuvre existante. Dans ce cas, il me semble que l'on conserve l'authenticité nominale : il s'agit d'une copie officielle d'un authentique Turner, elle préserve toutes les propriétés phénoménales de l'original (du moins les propriétés visibles à l'œil nu, i.e. les conditions normales d'appréciation d'une œuvre picturale) et n'empêche en rien d'apprécier cette œuvre de Turner. L'œuvre est un type à plusieurs instances : au moins, dans le cas de l'œuvre picturale, dès que l'instance est une bonne reproduction de l'original. Prétendre que les propriétés causalement inertes devraient jouer un rôle esthétique même dans le cas de la reproduction officielle parfaite ouvre la voie au fétichisme. On en est réduit à postuler l'existence (pour le moins problématique) de « vibes » émanant de l'œuvre du seul fait d'avoir été en contact avec l'artiste lui-même. C'est au moins aussi obscur que « l'aura » postulée par Walter Benjamin. Question : qu'est-ce que ces vibrations changent pour l'appréciation et la connaissance de l'œuvre par rapport à une reproduction parfaite ? Si une œuvre d'art a pour fonction de nous mettre en « relation spirituelle » avec son auteur, pourquoi ne se pas se contenter de manipuler des objets (cravate, fourchette, pinceau, montre-gousset, etc.) ayant appartenus à l'artiste ?

Why do we care about an artwork's history?
Paul Bloom
Sep 16, 2004 14:56 UT

Dan Sperber, as usual, asks exactly the right question: Since an original and a perfect duplicate strike the senses in exactly the same way, why do we care about the difference? As he frames it, it's a psychological question, but one can, of course, also ask the normative question of whether our caring is reasonable. Is it "fetish worship" as Arthur Koestler maintained? Is it just plain snobbery?

Denis Dutton has provided an appealing answer to both questions in an earlier paper, when he suggested that we see – and evaluate -- every work of art as a performance, as the result of human agency. We admire an original more than a fake for the same reason that we give an A to a brilliant original essay and an F to a copy of that essay. From this perspective, our psychological processes are perfectly reasonable—certainly an act of creation is a more impressive performance than mere copying.

The point I want to add is that the importance of history is not special to art. It turns out that our appreciation of history is relevant to our enjoyment in every domain that one can think of. Arthur Danto points out, for instance, that part of sexual pleasure is surely "the belief that one is having it with the right partner or at least the right sort of partner." Same for food; part of the joy of eating is the supposition that one is eating certain things: "the food may turn to ashes in one's mouth the moment one discovers the belief to be false, say that it is pork if one is an Orthodox Jew, or beef, if one is a practicing Hindu, or human if one is like most of us (however good we might in fact taste)." Our pleasures are related to how we see the nature of things, and this includes their history, their origin. It matters where the meat has been – what it has touched, for

instance – prior to ending up on your plate. Similarly, there are facts that one could discover about the history of a potential sex partner that could radically change your attitude towards him or her.

This focus on history is especially striking in the domain of artifacts. There is by now a large body of evidence that even 2- and 3-year-old children are sensitive to the historical properties of an artifact, such as a tool, when deciding what it should be called. They ask: What was it used for? What was it intended to be? Where did it come from?

So one way to answer Dan's question is as follows: History matters when we evaluate an artwork because history matters when we evaluate everything. Of course, this just pushes the question back: Why do we take history so seriously? I would answer this, but I see that I have run out of space.

Do children care about history in art evaluation? A question to Paul Bloom
Gloria Origgi
Sep 23, 2004 15:14 UT

Paul Bloom writes *"The point I want to add is that the importance of history is not special to art. It turns out that our appreciation of history is relevant to our enjoyment in every domain that one can think of.*

He mentions experiments on children's categorization in which children are shown to be sensitive to the historical properties of an artefact in order to classify it.

These experiments suggest that children are sensitive to the "proper functions" of artefacts, that is, to those functions that are historically responsible of the fact that an artefact has been produced and re-produced.

But is this kind of historicity that is at stake when children aesthetically evaluate a work of art? At what age do children develop an interest about the authorship of a work of art? Are there any experiments that show what are the aesthetic criteria to appreciate a work of art in young children? For example, does complexity matter, that is, a drawing with four overlapping triangles is considered by a child aesthetically "superior" to one with just one triangle? Do you know any work that has specifically explored children's aesthetic criteria?

Children and history: A reply to Gloria Origgi
Paul Bloom
Sep 26, 2004 14:37 UT

"But is this kind of historicity that is at stake when children aesthetically evaluate a work of art? At what age do children develop an interest about the authorship of a work of art? Are there any experiments that show what are the aesthetic criteria to appreciate a work of art in young children? For example, does complexity matter, that is, a drawing with four overlapping triangles is considered by a child aesthetically "superior" to one with just one triangle? Do you know any work that has specifically explored children's aesthetic criteria?"

Nope. There is some work that has explored the developing appreciation of artistic style, but, as far as I know, all of this research is with quite older children and none of the results are that exciting. And I don't think there is any work at all asking the central question: Do children value an original more than a forgery?

One can also ask the parallel question outside the domain of art. For instance, will a 5-year-old sports fan care whether he gets an actual autograph from Lance Armstrong or Tony Hawk, as opposed to an indistinguishable fake?

This isn't difficult research to do. The experimental design is quite straightforward, and the predictions are pretty clear. And it's a neat line of inquiry--let me know if you'd like to collaborate.

Observe, Describe & Explain
Jose Luis Guijarro
Sep 15, 2004 18:41 UT

As I announced before in my response to “Should we care about nominal authenticity?” my idea about the FAKE evaluation of an object or event needs to be explained in an evolutionary frame.

(1) when I talk about ART I am pointing to something quite specific, a human attitude: an evaluative attitude. That is to say, I am not YET interested in the possible essential characteristics of objects or events (which doesn’t mean I won’t be, later on!), nor in the intentions of what we call “artists” or even in the cultural frame that conditions our “reception” of them. I call this attitude for want of better term “A-value”. And I represent it in this form:

[A-value {x}]

In which “x” stands for anything whatsoever. That is, there are no real limitations to what may be processed in an “A-value” nesting representation sort of way.

(2) Saying that ART is an attitude does not describe it, of course. This is the second requirement that I intend to fulfil. For want of better idea, I proposed that this A-value representation should be described in terms of resetting some mental devices or (parts of them) to adapt them to new situations. A lot of work needs to be done to specify what parts and how much resetting is needed to achieve the sort of elation we have when we solve problems in totally new ways. One possibility is to analyse the two mental pathways, the normal trajectory and the intensifying trajectory, that David Lewis-Williams (2002) proposes to explain the origin and functioning of cave art in Lascaux and Altamira. There are a lot of possibilities here that we cannot go into now.

(3) The explanatory requirement, the analysis of the human potential to meta-represent, and the universal (i.e., not cultural or historical) function of this resetting ability, are, in my view, the only possible jumping boards from which to dive into the problem of FAKES. Maybe, in its origin, the evaluation of something as a fake had to do with the insufficient resetting potential of the information processed. All this is still very tentative, of course, but I think it really opens up new paths of exploration and research. Or does it?

Some remarks on the "fetishism" issue
Jerrold Levinson
Sep 16, 2004 19:18 UT

One thing that has emerged from the discussion so far is that, whether we have a good rationale for it or not, most of us seem to feel that there is some aesthetic/artistic virtue in an original painting that is not possessed by even a perfect, that is, perceptually indiscernible, copy or duplicate of it. Why? By hypothesis they look the same, and thus at least seem to afford exactly the same possibilities of aesthetic delectation. Now one at least aesthetically- related reason--not quite the same as an aesthetic reason, perhaps--to privilege the original is epistemic: can we be sure that the copy has preserved all the features of the original's appearance that might make a difference aesthetically?

Answer: probably not, so let's not lose track of which is which. But apart from that, there is the more important idea that interacting with the original "puts us in touch" with the artist in the way the duplicate cannot, because of the different causal/historical properties of the two, those non-observable, extrinsic, relational properties to which Sperber, Pouivet and Bloom allude in different ways. The trouble with that appeal, though, right-minded as it is, is that it seems to be open to the charge of acknowledging in artistic originals only "fetish" value, such as any object with which an artist came in contact, e.g. his paintbrush, favored chair, or reading glasses, might be said to possess. But this is where a crucial difference emerges: the artist's painting, unlike the artist's paintbrush, is not just something with which the artist was in contact, but the real embodiment and repository of his or her creative activity, the actual site of his or her artistic accomplishment. *This* is what we want to be in touch with, not just the historical person of the artist, and this is what does not reside in the copy, however perfect. In other words, the perfect copy might serve to provide the same *aesthetic* experience as the original, in the fairly usual narrow sense of 'aesthetic', but it cannot honestly provide the same experience of *artistic appreciation*, which goes beyond a concern for the perceptual appearances that an artwork affords, and interests itself at least as much in what the artist has done in or through the artwork. (My concern with the "fetishism" worry and ways of countering it goes back to a short essay of mine called "Zemach on Paintings", Brit. J. Aes. 1987, which some may wish to consult.)

The return of the copy
Sébastien Réhault
Sep 16, 2004 21:56 UT

It seems Jerrold Levinson rules out the possibility for an excellent copy to be an objet of proper appreciation of “the” work it copies too quickly. Here is the epistemic argument of Levinson : even if the differences between the original and the copy are so subtle that we can’t normally perceive them, we can never be sure that “the copy has preserved all the features of the original's appearance that might make a difference aesthetically”. But if the original and the copy are so perceptually indiscernible, what kind of microscopic physical difference can have a causal effect such that it makes a real aesthetic difference ? If after careful examination we can see no visible difference, then the copy preserve the appearance of the original and admit the supervenience of the same aesthetic properties. If the copy is excellent, it is the embodiment of the artist’s creativity as well as the original work, even if he or she didn’t produce the copy by his or her own hands. That “we want to be in touch with” the original is not in question. But it seems unlikely that we have a rationale for it.

Objects and actions
John Zeimbekis
Sep 16, 2004 23:05 UT

“[...] the artist's painting, unlike the artist's paintbrush, is not just something with which the artist was in contact, but the real embodiment and repository of his or her creative activity, the actual site of his or her artistic accomplishment. *This* is what we want to be in touch with, not just the historical person of the artist, and this is what does not reside in the copy, however perfect. [...] the perfect copy might serve to provide the same *aesthetic* experience as the original, in the fairly usual narrow sense of 'aesthetic', but it cannot honestly provide the same experience of *artistic appreciation*, which goes beyond a concern for the perceptual appearances that an artwork affords, and interests itself at least as much in what the artist has done in or through the artwork.”

Here Jerrold Levinson shifts the definition of authenticity from numerical identity of an object to the individuation of an action. But I don’t see why focusing on the action can help explain the

interest taken in the original object. Showing the fake involves pointing to traits perceptually indiscernible from those of the original and saying, “Here is what the artist did” or, to express the individuation of the artist’s action, “Here is how the artist took up his/her position with respect to practice in that genre at the time of production”. Since we’re assuming the fake is perceptually indiscernible, the fake exemplifies all the properties of the object by means of which the artist accomplished his/her action. So the object of my appreciation (action included) is type-identical to the artist’s action. This even applies when I know that I’m looking at a fake: the object of my appreciation is the same in all relevant senses, and there is expressive authenticity without nominal authenticity.

The same points apply to Dutton’s claim for a dependence of expressive authenticity on nominal authenticity (eg when he writes “however pleasant and skilful a modern forgery of a sixteenth-century master drawing may be, it can never be a sixteenth-century achievement, and therefore can never be admired in quite the same way”).

reponse brieve
Jerrold Levinson
Sep 17, 2004 4:25 UT

Il me semble que Sebastien Rehault, quand il dit que, dans le cas ou elle est excellente "a copy is the embodiment of the artist's creativity as well as the original work", il confonde une *incarnation* de qqch et une *representation* de qqch. Une copie, aussi parfaite qu'elle soit, n'incarne précisement pas l'action creative historique de l'artiste, quoiqu'elle puisse bien sûr la représenter, la signaler, nous informer là-dessus, etcetera.

Answer to Jerrold Levinson
Roger Pouivet
Sep 17, 2004 6:22 UT

For theology, incarnation is a mystery, for it is impossible to explain how Jesus-Christ can both be God and have a human nature. It is very strange to see this notion, used with a lot of precautions by theologians, to appear in aesthetics. Another concept, close to this one, is *presentification* : x presentifies y iff x is not the representation of y, but y itself in presence. This notion has been used by Lucien Stéphan to characterize some african masks and sculptures : they do not represent a spirit, but are the spirit himself or herself in presence. This is exactly what was called "fetishism". Jerrold Levinson says: "the artist's painting, unlike the artist's paintbrush, is not just something with which the artist was in contact, but the real embodiment and repository of his or her activity, the actual site of his or her artistic accomplishment". He proposes a very religious and magical conception of artistic creation. I am respectful of animist religions (even if I think there are false). I am completely convinced that Christians have good reasons to accept Mysteries : Creation, Incarnation, Redemption, Faith, ... But some aestheticians are not animists, and they have no reason to accept aesthicical mysteries.

Reply to Jerrold Levinson and Roger Pouivet
Sébastien Réhault
Sep 17, 2004 15:50 UT

I completely agree with what Roger Pouivet said. The concept of embodiment or incarnation is a very problematic one if used literally in aesthetics. If it is used literally, then it refers either to a serious matter of faith or to a magical power and, as Roger Pouivet noticed, belief in that kind of

power is normally identified with fetishism. Once again you can say that you feel the embodied artist's activity (and not only the presence of the artist) by some special vibes that only the original work can produce. The conclusion must be that only a few persons can feel that kind of implausible experience and that it can't stand as a serious argument. But maybe the concept of embodiment or incarnation can be used in a weaker sense. This sense is maybe what Jerrold Levinson sometimes means. The artwork qua material object shows physical signs of the artist's intentional activity. Then if we say that the work embodies this activity we mean : the intentional activity is not present at the moment but the material object refers to it. But if we take this for granted, then once again the perfect copy can have the same function as the original work. The perfect copy or reproduction qua material object refers no less than the original work to the original artist's intentional activity and, in the weaker sense, it embodies it. As the concept of embodiment was used here to make a difference between original works and copies, now it's seems clear that we can leave it to theology and magical practices without regrets.

c'est vrai, actions speak louder...
Jerrold Levinson
Sep 17, 2004 19:39 UT

John Zeimbekis is right to put the accent on the artist's actions in deciding what's important about originals versus perfect fakes, but he is wrong to think that it is the type, rather than the token, action that matters, or that only the type action matters. (Of course it is not a matter of "ineffable spirits" lurking in an object--I am *not* making artworks out to be fetishes or totems. Nor is it a matter of "subtle differences" that only the eyes of connoisseurs can descry.) The historical token action of a creative painter is effected through the particular material artifact he produces; that artifact is thus the site and body of his expressive achievement, his technical invention, his visual discovery, or whatever, whereas other artifacts are not, though they may represent and refer to those token achievements/inventions/discoveries. Of course one *can* admire an achievement via representations of it, but is it not better to admire it directly, "in the flesh"? The original painting is *where* the achievement occurred; the historical, dated achievement is *embodied* in it, and not in its reproductions; this matters to us, and it does not seem irrational that it does. Consider a parallel universe, in which there is a perfect simulacrum of the Gettysburg Battlefield. Would you just as soon go there--travel costs aside--to experience the solemnity and gravity of the Civil War and ponder the significance of the events that unfolded there, rather than to the real Gettysburg Battlefield? And if Disney made a perfect replica of Paris in the middle of Iowa, would you feel you had experienced Paris, the historical Paris, if you visited it? Basta.

The Null Hypothesis
Richard Minsky
Sep 17, 2004 20:54 UT

I am disappointed with Sébastien Réhault's last comment.

"The conclusion must be that only a few persons can feel that kind of implausible experience and that it can't stand as a serious argument."

That makes no sense. If a few persons (or one) can feel something then it is not implausible. It then must be taken into account or the hypothesis is void. The null hypothesis is the foundation of any reasonable investigation.

Jerrold Levinson's rhetoric of embodiment
Roger Pouivet
Sep 17, 2004 21:29 UT

I agree with Jerrold Levinson: authenticity and history are closely related. A fake does not have the history the forger pretends it have. I also agree that it is not irrational to consider that there is a *real* distinction between the original and the perfect copy: they do not have the same history. But I do not see the point to say : "The original painting is *where* the achievement occured; the historical, dated achievement is *embodied* in it, and not in its reproduction". What is added by this rhetoric of embodiment, except the sense of something mysterious (the notion of achievement is not clear at all, not clearer than the notion of spirit) we experience in original paintings and that reproductions are supposed not to have? Their histories are not the same; is it not quite sufficient to indicate a strong difference, even it is not always a phenomenal difference?

Reply to Richard Minsky
Sébastien Réhault
Sep 17, 2004 22:03 UT

All right, maybe I was too quick. I only wanted to say that I never heard about people able to distinguish between an original work and a copy with a kind of “vibes sensus”. The only thing I heard is that when the copy is not good enough a connoisseur can perceive the difference thanks to his experience and knowledge of the artist. But that’s nothing mysterious.

Do we ever appreciate the token action?
John Zeimbekis
Sep 18, 2004 0:24 UT

I’m glad Levinson accepts the point about actions, but I think it commits him to more than he admits.

Levinson’s use of the twin battlefield example (“Would you just as soon go there [...] to experience the solemnity and gravity of the Civil War and ponder the significance of the events that unfolded there, rather than to the real Gettysburg Battlefield?”) implies that we have to know that x is a fake for x not to have the same value as the original. We can put the general case as follows:

If the subject does not know that x is a fake, and x is perceptually indiscernible from the original, and the subject has enough background knowledge to reconstruct the actions putatively carried out by means of x, then, the subject has the same appreciation of x as he/she would have of the original.

Suppose we define the value of the work as a disposition to cause appreciation under the right initiating causes, which in this case are the subject’s background knowledge. Then, a perfect fake *can* have the same artistic or aesthetic value as the original. (“Can”, because the condition of misleading the subject also has to be met.)

This is enough to show that expressive authenticity does not reside in the action *token*. The action token includes a token set of rationalizing mental states in the artist, token bodily movements of the artist, and the physical token of the original work. In the conditions described above, the subject appreciates none of these, yet has the same appreciation he/she would have before the original.

This can help us understand what lies behind the talk about “incarnation”: when we appreciate the original, we cannot, in any case, be appreciating the artist’s token mental states and token bodily movements. “Incarnation” just designates the relation of rationalizing descriptions to basic actions; in this case, those that determine the forms in the painting. Of course, this is something like a ghost or spirit, and it's moving to read the author's reasoning into the work. But that it is moving is not a matter of nominal authenticity.

Historically ideological or biologically universal conditions?
Jose Luis Guijarro
Sep 18, 2004 12:30 UT

It seems quite clear that the evaluation of something as original or a fake relies heavily in historical elements of our interpretative context. It should be noted that in many cultural environments, say, Medieval Europe, Spanish romancero tradition which is still alive, African “oraliture”, and so on, the craze about the importance of the CREATOR as the crucial feature of the evaluation of a work of art did not exist at all. In fact there are thousands of “artistic” texts, sculptures, paintings, etc. which are anonymous because the creator did not feel that it was important for posterity to know that (s)he existed at all –or, anyway, the social group didn’t!

The important thing in Spanish oral romancero tradition, or in the tales told under a big tree in some African villages, is THE WAY it is told, not in knowing who the author was. Some of this attitude can be seen nowadays in children when they listen to a well known tale; they are interested in listening to it THE RIGHT WAY, not schematised by the parent who wants to put them to bed. Therefore, what Paul Bloom says about how we are NOW interested in authorship is, of course, true, but it is a historical requirement, by no means a universal trend in humans.

Should cognitivists not try to find, FIRST, more general (and even UNIVERSAL!) human causes of evaluation than discuss such heavily culturally loaded ideas as this one of real authorship?

Levinson is right due to the role of epistemic perception in aesthetic perception
Nicolas Bullot
Sep 19, 2004 20:45 UT

According to my reading the discussion, Jerrold Levinson is justified in emphasizing characteristics such as (1) historicity, (2) embodiment, and (3) token primacy to resolve Dan Sperber’s puzzle.

One of the reasons that may render difficult to understand why Levinson is right is based on the failure to distinguish: (A) sense or simple perception [i.e., basic feature and object perception such as shape, size, texture or color perception] and (B) epistemic perception [perception of hierarchical facts about the perceived object] in Dretske’s sense (Dretske, 1969, 1995).

When conflating an original and a copy, the conflation may be based primarily, thought not exclusively, on similarities in the domain of *simple* perception of the artwork. In Sperber’s thought experiment: ‘the copy X is indiscernible from the original Y’ means ‘the copy X is indiscernible from the original Y *in sense perception*’. This is entailed by the clause of stimulus equivalence; for the concept of simple/sense perception refers to the basic and mandatory processes of the stimulus analysis.

This said, we all agree that aesthetic perception is not reducible to simple perception. Aesthetic perception integrates epistemic perception, i. e. perceiving (and reasoning about) *facts* about

the artwork. But what are the facts that one seeks to perceive in most cases of artwork perception? One namely wants to perceive *that* it has been produced by such and such persons and intentions, or in such and such conditions or contexts etc. All the latter are historical entities (historical objects, events, intentions and situations). Now, being historical, these entities generally fulfill a rule of uniqueness. Each is unique, and facts about them cannot be known without caring about such a uniqueness (and thus embodiment, which is required to get access to uniqueness in most externalist views). That may be enough to explain the crucial interest of caring about nominal authenticity. No need for magic events or vibrations. This analysis can rely on a plain physicalist and historical view of object perception and cognition.

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more cases, embodiment, and fetishism
Jim Hamilton
Oct 8, 2004 22:08 UT

The questions posed about fetishism have concerned (1) whether we should prefer the original work of art (belonging to an artform that allows for originals) over variously conceived non-originals with which originals may be paired and (2), if so, whether the grounds for our preference can be something other that having a fetish for originals. The fetishism discussion has focused on cases inspired by ontological reflection, cases in which we imagine pairing an "exact duplicate" with an original by a particular artist. *Ex hypothesi* the non-original appears to the eye to be the same work while, at the same time, it need not be the result of an intention and need not embody an achievement. (It can be imagined as a result of the 'Danto-machine.') Cases inspired by considerations about the practices of art pair an original by an artist, A, with (a) a fake of that original or (b) a forgery in the style of A. Another pairing within this range includes (c) the original by an artist paired with a (failed or successful) copy of the original made by one of the legion of painting students at museums who copy entire paintings or even just details. None of these cases seem to have been at issue in the fetishism thread. Surely we will struggle to find grounds for preferring originals to non-originals when they are conceived as indiscernible duplicates. Levinson's proposal in this connection – that originals are "the real embodiment and repository of [the artist's] creative activity, the actual site of his or her artistic accomplishment" – has seemed to collapse into the very fetishism he seeks to avoid because Levinson is taken to introduce a theological rather than a naturalistic sense of "embodiment." I think he meant something akin to Goodman's notion of "exemplification": reference to plus possession of a feature. Both to illustrate what I think Levinson had in mind but also to press his proposal from another direction, I suggest we consider the grounds for preferences of originals in two other pairings. In case (d) we pair the original by an artist, A, with a work from the same period and location, and in the same style, that had been attributed to A but has recently been reclassified as belonging to "the school of A." In addition, however, the object now reclassified as by some unknown artist working in A's workshop is actually more successful than any of the originals by A – more successful at the very same achievement for which we value A's work *in which the achievement was first accomplished.* In case (e) we pair the faded original and a successful copy of the original, made before it faded. In this case the original *now* possesses the achievement less than does the successful copy. Eventually the achievement may not be

discernible in many originals. These cases re-focus our attention on cases arising out of reflection on the practices of art. I think they also point to the stakes in the fetishism issue.

The Eyes Have It
Richard Minsky
Sep 17, 2004 12:43 UT

As Sébastien Réhault notes, a copy can be “the embodiment of the artist’s creativity.” I am looking right now at copy No. 32 of an artwork, signed by the artist. We regard an unsigned copy as less authentic. Was it not signed because the artist didn’t approve of it? Is it exactly the same as the signed copy, but the artist was paid to sign the other copy? Was it a reject?

Perhaps we believe that only the eye of the artist can discern the difference between one copy and the next. All eyes are not created equal.

Jerrold Levinson and Roger Pouivet are dancing with the notion of embodied spirits in fetish objects. That is exactly what I was referring to earlier. Just because “some aestheticians are not animists” doesn’t mean that those who see or feel something else can’t tell the fake from the real thing, no matter how visually exact the copyist appears to be. A lot depends on who is looking at it. On a more mundane level, look at all the “experts” who have different opinions about the authenticity of this or that object. To some it’s real, to others it’s fake, and I have seen some really obstinate attachment to various opinions. Whose eye is better? Are the opinions based on hunches or intuition? That’s all “fetish” stuff.

I’ve known some exceptional “eyes” in the art world, among curators, collectors, writers and dealers as well as artists. These are people who talk about artwork having “guts,” and being “the real thing.” They talk about artists who are “fakes,” not just artworks. They talk about artists who were doing the real thing at one time, and were fakes of themselves at other times, copying their own work. Not exact copies, but making new works that were dead. Is that stuff authentic or fake?

What they are seeing is a spiritual quality. It is very much related to the Congo Fetish and other objects that are embodying spirits, as distinct from representing them. Yes, art and religion have many similarities. And both have their bibles, temples, shamans, hucksters, priests and congregations.

Evaluation, Endorphins and Economics
Richard Minsky
Sep 19, 2004 12:46 UT

Jose Luis Guijarro brings up a fundamentally interesting point. There are a few notions and questions that come to mind.

“The important thing … is THE WAY it is told, not in knowing who the author was.”

This is another issue of authenticity, and also goes toward Sébastien Réhault’s comment that a copy can be “the embodiment of the artist’s creativity.” Authenticity in performance art is evaluated with a different set of parameters. The oral tradition exists in all cultures, including our own “historically ideological” ones, from folk tales of Alabama to Homer.

Attribution of authorship is not a necessary condition of authenticity. In order to be perceived as authentic, the audience requires a particular telling of the story, so that it communicates the metaphor that the culture intends the story to preserve or transmit to the next generation.

Whether Homer was an individual author, was the actual creator of the works attributed to him, or was a raconteur who embodied centuries of story line development, is irrelevant. The Homeric epics

predate their written form. They are repositories of cultural values. Did they lose their authenticity when they were written down?

“children … are interested in listening to it THE RIGHT WAY”

I seem to recall that this may be a “biologically universal condition.” About 30 years ago I read that endorphin production is stimulated by things that are familiar, with greater familiarity causing greater endorphin production. Is that still thought to be true? If so, it goes toward explaining why children want to hear stories exactly the same way—otherwise they are not getting their “fix.”

Discussing the cognitive aspects of “heavily culturally loaded ideas” is a reasonable activity. One of the outcomes of this discussion may be a better understanding of how these ideas developed, whether they have value to the culture, and perhaps whether that development is a significant contribution to cultural evolution. Our culture developed patents and copyright laws to foster creative developments among authors and inventors. The economic impact of attribution of authorship affects cognitive processes, as well as the biological, aesthetic, historic, and other factors.

My understanding problem
Jose Luis Guijarro
Sep 19, 2004 16:28 UT

Sometimes one reads or listens to impeccably phrased expressions which make us believe that we have understood them completely and that we cannot but agree with them. This happened to me with the following sentence of Richard Minsky:

“Authenticity in performance art is evaluated with a different set of parameters”

But after thinking about it, I became worried, because I don’t really understand the idea behind it. Granted, it may be due to my scant knowledge of the English language (which is patent all the time, I am afraid), but it may also depend on not well defined concepts that hang from certain terms.

For instance, nobody has as yet pointed to the likely object or relation that is meant by the term “authenticity”. Authorship has been proposed but strongly opposed to, as a kind of fetishm or as a historically loaded requisite. Expressivity remains a likely possible object; but what is expressivity? Instead of one mysterious object we have now two, and relating them to one another does not solve my problem of knowing what we are discussing about.

Secondly, Minsky affirms that there is a performance art which, apparently, is (radically? –In what way?) distinct from “another” type of art. It is true that in our current discussion, performance art has been identified with music, dancing, theatre and so on; but I can’t see the real distinction with the so-called “other” kind of art, as painting, sculpture, cooking, etc. Maybe the evaluation of an activity is clearly all that different –cognitively speaking, that is– from the evaluation of the result of that activity. If that were so, I would like to know how and why.

Finally, I have no idea what parameters are considered to be in operation when evaluating art or whatever. This has been my point in trying to go beyond historical ideologies in cognitive analysis: to speculate about the likely primitive biological evaluating parameters. Only after we have agreed on these general or universal human mental operations, can we, then, search how they became culturally loaded and why the work the way the do in that universal frame.

For instance, as far as I know (and I certainly know very little about Psychology), the “Fear of The Other” seems to be a general trend in human psychological life. But this fear has different actualisations in History: in ancient times, say for the Romans, it was the Carthaginian power,

and in modern times, say for the Allies, it was nazism, for cold war Americans, the communists, and for our present strongly americanised Western World, terrorism. My claim is that unless we manage to describe and explain the global mental disposition that we name “Fear of the Other”, all other “reasonable activities” (Misnky dixit) will not attain a high degree of explanatory power.

The parameters of authenticity
Richard Minsky
Sep 20, 2004 10:13 UT

Jose Luis Guijarro: “Minsky affirms that there is a performance art which, apparently, is (radically? –In what way?) distinct from “another” type of art.”

I thought Denis Dutton had made that distinction clear, but to elaborate:

A physical artifact that communicates a metaphor through material and image can be observed at any time. It is a finished, stable item, not itself a process, though it is the result of a process and may stimulate a process in the viewer. Perhaps it is a catalyst. Generally each viewer has a personal experience of the object independent of other viewers’ perceptions.

Sometimes the object becomes a prop for a performance, such as tour groups with a scholar or docent, or an art critic using it as an example to expound on some issue.

Performance is different because it exists only in its moment. Whether it is a string quartet played in private with only the musicians, a rock concert in a stadium, or a bedtime story told by a parent, it is different every time, and involves an interaction between the players and, if present, an audience. One person can be both the performer and audience. People talk about the “magic” that was in the room. What are they talking about?

Has anybody set up equipment to measure brain waves during performances? Do the electromagnetic patterns synchronize among the players and/or audience? Does an additive brain wave generate in the theater (or bedroom) that produces frequencies/amplitudes not otherwise possible? Sound waves do this—undertones and overtones (harmonics) are generated that are essential to the musical experience.

An object has parameters like material, image and metaphor. Those may have units like centimeters and grams, or descriptions like “cobalt blue,” “granite,” “portrait of a woman” or “radiant morning light.”

A performance has parameters with units, like duration, tempo, volume, or pitch, or descriptive parameters like “danceability.” For a few years I played in a Tango quintet, where that was a very important parameter.

How are these parameters used to describe “authenticity”?

Jose Luis Guijarro: “nobody has as yet pointed to the likely object or relation that is meant by the term “authenticity”.”

Denis Dutton pointed to its wide range of meanings in his first sentence, and narrowed it to the division between its nominal and expressive forms.

The parameters apply to both aspects. It can be very simple. Investigating its nominal authenticity, we discover a Rembrandt etching is not authentic because the width is wrong. Or perhaps the watermark in the paper is from the wrong mill, or the composition of the ink is wrong. Physical parameters that have units are often cited when establishing the attribution of an object because they are believed to be indisputable.

At a Tango party, the music was not authentic. The tempo of Felicia was wrong, the dynamics were insensitive, and there was no expression of the balance between joy and pathos. Whose idea was it to hire the harpsichordist?

La distinction Art/performance et art/objet n'est-elle pas historiquement contingente ?
Sylvie Allouche
Sep 30, 2004 17:21 UT

1) Denis Dutton dit en (2.4) que l'authenticité en musique diffère de l'authenticité en peinture car la rencontre directe avec l'oeuvre historique originale qui est possible en peinture ne l'est pas en musique."The original work is specified by a score, (…) correct performances may differ markedly ».

Certes mais est-ce que cette différence n'est pas en partie contingente, du fait de sa soumission aux conditions techniques de conservation des œuvres ? Ainsi, grâce aux nouvelles technologies, il pourrait y avoir un marché de l'authentique "bande-son." a) L'authentique enregistrement sur CD. b) LA bande-son original de tel ou tel concert, aussi abîmée soit elle. Je pense en particulier aux enregistrements de morceaux de jazz ou aux enregistrement pirates de concerts. Ce phénomène pourrait se généraliser.

2) Denis Dutton continue son argumentation en généralisant sa réflexion aux arts de performance:"The very idea of a performance art permits performers a degree of interpretive freedom consistent with conventions that govern what counts as properly following the score ».

Mais il me semble que cela est essentiellement dû à des raisons historiques. Maintenant qu'on a des méthodes d'enregistrement audiovisuel, l'attachement de la notion de performance à la musique ou au théâtre est essentiellement traditionnel et l'attachement à une authenticité plus étroite de l'œuvre pourrait surgir plus tard, comme elle a surgi pour les œuvres d'art plastiques (cf. telle pièce jouée par la troupe du théâtre du soleil et pas une autre). Il y avait autrefois cette même liberté interprétative qui s'attachait aux œuvres d'art plastique et littéraires.

3) Enfin, Denis Dutton note que ", the twentieth-century witnessed the development of an active movement to try to understand better the original sounds especially of seventeenth-and eighteenth-century European music. ".

On peut retrouver le même genre de phénomène ailleurs. Ainsi, à Roskilde au Danemark, un projet de reconstruction de bateaux vikings avec les techniques de l'époque est en cours de réalisation. La démarche est censée être scientifique, mais elle pourrait aussi être considérée comme artistique et il y a sans doute des exemples dans les arts plastiques que je ne connais pas. Y aurait-il par exemple des sculpteurs qui préféreraient par souci d'authenticité utiliser les mêmes outils que les Anciens, plutôt que les outils électriques d'aujourd'hui? Si le résultat final est le même, on pourrait considérer que ça n'apporte rien d'utiliser des techniques beaucoup moins efficaces et beaucoup plus pénibles (pour la sculpture, en tous cas). (cf. suite de la discussion). En outre, je crois me souvenir que le mouvement qui consiste à rejouer de la musique ancienne est lui aussi daté (du romantisme ?) Et le théâtre ?

Ce que je veux suggérer comme hypothèse est la chose suivante : la distinction entre art de performance et art-objet est en partie historiquement contingente, ce qui ne permet pas de distinguer

des types d'authenticité par type d'art, si ce n'est à un moment donné de l'histoire de l'art, en fonction des techniques, de la tradition et des valeurs du moment.

Authenticité nominale de l'oeuvre & authenticité de l'expérience esthétique
Sylvie Allouche
Sep 30, 2004 19:11 UT

Denis Dutton défend l'idée que l'intérêt pour l'authenticté d'une œuvre ne doit pas être réduit à des questions purement mercantiles et qu'il y doit y avoir des raisons valables pour s'attacher à l'authenticité nominale d'un objet. *« The nominal authenticity of a purported Rembrandt or a supposedly old Easter Island carving may be keenly defended by its owners (collectors, museum directors), but the vast majority of articles and books that investigate the provenance of art works are written by people with no personal stake in the genuineness of individual objects."*

Certes, mais il faut prendre garde à ne pas réduire l'intérêt d'une œuvre d'art à sa contextualisation. Il faut distinguer les gens qui ont un intérêt explicite pour la dimension historique d'une œuvre et ceux qui sont censés être mus par un intérêt esthétique. Les premiers (museum directors, historians, etc.) ont un intérêt latéral par rapport à l'œuvre. Leur travail n'a de sens qu'à partir du moment où l'évaluation positive (ce peintre est un authentique artiste) a été produite. Et pour des raisons qui sont externes à l'art en tant que tel, ils fournissent un travail classique d'authentification, mais ce travail, ils pourraient le fournir pour n'importe quelle œuvre, et peut-être qu'une mauvaise œuvre ne témoigne pas moins bien de son temps (ou mieux) qu'une œuvre d'art considérée comme un chef d'oeuvre. D'un point de vue strictement historique, en effet, la mauvaise pourra témoigner par exemple des clichés du temps où elle a été produite.

En revanche, en ce qui concerne les gens qui ne sont pas impliqués dans une démarche historique, on pourrait les suspecter de ne pas accéder à une authentique expérience esthétique ou seulement à une expérience esthétique faible et de trouver dans l'authenticité nominale un substitut à leur manque d'authenticité personnelle en matière d'art. Car il a bien fallu à un moment que quelqu'un ait une expérience esthétique authentique de l'oeuvre de Rembrandt pour que par transposition cette valeur soit conférée à toutes ses oeuvres, y compris les plus mauvaises. (ce n'est pas tout à fait l'accusation de snobisme)

Il est vrai cependant que l'expérience esthétique globale (mais peut-être pas purement authentique) peut être augmentée par le SAVOIR que c'est une œuvre de Rembrandt. On peut alors considérer qu'il importe peu de savoir quelle est la source de notre expérience esthétique, peu importe qu'elle soit impure, du moment qu'on la vit. (je développerai cette idée dans un autre message).

Le savoir au sujet d'une œuvre créerait ainsi des prédispositions au moment d'entrer en contact avec celle-ci. Et toute l' « aura » (dans un sens courant, non benjaminien) suscitée par la question de savoir si une œuvre est authentique ou non joue en sa faveur. Le savoir de l'authenticité d'une œuvre jouerait alors le même rôle introductif que n'importe quel discours sur l'œuvre, tout en ayant une force particulière du fait que s'y ajoute la dimension qualitative de la « présence », qui est pour moi une forme de fétichisme, mais que je trouve légitime dans la notion d'expérience esthétique cristallisée que je proposerai plus tard.

Réponse à Sylvie Allouche
Sébastien Réhault

Oct 1, 2004 10:22 UT

Contrairement à ce que semble dire Sylvie Allouche, le rôle de l'authenticité nominale n'est pas de nous dire en quoi une oeuvre d'art est le "témoin de son temps", mais de nous indiquer les propriétés relationnelles pertinentes qu'une oeuvre entretient avec d'autres oeuvres et avec tout un contexte de production du fait d'être historiquement l'oeuvre qu'elle est. Sans la connaissance de ses propriétés, notre appréciation esthétique de l'oeuvre ne saurait prétendre à l'objectivité et à la correction.

On pourrait objecter que l'on ne devrait pas parler d'appréciation esthétique correcte, comme si les oeuvres d'art pouvaient être évaluées de façon ordinaire : ce qui importe, c'est d'avoir "l'expérience esthétique authentique" d'une oeuvre. Mais on est en droit de se demander en quoi consiste une telle expérience : ne s'agit-il pas là de l'idée devenue courante dans l'esthétique continentale, selon laquelle l'oeuvre d'art livre une vérité supérieure et indicible à celui qui sait en avoir une expérience authentique, indépendamment de toutes les connaissances "triviales" des historiens et critiques d'art ? Question simple : comment sait-on qu'on vit une "expérience esthétique authentique" d'un Rembrandt et comment la distinguer d'une expérience esthétique "inauthentique" ? Autrement dit : quels sont les critères de "cette" authenticité ?

Enfin, que faut-il comprendre par "augmentation de l'expérience esthétique globale" ? S'il s'agit d'indiquer qu'on est davantage ému à l'idée qu'on a devant soi un vrai Rembrandt, le problème ne progresse pas d'un pouce. La question demeure toujours de savoir si cette émotion est esthétiquement appropriée ou si elle n'est qu'une preuve de notre tendance irrationnelle à fétichiser, sans aucun rapport avec la signification de l'oeuvre. Aucun argument n'a pour l'instant montré que la première option était la bonne, sauf à verser avec une étonnante assurance dans la "réthorique de l'incarnation", pour reprendre la formule de Roger Pouivet.

Réponse (1ère partie) à Sébastien Réhault
Sylvie Allouche
Oct 3, 2004 12:23 UT

" *Contrairement à ce que semble dire Sylvie Allouche, le rôle de l'authenticité nominale n'est pas de nous dire en quoi une oeuvre d'art est le "témoin de son temps", mais de nous indiquer les propriétés relationnelles pertinentes qu'une oeuvre entretient avec d'autres oeuvres et avec tout un contexte de production (…). Sans la connaissance de ses propriétés, notre appréciation esthétique de l'oeuvre ne saurait prétendre à l'objectivité et à la correction*."

Oui, c'est vrai que ma formulation un peu rapide prête à confusion et il va de soi que la fixation de l'authenticité nominale d'une oeuvre d'art n'a pas pour but essentiel de témoigner de son temps. Cependant, le fond de mon argument demeure, dans la mesure où un travail d'authenfication nominale est susceptible d'être conduit sur n'importe quelle chose, qu'elle soit naturelle ou artificielle, matérielle ou idéelle, historique ou actuelle et aussi anodine soit-elle. Et de fait, en histoire, et plus particulièrement en archéologie, c'est bien comme cela qu'on procède, l'authentification nominale d'un ossement permettant elle aussi de «nous indiquer les propriétés relationnelles pertinentes» que cet ossement entretient avec d'autres ossements et données historiques, tant d'ailleurs du point de vue synchronique que diachronique.

Or certaines données (un ossement, un texte écrit en linéaire B, un tesson de poterie) ne sont nominalement authentifiées et étudiées que dans le but de construire la meilleure représentation possible de leur époque, c'est un but épistémique ; mais d'autres, appelées «œuvres d'art» ne jouent plus seulement le rôle de moyen de l'entreprise historique, mais

deviennent elles-mêmes une fin possible de celle-ci. Ainsi l'effort d'authentification des œuvres de Vermeer n'a pas pour but principal de recueillir des données sur le mode de vie des femmes de la bourgeoisie dans la Hollande du XVIIe siècle (même si cela en est un usage possible), mais celui de mieux «apprécier esthétiquement» chacune des œuvres attribuées au peintre. Et de fait, considérer «La Jeune fille au virginal» comme un authentique Vermeer change non seulement l'appréciation esthétique de «La Femme au luth» mais aussi celle de l'ensemble de son œuvre, et de l'ensemble de la peinture hollandaise du XVIIe siècle, etc. Certes.

Mais pourquoi chercher à avoir une «appréciation esthétique objective et correcte» de Vermeer, et pas de Van Meegeren, son célèbre faussaire ? Et d'ailleurs, pourquoi personne, avant que Thoré-Burger ne «redécouvre» Vermeer, ne se souciait de le faire? Quant le critique tombe en 1842 sur "La vue de Delft" et note «Voilà quelqu'un que nous ne connaissions pas et qui pourtant mérite fort d'être connu!» (Réf. 1), ne vient-il pas d'éprouver quelque chose comme une «expérience esthétique authentique»* ?

Et pourquoi, un beau jour, les objets cultuels africains, jusque-là uniquement considérés pour leur intérêt ethnologique changent de statut et deviennent des œuvres d'art dignes d'une appréciation esthétique autonome ? (cf. Réf. 2 pour une discussion intéressante de cette question). Les «propriétés relationnelles pertinentes» de ces objets avec d'autres objets et «tout un contexte de production» ont-elles brutalement changé ? Non, mais il me semble bien en revanche (cf. Réf. 3) que Matisse et Vlaminck aient vécu quelque chose comme une «expérience esthétique authentique» (ou «pure» dans le nouveau vocabulaire que je propose dans les réponses suivantes) expérience qu'ils ont su, tels le rhapsode décrit par Platon dans *Ion*, transmettre à d'autres, le processus aboutissant de fil en aiguille à conférer aux objets cultuels africains le statut d'«œuvres d'art».

Réponse (2e partie) à Sébastien Réhault
Sylvie Allouche
Oct 3, 2004 13:00 UT

Sébastien Réhault demande en quoi consiste ce que j'appelais "expérience esthétique authentique" : *ne s'agit-il pas là,* demande-t-il, *de l'idée (…) selon laquelle l'oeuvre d'art livre une vérité supérieure et indicible à celui qui sait en avoir une expérience authentique, indépendamment de toutes les connaissances "triviales" des historiens et critiques d'art?"*

C'est bien à quelque chose comme ça que je pense (sans adhérer aucunement à l'idée que les connaissances des historiens et critiques seraient "triviales"). Cependant, la formulation initiale qui m'était venue "expérience esthétique authentique" me paraît maintenant contestable, et en lisant l'ensemble de la discussion j'en suis venue à admettre que toute expérience esthétique, en tant que telle, doit pouvoir être considérée comme authentique. Mais je distinguerais alors "une expérience esthétique pure", qui surgit à la seule perception de l'oeuvre et "une expérience esthétique enrichie" ou "cristallisée", qui comme son nom l'indique s'enrichit d'éléments externes à cette perception : - diverses connaissances concernant l'oeuvre, son contexte historique, son authenticité matérielle

- mais aussi perceptions annexes : mise en scène, circonstances de la perception (Proust à propos de *François le champi*, cf. Réf. 4 où ce point est discuté)

Une question qui se pose alors est : est-ce qu'une expérience esthétique peut advenir sans se fonder sur une «expérience esthétique pure» ? Par exemple, Paul est mis en présence d'une œuvre d'art plastique. Il reste de marbre devant celle-ci, sa perception ne produit en lui aucune expérience esthétique. L'oeuvre ne lui paraît pas plus une oeuvre que n'importe quel autre objet. Puis il lit la notice qui accompagne l'oeuvre ou Pierre son ami qui trouve cette œuvre

«splendide» lui explique pourquoi elle le touche. La perception de Paul (modalisée par un discours externe à l'oeuvre) en est transformée et la regardant à nouveau, il rencontre alors cette expérience esthétique qui lui échappait jusqu'alors.

Dans cette saynète, il n'y a pas d'«expérience esthétique pure», et pourtant un discours externe sur l'oeuvre a catalysé une "authentique expérience esthétique". Donc ne pourrait-on pas faire l'économie du concept d'"expérience esthétique pure" (EEP)?

Une première objection serait de dire que certes, Paul n'a pas éprouvé l'EEP, mais le rédacteur de la notice ou Pierre l'ont éprouvée, sans quoi ils n'auraient pas été en mesure de la communiquer à Paul.

Une réponse possible : non, l'expérience esthétique de Pierre ou du rédacteur (Jacques) n'est pas plus pure que celle de Paul, c'est seulement leur savoir plus étendu sur l'oeuvre et son contexte qui leur a permis de l'appréhender dans sa dimension esthétique. Et ainsi de suite à l'infini. Donc il n'y a pas d'EEP, toute expérience esthétique serait culturellement informée.

Mais un partisan de l'EEP peut alors rétorquer qu'il faut postuler l'existence d'au moins une EEP au cours de laquelle un jour un homme s'est dit: "tiens, cette chose que je regarde, j'en ignore la raison, mais j'éprouve un plaisir étrange juste à la regarder" (cf. mes remarques sur la «découverte» de l'art africain dans ma réponse précédente).

Pour ma part, je dirais plus volontiers que, telle l'idée d'un acte parfaitement conforme au devoir chez Kant, l'EEP n'advient jamais dans sa forme parfaitement achevée mais qu'elle peut (et non "qu'elle doit" comme en morale) fonctionner comme une idée régulatrice de l'expérience esthétique. On peut alors espérer au moins atteindre ce qu'on pourrait appeler, en filant la métaphore chimique, des «expériences esthétiques quasi-pures».

J'en viens donc à répondre à la question pas simple du tout de Sébastien Réhault (3e partie)

Réponse (3e partie) à Sébastien Réhault
Sylvie Allouche
Oct 3, 2004 14:05 UT

Comme indiqué dans le message précédent, j'en viens donc à répondre à la question pas simple du tout de Sébastien Réhault:
"Question simple: comment sait-on qu'on vit une "expérience esthétique authentique" (…)?"
en la transposant au nouveau vocabulaire que je propose: *comment sait-on qu'on vit une "expérience esthétique pure" d'un Rembrandt et comment la distinguer d'une expérience esthétique "impure"? Autrement dit: quels sont les critères de "cette" pureté?*

Voici donc ma réponse : il est possible qu'on ne vive jamais d'expérience esthétique pure d'un Rembrandt. Il est sans doute impossible de démêler totalement dans notre expérience d'un Rembrandt ce qui tient à la partie pure de notre expérience esthétique et ce qui tient aux différentes cristallisations qui se sont formées et se forment en nous au moment de sa perception. Mais rien n'empêche (rien n'oblige non plus) de cultiver en soi sa "faculté de perception esthétique pure" et pour ma part, devant un Rembrandt, je me poserais la question : "et si je ne savais pas que c'était un Rembrandt, m'extasierais-je autant devant ce tableau?", m'efforçant de démêler en moi la part pure de mon expérience esthétique de sa part cristallisée (connaissance de l'oeuvre, snobisme, fétichisme, etc.). Il y a là quelque chose comme une ascèse de l'expérience esthétique, analogue sur le plan émotionnel à l'ascèse de la pensée sur le plan intellectuel.

Pour l'EEP, il n' y a pas de différence entre une oeuvre nominalement authentique et sa copie parfaite.

Maintenant, il me paraît légitime de hiérarchiser les différentes cristallisations possibles de l'expérience esthétique.
1) tous les éléments de connaissance objective auxquels vous faites référence. Mais (cf. la polémique Barthes/Picard sur la vérité en littérature - évoquée par exemple en Réf. 5), on peut considérer que chacun est libre de décider s'il souhaite enrichir ou non son expérience esthétique de cette connaissance.*
2) le fétichisme : savoir que l'oeuvre qu'on regarde est bien l'oeuvre originale.
3) les circonstances singulières de la réception de l'oeuvre (Proust lisant ou relisant *François le Champi*)
4) le snobisme (vouloir appartenir au "happy few" - ou du moins ne pas appartenir aux "unhappy many")

Sébastien Réhault:
"Enfin, que faut-il comprendre par "augmentation de l'expérience esthétique globale" ? S'il s'agit d'indiquer qu'on est davantage ému à l'idée qu'on a devant soi un vrai Rembrandt, le problème ne progresse pas d'un pouce."

Je ne crois pas que l'expérience esthétique se réduise à sa composante émotionnelle même si elle est fondamentale. En outre, je distinguerais l'intensité proprement dite de l'émotion de sa qualité (de ses nuances, de ses "couleurs"), et ce d'autant que la critique a tendance, me semble-t-il, à valoriser plutôt la seconde. On pourrait alors dire que le gradient d'"expérience esthétique globale" se mesure selon ces deux critères. Un roman à l'eau-de-rose peut nous faire pleurer des torrents de larmes (grande intensité) mais en réveillant en nous des sentiments sans nuance ni subtilité (quelques couleurs, franches et sans grande originalité). En revanche, tel autre roman peut susciter des nuances émotionnelles que nous n'avions jamais éprouvées tant du point de vue de leur qualité intrinsèque que de leurs rapports, produisant ainsi tout un jeu subtil d'irisations (Il me paraît très difficile, et il est peut-être impossible de parler en termes de qualité émotionnelle autrement que par des métaphores. Nous rencontrons là je crois l'une des limites d'un discours argumentatif sur l'art). Et pour ma part, j'ai l'impression d'avoir avancé un peu.

Réponse (4e partie) à Sébastien Réhault
Sylvie Allouche
Oct 3, 2004 20:21 UT

Sébastien Réhault: *"La question demeure toujours de savoir si cette émotion est esthétiquement appropriée ou si elle n'est qu'une preuve de notre tendance irrationnelle à fétichiser, sans aucun rapport avec la signification de l'oeuvre."*

Comme j'espère qu'il suit de mes propositions précédentes, je ne pense pas qu'il y ait nécessairement contradiction entre la fétichisation de l'art et le caractère approprié de l'émotion esthétique qu'on en retire.

En outre, pourquoi la fétichisation serait une tendance particulièrement irrationnelle? Il me semble au contraire que la recherche de l'authenticité relève de la tendance naturelle de l'esprit humain à chercher la correspondance entre la chose et ce qu'il en sait. Certes, ce savoir n'est pas pertinent pour ce que je définissais comme une "expérience esthétique pure", mais dans le cadre de mon expérience esthétique "enrichie", "cristallisée" ou "globale"(EEG), la cristallisation fétichiste me paraît légitimable de deux façons: 1) sur un mode relativiste, on pourrait dire que toute cristallisation est légitime, qu'elle prenne sa source dans le fétichisme, le snobisme, ou

quoi que ce soit d'autre, du moment que l'EEG est augmentée - et de cela, seule la personne qui vit l'EEG peut en témoigner. 2) mais vous n'accepteriez sans doute pas ce premier argument, compte tenu de votre attachement à la correction de l'appréciation esthétique. Donc une hypothèse plus forte consisterait à reprendre l'idée que j'énonçais quelques lignes plus tôt selon laquelle "la recherche de l'authenticité nominale relève de la tendance naturelle de l'esprit humain à chercher la correspondance entre la chose et ce qu'il en sait" ; et à dire que la satisfaction de cette tendance épistémique favorise d'autant, pour parler comme Kant, le "libre jeu des facultés de connaissance" en cause dans l'expérience esthétique. Cela dit, on pourrait aussi utiliser la définition kantienne de l'expérience esthétique pour argumenter en sens inverse – et à l'appui de l'EEP seule. Cela aurait un certain nombre de conséquences que je ne développerai pas ici.

Sébastien Réhault:
Aucun argument n'a pour l'instant montré que la première option était la bonne, sauf à verser avec une étonnante assurance dans la "rhétorique de l'incarnation", pour reprendre la formule de Roger Pouivet.

Histoire de bien clarifier les choses, je précise que je ne crois pas que le tableau original ait des "vibes" particulières, et comme je l'ai déjà dit, je pense que pour la "perception esthétique pure", il n'y a pas de différence entre l'original et sa copie parfaite. Et quand je dis que j'accepte la fétichisation de l'œuvre comme légitime du point de vue de l'EEG, je ne veux pas dire que je considère qu'il y a quoi que ce soit d'«incarné» dans l'œuvre, esprit ou je ne sais quoi d'autre. Mais il y a bien une différence réelle (bien que non perceptible) entre l'œuvre et sa copie parfaite, puisque de l'une on peut dire « celle-ci fut peinte par Matisse » et de l'autre non (sauf à se tromper ou à mentir). Mais seule la croyance que l'on est devant un authentique Matisse et non le fait d'être devant un authentique Matisse joue un rôle dans l'EEG. Après, on peut toujours définir des degrés de fiabilité de cette croyance, mais c'est une autre histoire.

Réponse à Sylvie Allouche
Sébastien Réhault
Oct 4, 2004 14:51 UT

Une EEP serait-elle une expérience sans concepts ? Croyez-vous réellement qu'une telle expérience soit possible ? Comme le dit Gombrich, "le regard innocent est un mythe" (in 'L'art et l'illusion'). Notre regard est toujours chargé d'anticipations, rien n'est vu tout simplement, à nu. Or ces anticipations peuvent être inadéquates aux oeuvres que l'on considère : aborder la statuaire africaine avec l'habitude de la statuaire grecque, par exemple, ou encore aborder la peinture impressionniste avec les critères de la peinture classique dont le modèle serait Poussin. On ne peut pas non plus aborder une statuette africaine comme une chose marron composée de deux ou trois volumes plus ou moins distincts, ce qui constituerait une expérience, non pas pure, mais seulement esthétiquement très pauvre et incapable de comprendre son objet.

Le problème de la copie parfaite. Soit deux oeuvres indiscernables : l'une est un authentique Matisse, l'autre une copie parfaite dont le statut de copie est connu. Ontologiquement, on n'a pas affaire à la même oeuvre, mais les propriétés causales esthétiquement pertinentes sont les mêmes : l'authenticité sémantique est préservée car elle survient sur les mêmes propriétés dans le cas de l'original comme dans celui de la copie. Si malgré cela, vous continuez à soutenir que la croyance que l'on est face à un authentique Matisse dans un cas, face à une copie parfaite dans l'autre, joue un rôle dans l'expérience esthétique, je ne vois pas de quoi vous parlez. Trouverait-on l'authentique Matisse plus beau ou esthétiquement plus intéressant alors qu'il n'y a AUCUNE différence perceptible ? Je ne nie pas l'existence d'un tel phénomène, mais je ne vois décidément pas ce qui le justifie d'un point de vue esthétique.

2e réponse (1ère partie) à Sébastien Réhault
Sylvie Allouche
Oct 10, 2004 19:49 UT

Une EEP serait-elle une expérience sans concepts ?

Non, je ne crois pas, ce que je suggérais déjà dans la 3e réponse en disant "*Je ne crois pas que l'expérience esthétique se réduise à sa composante émotionnelle même si elle est fondamentale.*" Je considère, ce qui n'a rien d'original, que comme à peu près n'importe quelle perception (sauf certaines perceptions pathologiques peut-être) la perception esthétique est informée par l'ensemble de notre savoir, notamment par tout un jeu d'anticipations et autres phénomènes qu'étudient les sciences cognitives. Une EEP serait donc plutôt une expérience esthétique sans croyance ou connaissance préalables au sujet de l'oeuvre perçue. On pourrait alors raffiner avec des considérations du type : lorsqu'on est devant un tableau peint, muni d'un cadre, dans un musée, etc., la "pureté" possible de l'expérience esthétique est d'emblée plus limitée que si, tel Vlaminck, on a le regard arrêté par des statuettes dans un bistrot, etc.

Croyez-vous réellement qu'une telle expérience soit possible ?

Si une expérience sans concepts est possible? oui (il suffirait d'une fois). Si une telle expérience est couramment répandue ? non, mais certaines perceptions pathologiques sont peut-être de telles expériences, bien que cela soit sans doute très difficile à mettre en évidence. L'idée m'en paraît en tous cas d'autant moins absurde d'un point de vue esthétique que l'on pourrait considérer que l'impressionnisme vise, non sans doute à rendre une perception sans concepts, mais du moins à "enlever une couche" de conceptualisation de la perception courante, à moins qu'il s'agisse seulement de la conception de la perception courante impliquée par la peinture classique. Quoi qu'il en soit, dans le tableau impressionniste, l'objet ne précède pas la touche mais en résulte, ou plus précisément, le tableau impressionniste, tout en déployant une scène à la vue du spectateur, met en évidence la façon dont celle-ci résulte de ses mécanismes de synthèse perceptive.

Si une expérience esthétique pure est possible? Comme je le disais déjà dans ma 2e réponse, probablement que non, et c'est pourquoi je proposais d'en faire une sorte d'idée régulatrice plutôt qu'une expérience réelle, et de parler plutôt d'"expériences esthétiques quasi-pures".
Cela dit, comme les expériences dont nous parlons sont singulières, nous ne sommes pas en droit, me semble-t-il, de dire : cette expérience est possible ou n'est pas possible. Comme le choix d'un vocabulaire de la cristallisation y inclinait déjà, ces questions sont comparables à celles qui concernent l'amour : certaines personnes croient au "coup de foudre" parce qu'elles pensent l'avoir vécue. D'autres sont plus dubitatives, mais seraient bien mal avisées de leur dire : "mais non, vous croyez avoir vécu un coup de foudre, mais ce n'est pas le cas, ça n'existe pas". Ensuite, on peut toujours analyser, tel Descartes au sujet des "filles louches", les raisons de ses inclinations, mais ces émotions ne laissent pas d'avoir été vécues.
Une autre façon de répondre à cette question serait de s'intéresser à la façon dont les enfants accèdent à l'expérience esthétique et de voir si l'on peut déceler chez eux quelque chose comme une expérience esthétique pure. Cf. La question de Gloria Origgi "do children care about history in art evaluation?" 10e réponse de la 1ère discussion "should we care about nominal authenticity?"

2e réponse (2e partie) à Sébastien Réhault
Sylvie Allouche
Oct 10, 2004 20:30 UT

Comme le dit Gombrich, "le regard innocent est un mythe" (in 'L'art et l'illusion'). Notre regard est toujours chargé d'anticipations (...). Or ces anticipations peuvent être inadéquates aux oeuvres que l'on considère : (...) aborder la peinture impressionniste avec les critères de la peinture classique dont le modèle serait Poussin. (...).

Je suis d'accord avec vous. Mais le fait est qu'historiquement, les gens ont bien abordé la peinture impressionniste avec les critères de la peinture classique. L'ensemble des connaissances rétrospectives dont on dispose aujourd'hui pour apprécier "Impression, soleil levant" n'était pas disponible, et pour cause, à l'époque. Pourtant, certains spectateurs apprécièrent le tableau comme "authentiquement" esthétique (et d'autres non). Le concept d'horizon d'attente défini par Hans Robert Jauss pour la littérature dans *Pour une esthétique de la réception* trouverait ici un champ d'application très riche. L'on pourrait distinguer en tous cas deux types d'anticipations développées par le spectateur: des anticipations reproductrices, par lesquelles il s'attend à voir la même chose que ce qu'il a déjà vu, et des anticipations créatrices, par lesquelles il se rend ouvert à un certain écart par rapport à ce qu'il connaît déjà. Il me semble que la vision normative et historienne de l'"appréciation esthétique" que vous défendez néglige le caractère privilégié de la situation de l'historien de l'art et implique que l'on devrait se résoudre à être à chaque fois les Leroy des formes d'art en émergence.

Le problème de la copie parfaite. (...) Trouverait-on l'authentique Matisse plus beau ou esthétiquement plus intéressant alors qu'il n'y a AUCUNE différence perceptible ? Je ne nie pas l'existence d'un tel phénomène, mais je ne vois décidément pas ce qui le justifie d'un point de vue esthétique.

- Mais en quoi le fait de pouvoir mettre en relation l'oeuvre perçue avec son contexte de production changerait mieux la perception qu'on en a que le fait de croire en son authenticité nominale? Que l'on prenne perception uniquement dans son sens de "donnée sensible" ou perception dans le sens de "donnée sensible informée par l'ensemble des croyances du sujet percevant", il me semble que le savoir de l'authenticité et les connaissances objectives concernant l'oeuvre jouent, en tant que savoirs, un rôle analogue par rapport à l'appréciation esthétique.

- La distinction que je proposais entre "expérience esthétique pure" EEP et "expérience esthétique cristallisée ou globale" EEG était justement une tentative pour déterminer le statut accordé à ce "phénomène". Comme je le disais dans la 3e partie de ma réponse, "*Pour l'EEP, il n' y a pas de différence entre une oeuvre nominalement authentique et sa copie parfaite.*" Mais il n'y a pas non plus de différence dans le cas où le spectateur combinerait à cette EEP la cristallisation n°1 que je proposais, à savoir "*tous les éléments de connaissance objective auxquels vous faites référence*".Il n'est d'ailleurs pas nécessaire d'admettre la possibilité des EEP (cf. ma 2e réponse) et je reconnais qu'une expérience esthétique peut être activée conjointement par la perception de l'oeuvre et par la connaissance historique que l'on en a. On retrouve alors je pense l'expérience esthétique telle que vous l'entendez et dans ce cas, le savoir de l'authenticité nominale n'apporte rien. Je suis donc d'accord avec vous sur ce point. Mais là où je ne le suis plus c'est que vous faites visiblement de cette expérience la seule légitime alors que pour ma part, j'introduis plutôt une différence de degré de légitimité entre les différentes sortes d'expériences esthétiques distinguées et je me contente d'admettre que l'expérience esthétique informée par une connaissance historique est plus légitime que celle qui se fonde sur le fétichisme. Et encore, je serais aussi prête à le discuter.

L’authenticité en musique ne peut-elle pas avoir plusieurs critères combinables?
Sylvie Allouche
Oct 1, 2004 0:27 UT

Dutton se demande si la véritable authenticité en matière d’interprétation musicale est:

(1) la reconstitution historique de la façon dont une pièce est censée avoir été jouée à l’époque de sa composition, tant dans son instrumentation que dans son interprétation ou (2) l’authenticité d’un jeu qui **révèle** l'essence du morceau indépendamment de toute tentative de reconstitution historique?

(1)“the twentieth-century witnessed the development of an active movement to try to understand better the original sounds especially of seventeenth-and eighteenth-century European music. This has encouraged attempts to perform such music on instruments characteristic of the time, in line with reconstructions of the past conventions that governed musical notation and performance (Taruskin 1995).

(2)But there are other ways in which the music of Bach can be authentically rendered. For instance, Bach’s keyboard writing includes interweaved musical voices which, under the hands of a skilled pianist such as Glenn Gould, can often be revealed more clearly on a modern concert grand than on a harpsichord (Payzant 1978; Bazzana 1997).”

Mais il ne me semble pas qu’il y ait ici d’opposition, il faut seulement distinguer des cas. Dans l’exemple de Dutton, on peut considérer que Glenn Gould respecte plus l’esprit que la lettre de l’oeuvre de Bach. Et il se peut qu’il ne soit pas possible de respecter l’esprit de l’oeuvre de Bach avec les instruments de son époque, ou qu’on peut mieux le respecter avec les instruments contemporains. En ce sens, Bach aurait été visionnaire, puisqu’il aurait créé une musique pour laquelle il ne disposait pas encore d’instrument adéquat. Mais il se peut aussi que Glenn Gould soit un meilleur interprète de Bach que tel claveciniste et que si Glenn Gould jouait Bach sur un clavecin d’époque selon les standards de l’époque (et en conservant l’esprit), ce soit une interprétation encore plus authentique que quand il joue Bach au piano. Les critères d’authenticité proposés par Dutton ne sont donc pas nécessairement incompatibles.

Voici un tableau qui part de cette discussion par la définition d'un “gradient d’authenticité” où je distingue plusieurs critères qui sont susceptibles d'être combinés. Le tableau permet aussi de questionner l’authenticité de l’expérience esthétique de l’auditeur et d’éventuellement “mesurer” son goût. (cf suite de la discussion).

> **Révélation**
> Roger Pouivet
> Oct 1, 2004 8:33 UT
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> La solution a ces problèmes d'identité et d'authenticité est d'une simplicité biblique : la lettre tue et l'esprit vivifie ! Comment n'y avait-on pas pensé plus tôt ?
>
> > **Réponse à Roger Pouivet**
> > Sylvie Allouche
> > Oct 1, 2004 14:13 UT
> >
> > Le vocabulaire lettre/esprit était sans doute mal choisi. Je voulais juste trouver une façon de nommer l'opposition que suggère Dutton et qui me paraît légitime entre une interprétation "in line with reconstructions of the past notations that governed musical notation and performance"

et une interprétation par laquelle "interweaved musical voices (are) revealed more clearly". Je n'y mettais en tous cas pas la connotation spiritualiste que vous semblez y voir.

Mais voici une nouvelle proposition qui permettrait d'abandonner ce vocabulaire tout en reprenant la distinction de Dutton entre « authenticité expressive » et « authenticité nominale » ainsi que vos propres remarques au début de votre contribution "pas d'esthétique sans ontologie!".

Je renomme ce que j'appelai « la lettre » « interprétation historique » et en fais une forme de l'authenticité nominale.

En outre, je suis d'accord avec vous pour dire que la notion d'authenticité expressive de Dutton manque de clarté et qu'il faut distinguer deux sens possibles de celle-ci, (a) l'authenticité "sémantique" et (b) l'authenticité "éthique". Ici, je pourrais vous faire le même genre de querelle à propos d' "éthique" que vous me faites à propos de "lettre/esprit", du fait des connotations morales que comporte le sens courant d' "éthique", même si les hellénistes distingués peuvent supposer que c'est à la notion grecque d' « ethos » que vous vous référez. Mais gardons « éthique ».

Je suis aussi d'accord avec vous pour dire qu'il est tout à fait contestable de rabattre l'authenticité expressive sur l'"authenticité éthique", comme semble le faire Dutton, qui témoigne ce faisant d'une vision romantique de l'art, qui a été contestée depuis au moins la 2e moitié du XXe siècle et qui aurait dû pour cette raison, être discutée, même brièvement. Mais si l'on admet, que l'exigence d' « authenticité éthique » peut avoir du sens (comme c'est souvent le cas dans la musique populaire) alors il faut distinguer pour les arts performatifs l'authenticité de l'auteur de celle de l'interprète (et éventuellement encore du chef d'orchestre, etc.).

Quoi qu'il en soit, « l'esprit » de l'œuvre tel que je l'entendais me semble assez bien correspondre à votre propre «authenticité sémantique ». Seriez-vous donc d'accord pour dire que l'authenticité à laquelle parvient Glenn Gould quand il joue Bach, selon Dutton, est votre "authenticité sémantique"?

J'en arrive donc pour l'authenticité en musique à : - Une authenticité nominale (instruments historiques, interprétation historique) - Une authenticité expressive (sémantique et éthique (de l'interprète ou de l'auteur))

Les catégories pourraient encore être raffinées, mais je m'arrêterai là pour cette réponse.

Complément de réponse à Roger Pouivet
Sylvie Allouche
Oct 2, 2004 8:00 UT

A propos de "*La lettre tue et l'esprit vivifie*".

Comme l'indique explicitement le passage suivant de mon premier message "*il se peut aussi que (…) si Glenn Gould jouait Bach sur un clavecin d'époque selon les standards de l'époque (et en conservant l'esprit), ce soit une interprétation encore plus authentique que quand il joue Bach au piano*", loin de vouloir montrer que "la lettre (le respect des standards historiques d'interprétation) tue et l'esprit vivifie", je voulais suggérer que les critères d'authenticité musicale dégagés par Dutton sont compatibles (comme l'indique aussi le titre de mon message), contrairement à ce qu'il semble vouloir dire – et donc la lettre ne tue pas.

Il est vrai cependant que mon "tableau" pris indépendamment de son contexte peut induire en erreur dans la mesure où j'y oppose "selon l'esprit" et "selon la lettre" comme s'ils étaient

incompatibles. Mais en fait, “la lettre” est pour moi identique à “selon les standards anciens” (ou à “l’interprétation historique” définie dans la réponse précédente). C’est donc un “sans l’esprit” que j’aurais dû opposer à "selon l’esprit" dans mon tableau, mais j’ai cru, à tort visiblement, que l’opposition "selon l’esprit" "selon la lettre" serait plus claire étant entendu d’après ce que je disais plus tôt que «la lettre» correspondait aux standards anciens, et qu’il fallait donc comprendre «selon la lettre seulement», ce qui du fait de la redondance catégorielle au sein du tableau me paraissait implicite. Mais l’implicite ne doit pas avoir de place dans une argumentation et j’ai eu tort.

A la lumière des nouvelles catégories définies dans la première partie de ma réponse, il faudrait de toute façon recomposer le tableau notamment en distinguant clairement authenticité nominale et authenticité expressive, et en plaçant dans cette 2e catégorie l’“authenticité sémantique”, l’“authenticité éthique de l’interprète” (qui apparaissait déjà dans mon tableau) et celle de l’auteur.

Je ne crois pas que ces différentes suggestions soient d’une «simplicité biblique». Mon idée est au contraire de montrer que les oppositions que met en place Dutton sont contingentes, contrairement à ce qu’il semble dire, qu’il y a différents cas à examiner et donc que le problème est plus «complexe».

The ontology of forgery

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Abstract:
Forgery doesn't have an autonomous existence, because it depends on a mistaken, intentionally misleading, attribution. The mode of existence of forgery is parasitic on authenticity. Thus the right question is not "What is a forgery?" but "What can be the object of a forgery?" Roger Pouivet argues here that Nelson Goodman's most criticized distinction between *autographic* and *allographic* works of art provides a starting point for an ontology of forgery.

1. In Chapter 3 of *Languages of Art*, Goodman introduces an essential distinction between "autographic" art and "allographic" art. A work of art is "autographic if and only if the distinction between the original and the copy has meaning; or rather, if even its most exact reproduction does not have the status of authenticity." Unlike painting, music is allographic. It follows that a musical forgery is *ontologically impossible*.

Someone could certainly copy one of Bach's scores and present it as his own, but he would obtain exactly the same score, *viz*. Bach's, and not his own. If he claims to have found a score by Bach, which instead he himself has written, then we are back in the case of autographic art. He has not forged a musical work, nor, for that matter, a musical text. He presents something as having been written by Bach that is in fact his own production, just as one might do with a forged Rembrandt at an auction at Sotheby's. If he publishes a photograph of the score (allegedly by Bach) in the *Journal of the American Musicological Society*, it will be more effective. Yet it would be a mistake to believe that such a case represents a counter-example to Goodman's thesis that there are no forgeries in music, since such a forgery is autographic, not allographic.

One musical performance may be preferred over another because it exemplifies certain intentional properties of the work that the other failed to bring out. But the identity of a work is not threatened by an interpretation whose musical-historical grounds are neither satisfying nor accurate. It's even just on the basis of respect for the notational identity of the work that an interpretation is criticized for not respecting the "truth" of the work. We can distinguish between a *narrow identity*, which is independent of the interpretation, and a *thick identity*, which instead is dependent upon it. The narrow identity of a musical work does not depend upon the exemplification of its intentional properties. Playing *The Art of the Fugue* on a Steinway piano is questionable, but it is still Bach's work that's being played. The question of forgeries in art thus arises only for autographic works.

2. Goodman's autographic-allographic distinction applies not only to the arts, but also to things in general. In one case, their identity is historical, in the other, notational. Whether it concerns works of art—paintings, musical pieces, prints, novels, etc.—a recording of Thelonious Monk or Madonna, banknotes, signatures, passports, Lacoste shirts, Cartier watches or any number of other things, Goodman's theory can be applied. It doesn't belong only to the philosophy of art. The identity of things assumes that we can re-identify the same thing, *whatever it may be*.

Goodman's distinction results from a *unified*, precise and coherent theory of identity, but its edges are too rough for avoiding the difficult cases that have become the mainstay of critics. In order to avoid these problems, one may be tempted to drawn on as many concepts of authenticity as there are domains in which the problems arise. One would then have a concept that's characteristic of works of art, indeed, of works of certain arts, or even of certain works of art from certain periods. Some attempts in this direction, which do not take the form of a unified theory, are quite interesting, in particular Stephen Davies' *Musical Works and Performances* (2001). This approach, however, gives rise to a criticism of Goodman's autographic-allographic distinction since his aim is much broader than just providing a description of our various, complex practices for authenticating works of art. In these practices, we adapt the criteria of authenticity to the kinds of art, to the kinds of works, genres, periods, etc. through a procedure that might suit Goodman: that of reflective equilibrium. We change the rule

when it leads to a result we deem untenable; and we modify our habits when we think that the rule must be respected.

3. Unlike an ontological description of our practices for classifying works of art, an ontology of forgery is the result of prior metaphysical conceptions. These conceptions are obtained by answering the question of knowing whether the underlying metaphysical notion is that of substance, of parts and the whole, of tropes, events, etc. If, for example, one adopts a conception of the work of art as kinds of action (Currie, 1989) or as an activity that's identified by its provenance (what D. Davies, 2004 calls a "performance"), the ontology of forgery that follows will differ from that held by a follower of substantialist metaphysics. It's true that the fundamental ontological assumptions in the ontology of art, and in the ontology of forgery in particular, are rarely explicit. Authors often write as if it were a question for the philosophy of art, while they are in fact taking a position in a much larger debate regarding what exists in general. They give the impression of providing an ontological description of our artistic practices and sometimes smuggle in a general ontology.

4. In what follows I outline some elements of an ontology of forgery whose grounds are part of a metaphysics that treats works of art as particular, concrete artifactuals (artificial substances) possessing certain properties. The basic elements of this view borrow quite a bit from the Thomistic-Aristotelian tradition. First, this approach is taken without criticism of different views, such as a metaphysics of types of events. Second, this option maintains Goodman's distinction between the autographic and the allographic (Pouivet, 1996, ch. 5), i.e., between entities whose identity is historical and others whose identity is notational. One could, however, approach it according to the principle of reflective equilibrium in order to avoid the difficulties raised in the criticisms of Goodman.

5. A forgery possesses a certain property: that of being fake. In order to understand what being fake consists in for a painting, a passport or a banknote, one must first understand what this property consists in.

(a) Is it an *essential property*, that is, a property that makes a thing what it is? It seems odd to think that all forgeries, regardless of what they are, possess a generic property: inauthenticity. A forged Vermeer, for example, is an authentic Van Meegeren.

This claim could be disputed. There is a difference between the re-attribution of a painting by Piero della Francesca to Luca Signorelli, and that of a painting by Vermeer to Van Meegeren. In the first case, the painting was wrongly attributed; in the second, the painting is a forgery. Once the forger is unmasked, the painting remains a forgery. (The painting isn't admired as an excellent Van Meergeren, but always as a forged Vermeer). But this situation concerns, in fact, very few works. In most cases, one goes from attribution to re-attribution, and not from a discredited attribution to the elimination of all authentic value whatsoever. Yet, a forged Vermeer by Van Meegeren remains a forgery because it exemplifies what we mean by a forgery as regards paintings, and not because it possesses an essential property that makes it a forgery in itself.

One could still object that a forged Belgian passport or a forged Polish banknote is not at all an authentic Swedish passport or a real Moldovan banknote. The reason is not that they instantiate inauthenticity, but that they were not issued by the institution authorized to do so.

Thus, inauthenticity cannot be an essential property because a painting, passport or banknote is only real or fake according to a correct or mistaken attribution. If one attributes a Van Meegeren painting to Vermeer, the painting is not fake in itself, but under a description of this painting as a work of Vermeer. A passport or a banknote is fake under a description of the passport or banknote as being issued by an institution that, in fact, did not issue it. This is why Monopoly money isn't fake, because no one believes it was issued by a government. (But there could be fake Monopoly money that turned out to be authentic banknotes!)

Nothing is *a* forgery, as something can be *a* man or *a* tulip. Under a given attributive description, certain things are forgeries; that is, they cannot be attributed to the person or institution that are said to have made or issued them.

(b) If a thing is only a forgery under an attribution, then inauthenticity is not an *intrinsic property*, i.e., a property that a thing can have independently of everything else (a thing's form or matter is intrinsic). For a key, the property of opening a certain door or, more specifically, any door having a lock with the same cylinders, is not an intrinsic, but an extrinsic, property; it depends on the existence of that lock. As Frédéric Ferro says, "If the lock were changed, the key would lose what the fans of hidden properties might call its 'opening property' *vis-à-vis* that door, without losing any intrinsic property" (2002, p. 531). Likewise, a painting is not intrinsically fake; it is so only under an improper description, that is, if there is something else, a person, to say something false about it. If the attribution were changed and corrected, then as regards those who believed the first description, the painting would not lose any intrinsic property.

(c) Inauthenticity is not a *relative property* either. A property is relative if, in order to possess it, it suffices that it be attributed to something. One may speak here of an *exclusively phenomenal* or *projective* property—what was once called the associations of ideas, that is, a perspective on something that's accessible only from one point of view. (For example, the property of evoking something: "This path reminds me of my childhood.") Even if they aren't intrinsically authentic or fake, the authenticity or inauthenticity of paintings, passports or signatures is not a relative property.

(d) Inauthenticity is an *extrinsic property*. It assumes an attribution, that is, a statement declaring that the thing is the work of *y*. The same holds for a passport or banknote. When one gives it to a police officer or a merchant, one implicitly says that this passport was issued by the relevant authority, or that this banknote was made by an institution having the right to print money.

A thing is authentic or fake, not because it possesses an essential property (the inauthenticity common to all forgeries), or an intrinsic property (which it possesses independently of any attribution), but on the basis of an attributive statement, true or false, saying that this thing is the work of *y* (a person, authority, institution).

6. But by examining the thing itself, isn't it possible to determine whether it's a forgery? The expert examines a painting in order to detect signs of its inauthenticity; the policeman examines a passport and the merchant a banknote. Doesn't this mean that these are intrinsic properties that constitute its authenticity, and that their absence or at least internal difference constitutes its inauthenticity? If *x* is a forgery, then there is a property *P* that it does not possess and that it should possess, or there is a property *P** that it should not possess and that it instead does. Forgers hide these properties that experts (sometimes) discover.

However, what makes a thing a forgery is neither this property *P* nor the property *P**. Making a *copy* of a painting (a reproduction) or a *photocopy* of banknotes (e.g., to teach one's children how to use banknotes) is not enough for them to be forgeries. If two paintings, one authentic and the other a copy, are impossible to distinguish, there remains an extrinsic property which makes one the original and the other a reproduction (cf. Pouivet, 2000, ch. 8).

A painting by Lorrain becomes a painting by La Tour without any change in its intrinsic properties due to the single fact of a new attribution. A careful examination of an object whose authenticity is in doubt is made relative to a statement of attribution whose truth needs to be examined. It's a conditional statement of the type, "If *x* is the work of *y*, then *x* must possess property *P* and cannot have property *P**." For example, *x* must be composed of certain chemical compounds, it must be a certain color, and it cannot be made of or have any others. The examination of *x*'s intrinsic properties remains linked to a statement of attribution saying who the author of it is, so that we may be able to make a claim regarding the authenticity of *x*. This is why authenticity is an extrinsic property even though a well-informed judgment of authenticity and inauthenticity involves the examination of its intrinsic properties.

7. Authenticity is a matter of identity. *Identity is relative*, in the sense that *x* is not authentic or fake in itself, but it is an authentic or fake *y*. When we say that a painting, passport or banknote is fake we claim that, appearances notwithstanding, it is not of the same kind of another thing, *z*, which is really the work of *y*. Authenticity is a matter of *sortal* categories: a category is a classification of kinds if it provides the identity conditions for distinguishing and counting its members. Everything that is attributed to *y* forms a class. To say that *x* is fake amounts to claiming that it does not belong to a certain class of objects under a certain description.

8. This means that properties that make it possible to establish the authenticity of an object do not make up an *individual essence*. A definition of 'individual essence' is: *E* is an individual essence if, by definition, there is an *x* that necessarily exemplifies *E*, and there is no *y*, different from *x*, that exemplifies *E*.

However, we may be reluctant to accept the idea that properties that establish the authenticity or inauthenticity of a thing are kinds and not individual properties. Isn't an object the work of *y* independently of its correct or incorrect (misleading) attribution to *y*? Thus, something just is *this* object since it possesses an individual essence that makes it that object, independently of any attribution.

Yet assuming this to be the case, one must not confuse, on the one hand, what it is that makes an object that object—an object unlike any other—and on the other hand, its authenticity or inauthenticity. Saying that a thing is fake doesn't say anything about what it is, but instead amounts to refusing to take it as an element of the class of things that can be attributed to *y*. As a result, the property of being authentic or being fake does not individuate, rather, it *specifies*. It says that *x* possesses a property shared by all those things that can be attributed to *y*, or that it does not possess this property. And this property is extrinsic since it assumes an attribution.

9. However, that the property of being fake (like that of being authentic) is extrinsic does not imply that it cannot be necessary. Having a father (or a parent) is an extrinsic property that a good number of animals necessarily possess: just as every artifact, regardless of what it is, necessarily has a maker. But the attribution does not say:

(1) (*x*) (*x* is authentic) → □ (∃ *y*) (*y* is the maker of *x*)

(*for all x, if x is authentic then there necessarily exists a y that is the maker of x*)

but says instead,

(2) □ ((*x*) (*x* is authentic) → (∃ *y*) (*y* is the maker of *x*))

(*necessarily, for all x, if x is authentic then there exists a y that is the maker of x*)

(1) attributes *de re* to *x* the necessary property of being the work of *y*. (2) attributes *de dicto* to *x* the property of being authentic only under the attribution to *y*.

The forger's aim, the one who knowingly makes a false attribution, i.e. who lies, is to give the impression that authenticity is tied to the thing's individual essence. Through his "intuition" he may be able to detect the presence of this individual essence defined in (IE). He says that, for someone endowed with the appropriate receptivity or sensibility, the painting *manifests* this individual essence. As a result, by applying (1) he arrives at the conclusion that *y* is necessarily the maker of *x*. Instead, with (2), if *y* is the author of *x*, then no one else can be; but one cannot say that *x* possesses a property for which no one else could be the author of *x*. A Vermeer does not possess (*a property of*) *vermeerness*. It follows that the only possible justification of authenticity is *historical inquiry*. It is not a miraculous intuition by which inspired individuals grasp the individual essence of an object. This is, however, what the forger suggests in order to cut short the historical inquiry that could unmask him.

If something is a forgery then it possesses a temporal (historical) property that it should not have, and it does not possess a temporal (historical) property that it should have.

11. If the arguments just outlined are correct, a number of conclusions can be drawn. First, things are only authentic under an attributive description; hence, authenticity is an extrinsic property. Second, an ontology of forgery for which the authenticity of an object is in part tied to its individual essence suits the forger. The one who attributes to an object the property of being only and nothing other than *y*, while knowing that such is not the case, is more a of forger than the one who makes a forgery. For the forger, the metaphysical fiction of individual essences suits his claim of "artistic intuition" quite well. Third, the only way for determining the authenticity of an autographic entity is to examine its history in

order to assure oneself of the likelihood that it is in fact the work of the person to whom it is attributed (cf. Pouivet, 1992).

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FRENCH VERSION

L'Ontologie du Faux de Roger Pouivet

Dans le chapitre III de *Langages de l'art*, Goodman introduit une distinction fondamentale entre arts « autographiques » et arts « allographiques ». On désigne « une œuvre comme autographique si et seulement si la distinction entre l'original et la contrefaçon a un sens ; ou mieux, si même sa plus exacte reproduction n'a pas, de ce fait, statut d'authenticité » (tr. fr. 1990, p. 147). À la différence de la peinture, la musique est allographique. Donc, un faux musical est une *impossibilité ontologique.*

Quelqu'un peut bien sûr recopier une partition de Bach et la présenter comme sienne, mais il obtient exactement la même partition, celle de Bach, pas la sienne. S'il prétend avoir trouvé un manuscrit de Bach, qu'il a lui-même rédigé, nous revenons au cas des arts autographiques. Il n'a pas contrefait une œuvre musicale, ni même un texte musical. Il présente comme étant de la main de Bach ce qui est de la sienne, exactement comme on le ferait d'un faux Rembrandt lors d'une vente chez Sotheby. S'il publie une photographie de ce manuscrit (prétendument de Bach) dans le *Journal of the American Musicological Society,* ce sera plus efficace. Mais ce serait une erreur de croire qu'un cas de ce genre est un contre-exemple à la thèse de Goodman qu'il n'existe pas de faux en musique. Car un tel faux est autographique et pas allographique.

Une exécution musicale peut être préférable à une autre parce qu'elle exemplifie certaines des propriétés intentionnelles de l'œuvre que l'autre échoue à manifester. Mais l'identité d'une œuvre n'est pas menacée par une interprétation dont les attendus musico-historiques ne sont pas satisfaisants ni même corrects. C'est même seulement sur la base du respect de l'identité notationnelle de l'œuvre qu'une interprétation est critiquée pour ne pas respecter la « vérité » de l'œuvre. On peut distinguer une *identité étroite,* indépendante de l'interprétation et une *identité épaisse*, qui en dépend. L'identité

étroite d'une œuvre musicale ne dépend pas de l'exemplification de ses propriétés intentionnelles. Jouer l'*Art de la fugue* sur un piano Steinway est contestable, mais c'est bien l'œuvre de Bach qui est jouée.

Dès lors, la question du faux en art ne se pose que pour les œuvres autographiques.

La distinction goodmanienne entre autographique et allographique porte non pas seulement sur les arts, mais sur les choses en général. Dans un cas leur identité est historique, dans l'autre notationnelle. Qu'il s'agisse d'œuvres d'art : tableaux, œuvres musicales, estampes, romans, etc., d'un enregistrement de Thelonious Monk ou de Madonna, de billets de banque, de signatures, de passeports, de polos Lacoste, de montres Cartier ou de bien d'autres choses, la théorie proposée par Goodman peut s'appliquer. Elle n'est pas propre à la philosophie de l'art. L'identité des choses suppose que nous sachions ré-identifier la même chose, *quelle qu'elle soit.*

La distinction goodmanienne relève d'une théorie *unifiée* de l'identité, précise et cohérente, mais son grain est trop gros pour ne pas se heurter à des cas difficiles dont les critiques ont fait leurs choux gras. Pour éviter ces difficultés, on peut être tenté de recourir à autant de conceptions de l'authenticité que de domaines où le problème se pose. On aurait ainsi une conception propre aux œuvres d'art, voire aux œuvres de certains arts, et pourquoi pas à certaines œuvres d'art de certaines périodes. Certains efforts en ce sens, qui donc ne prennent pas la forme d'une théorie unifiée, sont fort intéressants, tout particulièrement ceux de Stephen Davies dans son ouvrage de 2001 : *Musical Works and Performances.* Cette approche conduit à une critique de la distinction goodmanienne entre autographique et allographique, parce que l'intention de Goodman est bien plus générale que de faire une description de nos pratiques différenciées et complexes d'authentification des œuvres d'art. Dans ces pratiques, nous adaptons les critères d'authenticité aux types d'arts, aux types d'œuvres, aux genres, aux époques, etc., par un procédé qui ne déplairait pas à Goodman, celui de l'équilibre réfléchi. Nous changeons la règle quand elle aboutit à un résultat qui nous semble intenable et nous modifions nos habitudes quand nous pensons que la règle doit être respectée.

À la différence d'une description ontologique de nos pratiques de classification des œuvres d'art, une ontologie du faux résulte de conceptions métaphysiques préalables. Ces conceptions sont obtenues en répondant à la question de savoir si la notion métaphysique de base est celle de substance, de parties et de tout, de trope, d'événement, etc. Si, par exemple, on adopte une conception de l'œuvre d'art comme types d'action (Currie, 1989) ou comme activité identifiée par sa provenance (ce que D. Davies, 2004 appelle une « *performance* »), l'ontologie du faux qui s'ensuit sera différente de celle d'une partisan d'une métaphysique substantialiste. Il est vrai que les présupposés ontologiques fondamentaux en ontologie de l'art, et en ontologie du faux en particulier, sont rarement explicités pour eux-mêmes. Les auteurs font souvent comme s'ils traitaient d'une question de philosophie de l'art, alors qu'ils prennent en réalité position dans un débat plus large au sujet de ce qui existe en général. Ils donnent l'impression de faire une description ontologique de nos pratiques artistiques et proposent, parfois en contrebande, une ontologie générale.

La suite de ce texte donne quelques éléments d'une ontologie du faux dont les attendus appartiennent à une métaphysique qui traite les œuvres d'art comme des particuliers concrets artefactuels (substances artificielles) possédant des propriétés. Les éléments de base de cette conception empruntent beaucoup à la tradition thomistico-artistotélicienne. Premièrement, cette option est retenue sans critique des conceptions différentes, comme celle d'une métaphysique de types d'évènements, par exemple. Deuxièmement, cette option retient la distinction goodmanienne entre autographique et allographique (Pouivet, 1996, chap. V), c'est-à-dire entre des entités dont l'identité est historique et d'autres dont l'identité est notationnelle. Cependant, il serait possible de l'aménager selon le principe de l'équilibre réfléchi afin de ne pas aboutir à des difficultés signalées par les critiques de Goodman.

Un faux possède une certaine propriété, celle d'être faux. Pour savoir ce qui, pour un tableau, un passeport ou un billet de banque, consiste d'être un faux, il est d'abord nécessaire de savoir ce en quoi consiste cette propriété.

(a) Est-ce une *propriété essentielle,* c'est-à-dire une propriété qui en fait la chose qu'elle est ? Il semble étrange de penser que tous les faux, quels qu'ils soient, possèdent une propriété générique : la fausseté. Car un faux Vermeer est, par exemple, un authentique Van Meegeren.

Cette affirmation pourrait être contestée. Il existe une différence entre la ré-attribution d'un tableau de Piero della Francesca à Luca Signorelli et celle d'un tableau de Vermeer à Van Meegeren . Dans le premier cas, le tableau a été faussement attribué ; dans le second le tableau est un faux. Le faussaire démasqué, le tableau reste un faux. (On n'admire pas le tableau comme un excellent Van Meegeren, mais toujours comme un faux Vermeer.) Mais, cette situation concerne en fait fort peu d'œuvres. Dans la plupart des cas, on va d'attribution en ré-attribution, et non d'attribution dénoncée en destitution de toute valeur d'authenticité correcte. Cependant, un faux Vermeer par Van Meegeren reste un faux parce qu'il exemplifie ce que nous entendons par la contrefaçon en matière de peinture, et non parce qu'il possèderait une propriété essentielle qui en fait un faux en soi.

On pourrait objecter encore qu'un faux passeport belge ou un faux billet de banque polonais n'est pas du tout un authentique passeport suédois ou un vrai billet de banque moldave. La raison n'est pas qu'ils instancient la fausseté, mais qu'ils n'ont pas été émis par l'institution habilitée à le faire.

Ainsi, la fausseté ne peut pas être une propriété essentielle parce qu'un tableau, un passeport ou un billet de banque ne sont vrais ou faux qu'en fonction d'une attribution correcte ou erronée. Si l'on attribue un tableau de Van Meegeren à Vermeer, à vrai dire, le tableau n'est pas faux en lui-même, mais sous une description de ce tableau comme œuvre de Vermeer. Un passeport ou un billet de banque est faux sous une description de ce passeport ou de ce billet de banque comme émis par une institution qui, en réalité, ne l'a pas émis. C'est pourquoi les billets de banque du Monopoly ne sont pas faux, car personne ne croit qu'ils ont été émis par un État. (Mais il pourrait exister de faux billets de Monopoly qui se trouveraient être d'authentiques billets de banque !)

Rien n'est *un* faux, comme quelque chose peut être *un* homme ou *une* tulipe. Sous une certaine description attributive, certaines choses sont fausses, c'est-à-dire ne sont pas attribuables à la personne ou l'institution dont on prétend qu'elles les ont faites ou émises.

(b) Si une chose n'est un faux que sous une attribution, la fausseté n'est pas une *propriété intrinsèque,* c'est-à-dire une propriété qu'une chose peut avoir indépendamment de tout autre chose (la forme ou la matière d'une chose est intrinsèque). Pour une clef, la propriété d'ouvrir une certaine porte ou, plus précisément, toute porte sur laquelle est montée une serrure avec les mêmes barillets, n'est pas une propriété intrinsèque, mais extrinsèque ; elle dépend de l'existence de cette serrure. « Si la serrure était changée, la clef perdrait ce que les amis des qualités occultes appelleraient sa "vertu apéritive" vis-à-vis de cette porte sans perdre aucune propriété intrinsèque », dit Frédéric Ferro (2002, p. 531). De même un tableau n'est pas intrinsèquement faux ; il ne l'est que sous une description abusive, c'est-à-dire s'il existe autre chose, une personne, pour en dire quelque chose de faux. Si l'attribution était modifiée et correcte, à l'égard de ceux qui croyaient l'attribution initiale, le tableau ne perdrait aucune propriété intrinsèque.

(c) La fausseté n'est pas non plus une *propriété relative.* Une propriété est relative s'il suffit que quelqu'un l'attribue à une chose pour qu'elle la possède. On peut aussi parler de propriété *exclusivement phénoménale* ou *projective* – ce qu'on appelait autrefois des associations d'idées, c'est-à-dire une perspective sur quelque chose, accessible seulement d'un point de vue. (Par exemple, la propriété d'évoquer quelque chose : « Ce petit chemin me fait penser à mon enfance »). Même s'ils ne le sont pas intrinsèquement authentiques ou faux, l'authenticité ou la fausseté des tableaux, des passeports ou des signatures n'est pas une propriété relative.

(d) La fausseté est une *propriété extrinsèque.* Elle suppose une attribution, c'est-à-dire un énoncé déclarant que la chose est l'œuvre de *y*. Cela vaut aussi pour un passeport ou un billet de banque. Quand on tend l'un à un policier ou l'autre à un commerçant, on dit implicitement que ce passeport a été attribué par l'autorité compétente, que ce billet de banque a été fait par une institution possédant le droit de battre monnaie.

Une chose est authentique ou fausse non pas parce qu'elle possède une propriété essentielle (la fausseté de tous les faux) ou une propriété intrinsèque (qu'elle possède indépendamment de toute

attribution), mais en fonction d'un énoncé attributif, vrai ou faux, disant que cette chose est l'œuvre de *y* (personne, autorité, institution).

Mais n'est-ce pas en examinant la chose elle-même qu'il est possible de déterminer si c'est un faux ? L'expert examine le tableau pour y déceler des indices de sa fausseté, le policier examine le passeport et le commerçant le billet de banque. Cela ne signifie-t-il pas que ce sont des propriétés intrinsèques qui font l'authenticité et leur absence ou au moins une différence interne qui fait la fausseté ? Si *x* est un faux, il existe une propriété *P* qu'il ne possède pas et qu'il devrait posséder ou il existe une propriété *P** qu'il ne devrait pas posséder et qu'il possède. Les faussaires cachent ces propriétés que les experts (parfois) découvrent.

Pourtant, ce qui fait d'une chose un faux n'est pas cette propriété *P* ou cette propriété *P**. Il ne suffit pas que quelqu'un fasse une *copie* d'un tableau (une reproduction) ou *photocopie* des billets de banque (par exemple, afin d'apprendre à des enfants à utiliser des billets de banque) pour qu'il s'agisse de faux. Si deux tableaux, l'un authentique, l'autre une copie, sont impossibles à distinguer, il reste une propriété extrinsèque qui fait de l'un l'original et l'autre la reproduction (voir Pouivet, 2000, chap. VIII).

Un tableau du Lorrain devient un tableau de La Tour sans aucune modification de ses propriétés intrinsèques, du seul fait d'une nouvelle attribution. L'examen minutieux d'un objet dont l'authenticité est douteuse se fait relativement à un énoncé d'attribution dont il s'agit d'examiner la vérité. C'est un énoncé conditionnel du type « Si *x* est l'œuvre de *y,* alors *x* doit posséder la propriété *P* et ne peut pas avoir la propriété *P** ». Par exemple, *x* doit être composé de certains composés chimiques, être d'une certaine couleur, et ne peut pas en posséder d'autres ou en avoir d'autres. Ainsi, pour que nous puissions nous prononcer sur l'authenticité de *x,* l'examen de ses propriétés intrinsèques reste lié à un énoncé d'attribution disant quel est l'auteur de la chose. C'est pourquoi l'authenticité est une propriété extrinsèque, même si un jugement informé en matière d'authenticité et de fausseté passe par l'examen de ses propriétés intrinsèques.

L'authenticité est une affaire d'identité. *L'identité est relative,* au sens où *x* n'est pas en soi authentique ou faux, mais il est un authentique ou un faux *y.* Quand nous disons qu'un tableau, un passeport ou un billet de banque est faux, nous affirmons que, malgré les apparences, il n'est pas de la même sorte qu'une autre chose, *z*, qui est vraiment l'œuvre de *y*. L'authenticité est affaire de catégories *sortales* : une catégorie est sortale si elle nous fournit des conditions d'identités pour distinguer et compter ses membres. Toutes les choses qui sont attribuées à *y* forment une classe. Dire que *x* est faux revient à affirmer qu'il n'appartient pas à une certaine classe d'objets sous une certaine description.

Cela signifie que les propriétés qui permettent de fixer l'authenticité d'un objet ne forment pas une *essence individuelle*. Voici une définition de l'essence individuelle :*E*est une essence individuelle si, par définition, il existe un *x* qui exemplifie nécessairement *E* et il n'existe pas d'*y,* différent d'*x*, qui exemplifie *E*.

Cependant, nous pouvons être réticent à l'idée que les propriétés fixant l'authenticité ou la fausseté d'une chose sont sortales et non pas individuelles. Un objet n'est-il pas l'œuvre de *y* indépendamment de son attribution correcte ou incorrecte (mensongère) à *y* ? Dès lors, quelque chose est bien *cet* objet parce qu'il possède une essence individuelle qui en fait cet objet-là, et cela indépendamment de toute attribution.

Pourtant, à supposer que ce soit le cas, il ne faut pas confondre d'une part ce qui fait d'un objet cet objet-là, différent de tout autre, et d'autre part son authenticité ou sa fausseté. Dire qu'une chose est fausse ne dit rien sur ce qu'elle est, mais revient à refuser de la tenir pour un élément de la classe des choses qu'on peut attribuer à *y*. Ainsi, la propriété d'être authentique ou d'être faux n'individualise pas, mais *spécifie*. Elle dit que *x* possède une propriété commune à toutes les choses qu'on peut attribuer à *y* ou qu'elle ne possède pas cette propriété. Et cette propriété reste extrinsèque parce qu'elle suppose l'attribution.

Cependant, que la propriété d'être faux (comme d'être authentique) soit extrinsèque n'implique pas qu'elle ne puisse être nécessaire. Avoir un père (disons un géniteur) est une propriété extrinsèque

que possèdent nécessairement bon nombre d'animaux. De même tout artefact, quel qu'il soit, possède nécessairement un producteur.

Mais l'attribution ne dit pas :

(*x*) (*x* est authentique) ® □ ($ *y*) (*y* est le producteur de *x*)

(*pour tout x, si x est authentique alors il existe nécessairement un y qui est le producteur de x*)

mais

(2) □ ((*x*) (*x* est authentique) ® ($ *y*) (*y* est le producteur de *x*))

(*nécessairement, pour tout x, si x est authentique alors il existe un y qui est le producteur de x*)

(1) attribue *de re* à *x* la propriété nécessaire d'être l'œuvre de *y*. (2) attribue *de dicto* à *x* la propriété de n'être authentique que sous l'attribution à *y*.

10. Tout l'intérêt du faussaire, celui qui fait sciemment une fausse attribution, qui ment donc, est de donner l'impression que l'authenticité est liée à l'essence individuelle de la chose. Son « flair » lui permettrait de déceler la présence de cette essence individuelle définie dans (EI). Il dit que, pour quelqu'un qui est doté de la réceptivité ou de la sensibilité appropriée, tel tableau *manifeste* cette essence individuelle. Dès lors, en appliquant (1), il en arrive au résultat que *y* est nécessairement le producteur de *x.* En revanche, dans (2), si *y* est l'auteur de *x*, alors personne d'autre ne peut l'être, mais on ne peut pas dire que *x* possède une propriété qui fait que personne d'autre n'aurait pu être l'auteur de *x*. Un Vermeer ne possède pas (une *propriété*) *de veermérité.* Dès lors, la seule justification possible de l'authenticité est *l'enquête historique.* Ce n'est pas une intuition merveilleuse grâce à laquelle des personnes inspirées saisissent l'essence individuelle d'un objet. C'est pourtant ce que le faussaire suggère pour couper court à l'enquête historique qui pourrait le démasquer.

Si quelque chose est un faux il possède une propriété temporelle (historique) qu'il ne devrait pas avoir et il ne possède pas une propriété temporelle (historique) qu'il devrait avoir.

11. Si les idées qui viennent d'être esquissées sont correctes, certaines conclusions peuvent être tirées. Premièrement, rien n'est authentique sinon sous une description attributive. Donc, l'authenticité est une propriété extrinsèque. Deuxièmement, une ontologie du faux pour laquelle l'authenticité d'un objet a partie liée avec son essence individuelle n'est pas pour déplaire au faussaire. Le faussaire est donc moins celui qui fait un faux que celui qui attribue à un objet la propriété de ne pouvoir être que de *y*, tout en sachant que ce n'est pas le cas. Pour le faussaire, la fiction métaphysique des essences individuelles accompagne fort bien son prétendu « flair artistique ». Troisièmement, la seule façon de déterminer l'authenticité d'une entité autographique est d'en faire l'histoire afin de s'assurer de la probabilité qu'il soit bien l'œuvre de celui auquel il est attribué (voir Pouivet, 1992)

Discussion

Temporalité de l'attribut FAUX
Jean-François Gauchotte
Oct 4, 2004 11:29 UT

Restons sur l'exemple du tableau. Parler d'un FAUX, c'est se placer après la découverte de la contre-façon. Ainsi le faux en soi serait un leurre ou une figure de rhétorique alors que l'attribut de fausseté à un objet désigné serait l'actualisation d'un jugement moral normatif implicitement réaliste.

Dissection ou explication?
Jose Luis Guijarro
Oct 4, 2004 16:42 UT

Je trouve les distinctions « ontologiques » de Roger Pouivet très intéressantes mais (malheureusement, il y a toujours un « mais » !) elles me posent au moins deux problèmes d'interprétation :
(1) S'agit-il vraiment des distinctions ontologiques ou ne serait-ce plutôt que des distinctions purement mentales sur des concepts symboliques (i.e, autographe, allographe, vrai, faux, etc.) acquis historiquement ?
(2) Comment une dissection structurée d'une construction symbolique, tout aussi détaillée que possible réussirait-elle à nous expliquer pourquoi à un moment donné on rejette comme non authentique un objet quelconque ? L'idée de la fausse attribution à Y fonctionne institutionnellement seulement (donc, est tout à fait liée à la culture). Ce que je voudrais comprendre est pourquoi, à quel moment et de quelle manière ces fausses attributions nous font perdre l'enthousiasme qu'une expérience esthétique nous cause.
Evidement, cela n'a pas été l'intention de l'auteur. Je crains d'être tombé dans le syndrome-des-membres-de-jurys-de-thèse qui voudraient que le candidat écrive la thèse qu'ils auraient voulu écrire eux mêmes !

Wich ontology?
fabrice bothereau
Oct 4, 2004 21:25 UT

1) The term "ontology", as used by R. Pouivet, is, as it seems, problematic. Why? Because ,when we talk about ontology, it is presupposed that we think of things as first; like the ontology of being, or the ontology of social objects. Then, we intend ontology as a quality prior to any subsequent fact. But what's happening when it is asserted that something like an ontology of forgery actually exist? If something like an ontology of forgery exist, say, if a forged work of art exist, it exist only in it's original link to the True one. So, if the forged work of art is ontologic, what have we got to say concerning the real one? The answer must be: the real ontology belongs to the Original work of art. But if the forged is ontologic, the real can no longer be ontologic either, since we'd get twice the same ontology for the same object, wich is absurd. So, how will we name the Real object? Super-ontologic? Arch-ontology? And what shall we say of a forged work wich have no Ur-ontology ? (like Les portes de l'Enfer, by Rodin, who never casted its plaster models). 2) The problem of the expression "ontology of forgery", is echoing the one concerning "authenticity", and "originality"; categories that R. Kraux has shown to be eminently classical (The Originality of the Avant-garde); and remind us of what W. Benjamin said. In the epoch of the industrial reproductibility of a work of art, the question of authenticity and originality don't prevail anymore. What is the original copy? one could ask. By the way, these questions about forged/real seem antedated according some artists (see Sherrie Levine and her true-false attributions of works of art). 3) We'd like to conclude with this little puzzle piece: "Also, apart from the actual occurrence of the same things in other occasions, every actual occasion is set within a realm of alternative interconnected entities. This realm is disclosed by all the untrue propositions wich can be predicated significantly of that occasion. It is the realm of alternative suggestions, whose foothold in actuality transcends each actual occasion. The real relevance of untrue propositions for each actual occasion is disclosed by art, romance, and by criticism in reference to ideals …/… The truth that some proposition respecting an actual occasion is untrue may express the vital truth as to the aesthetic achievement. It expresses the 'great refusal' wich is its primary characteristics. An event is decisive in proportion to the importance (for it) of its untrue propositions: their relevance to the event cannot be

dissociated from what the event is in itself by way of achievement. These transcendent entities have been termed 'universals'" (Whitehead, Science and the Modern World).

Qu'est-ce qu'il arrive?
Jose Luis Guijarro
Oct 8, 2004 16:43 UT

L'article de Pouivet, "L'ontologie du faux" semble ne pas susciter aucune discussion ; voilà presque une semaine qu'il est là et il a seulement trois réponses, dont une d'elles, la mienne n'est réellement pas une, puisque je crains de l'avoir mal interprété. Cependant, comme il n'a pas jugé intéressant de solutionner mon problème d'interprétation, la possible discussion a été avortée au départ. Je viens de relire l'article et je dois dire que, dans ma possible interprétation erronée, il me semble que, couché dans un langage philosophique, les idées qu'il expose sont en réalité des vérités de Lapalisse et que c'est pour cela justement que personne est en désaccord avec elles. Seulement Fabrice Bothereau fait quelques observations non sur le contenu mais sur les précisions philosophiques. Je ne suis pas philosophe et ne puis pas donner un jugement sur ces indications, mais je crois, par contre, que tout le monde est d'accord avec les idées que (1) pour être authentique il faut qu'il existe un canon extrinsèque attribué, (2) les faussaires essayent de faire passer les faux comme si c'était un vrai en essayant de reproduire le mieux possible tous les détails qui font croire aux gens que l'objet s'adapte au canon, et (3) la seule manière de qualifier un objet comme authentique est de découvrir son origine historique, et non pas d'énumérer ses caractéristiques sensibles qui, comme on vient de dire, peuvent être copiés magistralement par le faussaire. Avec ces trois idées incontestables comme base de la discussion, je ne vois pas comment on pourrait commencer à discuter.

Réponse à Fabrice Bothereau
Sébastien Réhault
Oct 11, 2004 9:54 UT

Il me semble que Fabrice Bothereau fait ici une confusion sur l'emploi du terme "ontologique". Il ne s'agit pas d'un prédicat décrivant une propriété d'objet, quand bien même on penserait que cette propriété est l'être ou l'existence. L'ontologie est une discipline philosophique dont dont les objets sont, grosso modo, l'être en tant que tel, les catégories fondamentales de la réalité et le mode d'existence des différents types de choses. Faire de l'ontologie, même appliquée à un certain domaine, comme le faux en art par exemple, ne présage en rien des thèses particulières qui seront défendues. Le texte de Roger Pouivet se présente simplement comme une interrogation sur le mode d'existence du faux : parler d'une "ontologie du faux" ne signifie pas que le faux existe en tant que tel, mais simplement que l'on va s'interroger sur la façon dont le faux existe. Même si l'on soutenait que le faux existe en tant que tel (ce que ne soutient pas Roger Pouivet), cela ne signifierait pas qu'il est "ontologique" et qu'on devrait donc trouver une autre catégorie (comme "super-ontologique"!) pour désigner les choses qui semblent exister davantage que lui. Le mode d'existence du faux est un problème ontologique ; dire que le faux en tant que tel est "ontologique" n'a tout simplement pas de sens.

Réponse à Fabrice Bothereau
Sébastien Réhault
Oct 11, 2004 9:55 UT

Il me semble que Fabrice Bothereau fait ici une confusion sur l'emploi du terme "ontologique". Il ne s'agit pas d'un prédicat décrivant une propriété d'objet, quand bien même on penserait que cette propriété est l'être ou l'existence. L'ontologie est une discipline philosophique dont dont les objets sont, grosso modo, l'être en tant que tel, les catégories fondamentales de la réalité et le mode d'existence des différents types de choses. Faire de l'ontologie, même appliquée à un certain domaine, comme le faux en art par exemple, ne présage en rien des thèses particulières qui seront défendues. Le texte de Roger Pouivet se présente simplement comme une interrogation sur le mode d'existence du faux : parler d'une "ontologie du faux" ne signifie pas que le faux existe en tant que tel, mais simplement que l'on va s'interroger sur la façon dont le faux existe. Même si l'on soutenait que le faux existe en tant que tel (ce que ne soutient pas Roger Pouivet), cela ne signifierait pas qu'il est "ontologique" et qu'on devrait donc trouver une autre catégorie (comme "super-ontologique"!) pour désigner les choses qui semblent exister

davantage que lui. Le mode d'existence du faux est un problème ontologique ; dire que le faux en tant que tel est "ontologique" n'a tout simplement pas de sens.

Réponse à Jose Luis Guijarro

Roger Pouivet
Oct 11, 2004 12:54 UT

Jose Luis Guijarro pense que les idées dans mon article sont des lapalissades, ce qui expliquerait que tout le monde ait été d'accord avec elles et que que personne ne prenne la peine des les discuter. Je suis tenté de prendre cette remarque pour très grand compliment, tant il est philosophiquement difficile de dire ce sur quoi nous sommes tous d'accord. Mais je ne crois hélas pas le mériter. La discussion de l'article de Denis Dutton a montré que, pour certains intervenants, la fausseté d'une oeuvre d'art est liée aux caractéristiques de l'expérience esthétique que nous en faisons et doit s'expliquer en cela. Or, comme l'a fort bien compris Jose Luis Guijarro, je propose des arguments en faveur d'une thèse incompatible avec cette conception esthétique du faux. Si je devais m'inquiéter d'un quelconque mutisme de mes virtuels interlocuteurs, j'y verrais plutôt le symptôme d'un profond désaccord que de l'évidence manifeste pour tout un chacun de mes affirmations.

Olé, cela s'anime!!!

Jose Luis Guijarro
Oct 11, 2004 16:12 UT

Roger a écrit :
« […] pour certains intervenants, la fausseté d'une oeuvre d'art est liée aux caractéristiques de l'expérience esthétique que nous en faisons et doit s'expliquer en cela. Or, comme l'a fort bien compris Jose Luis Guijarro, je propose des arguments en faveur d'une thèse incompatible avec cette conception esthétique du faux ».
Donc, si j'ai bien compris (je répète mon résumé tentatif des thèses de Roger)
« (1) pour être authentique il faut qu'il existe un canon extrinsèque attribué, (2) les faussaires essayent de faire passer les faux comme si c'était un vrai en essayant de reproduire le mieux possible tous les détails qui font croire aux gens que l'objet s'adapte au canon, et (3) la seule manière de qualifier un objet comme authentique est de découvrir son origine historique, et non pas d'énumérer ses caractéristiques sensibles qui, comme on vient de dire, peuvent être copiés magistralement par le faussaire. Avec ces trois idées incontestables comme base de la discussion, je ne vois pas comment on pourrait commencer à discuter ».
Je pense qu'il s'agit ici de deux points de vue (et non pas de deux idées différentes sur la fausseté ou l'authenticité de quelque chose). D'une part, le point de vue de Roger : de quelle manière reconnaît-on le faux ? D'autre, mon idée du problème, celui que j'aurais voulu voir résoudre et qui, d'après moi n'a pas été touché ici : comment s'expérimente la fausseté chez l'esprit humain. En d'autres termes, est ce qu'il y a un réfèrent cognitif réel pour la distinction culturelle du VRAI / FAUX ? Pourquoi notre esprit se dit charmé par le VRAI (et de quelle condition est ce « charme enthousiaste ») et n'éprouve pas la même chose quand il « traite » quelque chose de faux ? S'agit-il seulement d'un résultat d'accommodation culturelle externe qu'on prétend avoir pour être considéré un « expert » par la société, ou bien est ce qu'on éprouve cognitivement quelque chose de différent ? Quand, dans l'histoire que je racontais l'autre jour à la discussion précédente, le poète lorquien faisait des vers à la García Lorca et ils étaient perçus comme tels par le vieux critique, et quand ce même critique fût convaincu qu' un de ces poèmes était réellement de Federico G. Lorca, qu'est ce qui changeait dans l'esprit du vieux critique ? Il me serait difficile de croire que les qualificatifs « très mauvais » ou « très bon » proviennent uniquement d'une condition historique sans aucun rapport avec un effet cognitif.

Réponse à Sébastien Réhault

fabrice bothereau
Oct 11, 2004 22:01 UT

Je ne vois pas comment on peut soutenir que l'ontologie du faux signifie que le faux n'existe pas, qu'il s'agit d'une interrogation sur l'existence du faux. Si on s'interroge sur l'existence du faux, c'est que le faux existe. Et bien sûr que le faux existe, puiqu'il y a Van Meegeren !, et la Chasse Spirituelle, et des vrais-faux passeports : « dire que le faux en tant que tel est "ontologique" n'a tout simplement pas de sens ». Ce n'est pas moi qui le dit, c'est Roger Pouivet. Quand on titre « l'ontologie du faux », et qu'on

avance des exemples de faux, c'est qu'il existe des cas où le faux est exemplifié ; par conséqent, on asserte qu'il y a une ontologie du faux. Le problème que je voulais pointer, c'est que l'assertion ontologique est ontologique, il n'y a rien « avant ». Or, dans le cas du faux, il y a quelque chose « avant », soit le « vrai ». De fait, asserter une ontologie du faux est problématique. Ce qui est intéressant dans le texte de Goodman, c'est qu'on voit bien que le faux devient vrai pendant un certain temps ; le temps par exemple où tout le monde, y compris les experts, valident les Meegeren comme d'authentiques Vermeer. Que se passe-t-il alors, quand le faux est tenu pour vrai ?

Répétitions utiles ?
Roger Pouivet
Oct 12, 2004 11:30 UT

Il existe évidemment des faux, personne n'a dit le contraire. Mais le faux n'est pas faux en soi, mais en fonction d'une attribution incorrecte. Pourquoi parle-t-on d'une "ontologie du faux" ? Parce que le faux a bien un mode spécifique d'existence, lié à une attribution fausse. On peut faire une ontologie de choses qui dépendent d'autres, qui n'existent pas en elles-mêmes. Par exemple, on peut faire une ontologie de propriétés, sans penser que les propriétés existent au même titre que les choses dont elles sont les propriétés. C'est à peu près aussi original que d'être aristotélicien plutôt que platonicien!
L'article est donc une ontologie du faux, i.e. de la propriété "être un faux", laquelle est liée à une attribution. Autrement dit, "être un faux" est une propriété extrinsèque qu'une chose possède en fonction d'une attribution. S'il s'agissait de faire une "ontologie des boîtes de pâté pour chien à poil long", on pourrait être choqué d'un tel usage du terme "ontologie", mais une ontologie du faux n'a rien d'aberrant, pas plus qu'une ontologie des frontières, des trous ou des ombres, lesquelles ont été brillamment illustrées par Barry Smith ou Roberto Casati. (Les anglophones appellent parfois cela "folk ontology".)
L'article entend ainsi montrer qu'un "vrai" ne devient pas "faux", ou un "faux" un "vrai", mais que l'authenticité est une affaire d'attribution. Quand un tableau de Van Meegeren est tenu pour un tableau de Vermeer, aucun faux n'est vrai, mais un tableau est attribué incorrectement à Van Meegeren. Supposons que personne ne se soit jamais aperçu de la supercherie, cela ne faisait pas du tableau de Van Meegeren un tableau de Veermer. Cela faisait simplement une attribution incorrecte tenue pour vraie.
Fabrice Bothereau dit : "Si on s'interroge sur l'existence du faux, c'est que le faux existe". Il faudrait dire alors: "Si on s'interroge sur l'existence du Père Noël, c'est que le Père Noël existe". On pourrait aussi appliquer la formule à Dieu : si on s'interroge sur son existence, c'est qu'il existe. On est assez loin d'une preuve ontologique, mais assez près d'un sophisme.

le faux et le Faux
Sébastien Réhault
Oct 12, 2004 16:15 UT

Des oeuvres qu'on tente de faire passer pour ce qu'elles ne sont pas, certes, cela existe. Cela signifie-t-il, d'un point de vue ontologique, que le faux a une nature propre ou qu'une oeuvre peut être un faux en soi, par essence ? Pas nécessairement. A moins d'être un platoniste forcené, on n'est pas obligé d'admettre que si certains objets exemplifient la propriété d'être faux, c'est que cette propriété existe indépendamment des instances de choses fausses. Un faux en art ne signifie pas que l'objet en question possèderait des caractéristiques qui détermineraient sa nature et ferait de lui un faux absolu, quelques que soient les circonstances. Comme le dit Roger Pouivet : "la fausseté ne peut pas être une propriété essentielle parce qu'un tableau, un passeport ou un billet de banque ne sont vrais ou faux qu'en fonction d'une attribution correcte ou erronée." Le faux existe, certes, mais pas n'importe comment : c'est une propriété (extrinsèque) de certains objets dans certaines circonstances. Quand je disais que le faux n'existe pas en tant que tel, je voulais dire que le Faux n'existe pas en tant que tel.

Essence vs cognition?
Jose Luis Guijarro
Oct 13, 2004 11:27 UT

Sébastien Réhault demande: « Cela signifie-t-il, d'un point de vue ontologique, que le faux a une nature propre ou qu'une oeuvre peut être un faux en soi, par essence ? Pas nécessairement »
Cependant, il peut avoir une réaction COGNITIVE distincte quand on expérimente quelque chose qui est marquée (publique ou individuellement) comme vraie ou comme fausse. Pareil à Dupont et Dupond, je dirais même plus : la différence entre le VRAI el le FAUX, dans le domaine de l'art au moins (je ne crois pas que cela soit ainsi dans le domaine monétaire, par exemple), est une conséquence de cette opération mentale. C'est-à-dire : il y a dû avoir ORIGINELEMENT deux sortes de réactions aux stimuli que certains objets nous produisent. Puis, après, avec cette base cognitive, certains objets ont été marqués socialement (ou culturellement) avec le mérite d'être VRAIS (par exemple, si le vers à la García Lorca est écrit par Federico lui-même) et, donc, on peut traiter l'information qu'ils nous fournissent de la manière cognitive qu'on éprouve en face du « sublime ». Naturellement, pour aboutir à un explication, il faudrait décrire cognitivement cette manière de traiter l'information artistique et comprendre a quoi est-ce que cela revient pour une information que d'être considérée « sublime » dans le domaine de l'art.
Selon mon point de vue, donc, la description de l'ontologie du faux qu'on est en train de discuter ici vise surtout les indicateurs sociaux que chaque culture estime nécessaires (l'auteur original, la manière de faire quelque chose, le pouvoir magique qu'on y ajoute, etc.) et qui, je crois, sont indiscutables. Mais je ne vois pas comment cela expliquerait la différence cognitive de base.

Réponse à S.Réhault et R. Pouivet.
fabrice bothereau
Oct 13, 2004 14:44 UT

Il me semble qu'on ne peut pas poser qu'il existe des choses et des attributs. A moins de poser qu'il y a des substances, et des qualités ; ce qu'il aurait alors fallu préciser. Qu'est-ce que le faux par essence, quelle différence entre le faux et le Faux (Réhault) ? Y a-t-il un vrai par essence ? Il faudrait expliquer ce qu'est une proposition intrinsèque par rapport à une proposition extrinsèque. Etre en vie est aussi une attribution ; est-ce que la vie est intrinsèque, ou extrinsèque ? R. Pouivet : « intrinsèque, c'est-à-dire une propriété qu'une chose peut avoir indépendamment de tout autre chose ». Qu'est-ce qu'une propriété indépendante de tout autre chose ? Encore une fois, sommes-nous placés ici dans une philosophie de la substance ? Le vrai, en dehors de son attribution, n'existe pas non plus. Je ne parle pas de l'existence du Père Noël, ni de Dieu, je parle du faux, et il y a des exemplifications du faux ; mais pas du Père Noël, ni de Dieu. Je ne vois pas où est le sophisme. Peut-être est-ce là une marque d'agacement (sensible dans le titre de R. Pouivet). Mais si nous ne pouvons pas discuter, à quoi sert-il de proposer des textes à la discussion ?

Forgery and Reproduction

Gregory Currie (Professor of Philosophy, University of Nottingham)

(Date of publication: 18 October 2004)

Abstract: Forgery needs to be distinguished from reproduction. It is sometimes said that the aesthetically relevant question raised by the existence of artistic forgery is whether a picture visually indistinguishable from a valuable picture is itself valuable--to the same or to any degree. Yet I suggest that one may have no qualms, aesthetic or of any other kinds, about the honest reproduction of art while at the same time thinking that forgery is a bad thing.

Forgery needs to be distinguished from reproduction. It is sometimes said that the aesthetically relevant question raised by the existence of artistic forgery is whether a picture visually indistinguishable from a valuable picture is itself valuable to the same or to any degree. Yet I suggest that one may have no qualms, aesthetic or of any other kinds, about the honest reproduction of art while at the same time thinking that forgery is a bad thing.

A sensible case against forgery is based on the premise that misrepresentation of a work's origins has the capacity to mislead the viewer who seeks to experience and to understand that work. For understanding a painting depends on more than an awareness of what colours are where on its surface, It requires knowing something about how those colours got to be there, with what representational and other intentions, against what stylistic and generic background, in the light of what available techniques and theories of representation. Nor, as Goodman pointed out, is the damage confined to the work in question: thinking that a van Meegeren is a Vermeer has the effect of distorting your view about what class of works it is appropriate to compare other Vermeer's against--or, for that matter, other Dutch interiors.

But if that is right, then surely a practice of reproduction in painting is, if not misleading, then at least aesthetically self-defeating. After all, the reproduction has its origin in quite different circumstances from those of the original. It is made (generally) by a different person at a different time, using quite different methods. What could be more starkly contrasting than the painstaking work of Leonardo, and the pressing of a button on the standardly imagined "super Xerox machine" which churns out something that can't be distinguished, by appropriate means of looking, from Leonardo's work. In that case, the argument against forgery also shows that reproduction cannot achieve anything aesthetically equivalent to the original. You might reduce the starkness of the contrast by having the reproduction done by a skilled and sensitive painter, but even here there remains a substantial and surely relevant difference between the circumstances of making of these two works.

But consider the following line of thought. Painting is not the only art form where a proper appreciation of the work requires an understanding of its origins. The same is true of literary arts; one cannot be said to understand fully a literary work merely on the basis of an understanding of the meaning of its constituent words and sentences. Indeed, some people (I am one) go so far as to argue that two distinct literary works can have the same text: the same word sequence. However, no one will claim that my copy--produced by mechanical means--of *Emma* fails to provide me with full access to the work, that I would somehow be better off if I could read it in the original handwriting.[1]

What this shows, I think, is that the objection to forgery which is based on the need for historical understanding of the work cannot, on its own, be used as an argument against reproducibility.

One line of thought is this. With painting, the original canvas is the object in which the aesthetic properties of the work inhere. If the work is said to represent a flower, to be by Bosschaert, to have revived a flagging genre, then it is this thing--this painted canvas, which has these properties. No copy of the work, however faithful, has those properties. So the original is aesthetically distinctive. Not so with literature; if the novel is said to have revived a flagging genre, it is not this physical object--my copy--which has that property. In this respect, the original (the autograph) is not aesthetically

distinctive. The autograph gives us access to the aesthetic properties of the work, it does not possess them. And the same can be said of any correct copy.

In other work I have denied the premise of this argument: that the aesthetic properties of the work inhere in the original canvas. I denied this by denying that the work and the original canvas are the same thing. Not everyone wants to go so far. Suppose we accept that the original canvas is the work, that the aesthetic properties of the work are properties of this object. It follows that you are not looking at the work when you look at a perfect copy of it, that you are not looking at the thing in which the aesthetic properties of the work inhere. The question then is this. To what extent does this fact (assuming it is a fact) compromise your capacity to appreciate the work by examining a perfect copy of it?

One answer is that you cannot appreciate the work by examining a perfect copy because you are not thereby put in an appropriately direct relationship with the relevant properties. When you confront the original canvas, you confront those properties, because they inhere in the object. When you confront a copy, you do not.

However, one might draw a different conclusion, namely that this merely shows that appreciation of the work does not require any such direct confrontation. Instead, it requires merely that you be put in a position where you can fully appreciate those properties. And perhaps you can appreciate the properties of one object by being directly confronted with different properties of another object.

Take, for example, the case of a picture in which a group of figures is finely composed. This is a property that the work has in virtue of it having been made in a certain way, namely by an artist who set about putting paint on with a brush in accordance with a certain plan. If the picture had resulted from an accident with paint there would be no question of it being finely composed. But suppose that I am not in a location where I can view the original, so the Gallery obligingly super-faxes me a perfect copy of it. What I am looking at does not have the property of containing a finely composed set of figures, since the way in which this copy was made differs dramatically from the way the original was made; the copy was made by a mechanical process that did not involve the act of composition. But is it not still true that I can fully appreciate the compositional elements of the original just as well by looking at this perfect copy as I could by looking at the original? Similarly, if I want to know how far this picture differed from other pictures of the same artist, period or genre, I can find out as easily by looking at the copy as I can by looking at the original. Of course I cannot know these things *just* by looking at the original; I have to have a lot of art historical knowledge as well. But whatever art historical knowledge equips me to answer these questions when looking at the original, that same knowledge also equips me to answer them when I am looking at the copy.

To this extent at least, the art-historical case against forgery is not also a case against reproduction. What does this tell us about question with which I began? The question invites us, I think, to consider the perfect copy as somehow an independent work, and for us then to decide whether this work is aesthetically the equal of the original. But that is not the way to see the relation between original and copy. The creation of the copy is not the creation of any work; it is the creation of a copy of an existing work. The aesthetic equivalence between original and copy is not the aesthetic equality of two works. Rather, it consists in the fact that the copy is as good a means of appreciating the aesthetic properties of the work as the original canvas is.

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[1] Indications of revisions present in the original but lost in the copy might be thought to make a difference here. To simplify, let us consider only cases where the autograph copy provides no evidence of such revision. Certainly, it would be hard to argue that, from the point of view of understanding the process of revision, a chemically identical copy would do worse than the original autograph.

Discussion

copies and possession of properties
John Zeimbekis
18 oct. 2004 12:51 UT

Greg Currie makes a distinction between (i) appreciation of the properties of the work by “direct confrontation”, and (ii) being “put in a position where you can fully appreciate those properties”. This then serves as the ground for distinguishing originals from copies. The distinction may lead to equating appreciation of originals and appreciation of adequate copies, something I can see no reason to object to (especially since we’re assuming the copy is ‘adequate’ in just the relevant respects).

But I would like to point out why I think that copies *do* possess the properties that originals possess and which ground appreciation (or constitute value, or cause cognitive effects, or allow certain functions).

First, the case of “a picture in which a group of figures is finely composed”. Greg claims that when I am looking at a (super) photocopy of the picture, I am not looking at a composition. If we go down this road, then an email does not convey the meaning-intentions that structure its syntax. So we have to accept that what we are looking at is a composition and that the property of being composed is possessed by both original and copy. A possible objection to this could be that the painter did not issue the copy. But this can be answered by comparing the copy to a quotation (a forwarded email). Only an accidental identical configuration is not composed; copying techniques are devised precisely to preserve composition, just as computer programmes are designed to preserve type-identical syntax.

Second, Greg imagines someone saying: “you cannot appreciate the work by examining a perfect copy because you are not thereby put in an appropriately direct relationship with the relevant properties. When you confront the original canvas, you confront those properties, because they inhere in the object. When you confront a copy, you do not.” To this he replies: “this merely shows that appreciation of the work does not require any such direct confrontation.”

But I think it shows, instead, that there is appreciation (value, functions, etc) because there is instantiation of the properties by the copy. Imagine that I say to someone, “When you go to Greece, be careful never to make this gesture___”. “___” refers to the gesture at issue, but it also is that gesture. The same relation obtains between colours, shapes and aesthetic properties in the copy and colours, shapes and aesthetic properties in the original. Therefore, the copy and the original both possess those properties.

> **No more copies or too much copies ?**
> Roger Pouivet
> 18 oct. 2004 18:48 UT
>
> If John is right, it seems to me that it is simply useless to retain the distinction between original and copy. John says that all artworks can have many instantiations, even paintings. So, there are no more copies, only many instanciations of the work. Greg Currie's account is more restrictive and retains the distinction between original and copy. A copy with certain properties gives access to aesthetic properties of the original work. But John's remark about the greek gesture is quite important. Copy's properties could not be different from original aesthetics properties, they must be the same instantiated properties. If they were not, we would be in another case, this one : I look at the numbers on the balance (some properties) and I know that I lose weight. (Dretske calls it "displaced perception".)The properties of the balance give me access to a property of my own, even if the first are quite different from the second. In this case, even a not at all perfect copy, even a bad one, could be good access to some properties of the original. So it seems to me that (i) in John's account there are no more copies, and (ii) that Greg's account cannot be limited to "perfect" copy, and there are too much copies.

Reply to Roger Pouivet
John Zeimbekis
19 oct. 2004 16:20 UT

I take the point made by Roger. I'll try to suggest a way out below. But first, a comment to place the discussion back in a broader context: in claiming that copies share properties with originals, I do not deny that copies exist. First, because there are imperfect copies, which do not share all relevant properties with the original and yet are considered copies. Second, because even a perceptually indiscernible copy *is* a copy, since it is numerically distinct and was produced later and with different techniques. The broader context is that perceptually indiscernible replicas are fakes for investors, but not necessarily for anyone interested in making the object work as art, nor even necessarily for anyone interested in the object as a historical or even biograpical record.

The way out of Roger's "no copies" objection is that, in order to preserve the distinction between originals and copies, we need to find some way of defining an *imperfect* copy: if we claim (as I do) that there is a sharing of properties, then we have to define the status of copies with different properties which are *nevertheless* treated as copies, in the way the posters and slide projections of paintings are.

I think the relation that defines being an *discernible* copy should be a relation of pictorial representation to the original, plus something else: the discernible copy has to be a 2D scale-model of the original (thus preserving the proportions of occlusion shapes, the contours of colour regions and shading regions, etc). From here on, what counts as a copy seems to depend on the copying techniques available. There was a time when all copies were engravings, in black and white. Today there seems to be great tolerance of colour variation in copies, but not of changes in the contours of occlusion shapes and shadings.

However, if this is what a criterion would look like (a criterion capable of narrowing down the notion of a discernible copy to keep us from having "too many copies"), it cannot be a description of how copies work, or of how we perceive them: copies do not function as representations of originals, even if, in the occurrence, they do bear that relation to originals. We cannot look at an imperfect copy (a poster) in order to be informed of the original, because to the extent that the poster is an imperfect copy, it does not give the right information: if the colours are slightly different, we will not be able to visualize the colours of the original any better by looking at the copy; and so on.

Reply to John Zeimbekis
Roger Pouivet
20 oct. 2004 14:47 UT

1) I agree with John that his way out my objection works quite well. We must distinguish instantiation of properties and instantiation of the work itself. But I wounder if there is not still a Cratylian ghost behind this position : a *perfect* copy would not be a copy but another instantiation of the work.

2) John says: "perceptually indiscernable replicas are fakes for investors, but not necessarily for anyone interested in making the object work as art". I prefer to say that something is or is not a fake, and that the notion of "fake for x and not for y" is dangerous, as would be the notion of "lies for x and not for y". Forgery is not related to what we think about something and our insterest for it, but to a claim about its author.

An aside
Jose Luis Guijarro
21 oct. 2004 17:03 UT

The distinction about originally manufactured objects (in this present case, restricted to paintings) and copies of them works when those copies are made by humans who somehow achieve their own "interpretation" of what the original object is. That interpretation may be

further limited (or perhaps enhanced!) by the imitator's skills. In both those cases it is perhaps philosophically interesting to determine which aspects of the two objects coincide and which differ.

I have two questions here:

The first is the following. What happens if some student of painting gets crazy about the sort of pictures that were made in, say, the Italian Renaissance , and starts painting pictures in that style, without copying any known author and tries to sell them as anonymous paintings of that time? Would that be considered a fake (i.e., a "bad ting" in a theological perspective), or would it?

The second is: if by using super-modern technological devices, one could nowadays make exact copies of whatever object we would like, which would be the different relevant aspects worth to take into account? The carbon 14 test to ascertain the age of the board and the pigments? Is that kind of difference essential in any way to characterize our cognitive "artistic" reaction to that object? In what way?

Incidentally, and to soften the "divine" condemnation of imitators, I read the other day that a young Spanish Philosopher has recently received an important prize for his work on IMITATION. He seems to argue that imitation, from nature, or from others that are admired and used as models of behaviour, is not "bad" in any way whatsoever. According to him, imitation is one of the most important tendencies of the human mind; its possible importance may be linked to its power to create strong communal bonds in human groups by assimilating some of its social representations. After all we all learn to socialise by imitation!

Reply to José Luis Guijarro
Richard Minsky
23 oct. 2004 10:46 UT

If an artist "tries to sell them as anonymous paintings of that time," it is both an authentic original work, and a fraud, because it is not true. It makes no difference whether the attribution is to Vermeer or Anonymous. Perhaps the artist is motivated by profit, because the Renaissance attribution brings more cash than that of an unknown artist. But if (s)he is copying the style and misattributing it as a conceptual work of art, as a commentary on the art world and its values, does that make it not a fraud, or is fraud then an act of art? If the artist later reveals the deception, does that change what it is? "Appropriation" was a popular art "movement" a couple of decades ago. That was the copying of another's work and claiming originality of the copy, as not plagiarism.

José Luis Guijarro brings up another issue, "we all learn to socialise by imitation!"

Many accounts have been written (and filmed) about true-life examples of social "forgery," such as *The Great Imposter*. Someone who never went to medical school masquerades as a doctor and performs surgery. Perhaps this is performing art, in which art imitates life, and becomes life, since the patient is unaware that the person amputating his real life leg is a fraud who is imitating a doctor. Is this 'not "bad" in any way whatsoever?'

On the other hand, when I saw Hal Holbrook on stage in *Mark Twain Tonight*, the actor was reproducing the appearance, dress, mannerisms and words of the author, creating the (very effective) illusion that the dead poet was on the stage. Imitation is a necessary condition of both forgery and reproduction.

Unanswered questions
Jose Luis Guijarro
18 oct. 2004 16:25 UT

I am lost! It seems to me that we have passed from the socio-cultural frame (Denis Dutton), to the philosophical one (Roger Pouivet), and have ended up in the theological one (Gregory Currie): “forgery is a bad thing”. This, I take it, must necessary have to do with God and one of his commandments, THOU SHALT NOT LIE!

How silly of me who thought this discussion was mainly a cognitive endeavour!

Anyway, let me try some (probably misplaced) comments on the present paper. First, it looks as if Currie would place the cognitive value we attribute to an art object (whatever that is!) in the human effort with which it has been construed (i.e., “[…] the painstaking work of Leonardo […] vs. […] the pressing of a button […]”). This would fit with the general idea that considers elaborativity a necessary condition for producing artistic experiences (see, for instance, Ellen Dissanayake, 2000: What is Art for?). In which case, some experiences, like a sunset, a landscape, a living being, and so on, could never be experienced artistically.

Secondly, although the author tries to show the difference between what happens in literature and in plastic arts, he ends up by saying that a good copy may produce the same aesthetic experience as the real thing, as a novel we may read in handwriting, hard back edition or pocket edition. I confess I am unable to understand what he tries to say, but he has indeed made me think of something, maybe indirectly. Let me explain.

One of my most impressive artistic impressions was when I unexpectedly discovered Mark Rothko’s paintings in the Tate Gallery, ages ago. Another strong one, happened when I spent a few days in a Picasso retrospective in Paris while he was still alive. I have also had similar experiences with Velázquez, Tàpies and Paul Klee. But, as far as I am aware, I never had them when looking at reproductions of works in good art books. In that case, all I know is that I can’t stand Dalí or Chirico, but I enjoy Magritte. I also know that I value highly Vassarely and Barcelo, but I don’t pay attention to Botero or Constable.

I can’t explain this double phenomenon: (1) I only have strongly experienced art when pictures were in the right environment (i.e., a museum or an exhibition). I don’t know whether I would have had it if the pictures had been exact copies (or fakes!). My guess is that I would.

(2) Although I have seen hardly a few or even none of some of the other mentioned artists’ paintings, but only copies of them (and not even perfect copies at that), I am quite sure that my positive or negative evaluations concerns those artists’ works.

My question is: what are the cognitive relationships between a strong artistic experience and may personal tastes? Do copies, fakes, originals, and whatnot have a relevant role in those instances? Would the environment (or rather, the mental construct we call context) have to be considered as well?

Questions, questions ...

> **The Theory of Museum Finish**
> Richard Minsky
> 23 oct. 2004 11:38 UT
>
> The questions you raise do bring up several issues surrounding the nature of the aesthetic experience, value systems, and perception. In the mid-70’s, while teaching at The School of Visual Arts in New York City, I developed a 15 week curriculum titled **The Theory of Museum Finish** that addressed these questions. It drew on many sources, including Koestler’s *The Act of Creation*, and was intended as an analytical tool for creating art. It also explains why a

reproduction in a book does not create the aesthetic experience you have when viewing original works in a museum.

It approaches the question, “why do great works of art in the museum seem to shimmer, or vibrate the space around them.” The applied theory (it is a studio course) is that the artist balances the power of the object, the image and the metaphor. The viewer sees the object (stone, paint on canvas, etc.), is taken by the image (a scene or whatever) to a metaphor, which is an internal experience relating to the viewer’s accumulated history (which may involve genetic and cultural parameters). The strength of the material brings the viewer back to the object (it’s just stone, or paint), and the cycle repeats. The mind (attention) of the viewer flickering back and forth from the internal to the external experience of the work acts like the shutter of a movie projector, creating the illusion of flickering space.

The students are encouraged through a series of exercises to create works with “Museum Finish” that cause this effect. In the process they learn color theory--not the Itten, Albers, Goethe sort (which they learn in other classes), but color meditations based on the chakras, color healing, color stimulation of the endocrine system. This helps them to manipulate the biological, emotional and consciousness states of the viewer.

They also produce examples of good work without “Museum Finish.” That includes work with strong object properties and image but not metaphor, which is *Decorative Art*, and work with strong image and metaphor but not object properties, which is *Illustration*.

The above is greatly simplified, but presents the notion that your art books do not give the same aesthetic experience because they are illustrations. For example, the great magic of Degas is that as you approach the canvas the image dissolves into a beautiful, glowing use of paint as paint. That does not show in a halftone illustration.

Comments on Minsky's responses
Jose Luis Guijarro
23 oct. 2004 19:50 UT

Let me ask you a few questions, Richard:

If “it is both an authentic original work, and a fraud, because it is not true”, are we not moving in two worlds of interpretation? The way I understand it is that, when considered “an authentic original work”, we look at it from the point of view of the (possibly distinct) information processing manner which the “artistic” object is supposed to require; a manner that would resemble (loosely, but since you mentioned him …) Koestler’s AH! sort of reaction. This way of looking at it is specific to the art phenomenon.

However, when we look at it as “a fraud. Because it is not true”, we abandon this “art frame” and plunge into a social (or as I said facetiously above, theological) world in which there’s no difference between imitated art objects and, say, forged bank notes, husbands’ lies, doctors’ impersonations or faked chemises lacoste. Here we may indeed talk about “bad” behaviours, but does it really matter? What I was trying to say all the time is that, if we are moving in the art domain, this second point of view is really not interesting in determining what the difference might be between an original artistic object and an imitation of it.

You say further that “imitation is a necessary condition of both forgery and reproduction”. It’s funny: I would perhaps agree if the sense of “reproduction” was linked to biological terms. After all, we may interpret life as a constant genetic imitation with imperfect copies here and there, some of which ameliorate the “original” model and make life evolve. But since I don’t think you had this context in mind, I don’t really know whether you still think imitation is always as bad as when trying to pass forged dollars to absentminded people.

Finally, you give us a sketchy account of the Museum Finish idea in which there are very interesting points. I have some difficulty in following the argument, though. Would it be possible to have some expansion of it?

For instance, I would like to know what you understand by “the power of the object” and what do you consider to be a “metaphor” (of what?). Are there describable cognitive relationships that show how they function the way you say they do?

Let me explain why I am so interested in those ideas:

I have never seen the Giotto’s pictures in Assisi; but this last summer, in Paris, there was an exhibition of those paintings (or rather, of their life-size and very accurate photocopies or whatever) at La Sorbonne. The effect was astonishing, very, very, VERY attractive, I spend almost a couple of hours there. But I failed to have the strong experience I had with Rothko in the Tate. Somehow, I have the intuition that, considering the real beauty of the pictures exposed in Paris, if I had seen them in Assisi, I would perhaps have approached the strong emotion I felt with Rothko. I cannot explain why, but maybe your idea of Museum Finish might be a possible way to do it.

So, please, it would be very much appreciated if you gave us (or at least, gave me!) a more extended description of it! Thanks!

Reply to José Luis Guijarro’s comments
Richard Minsky
27 oct. 2004 21:21 UT

I’ll try to answer your questions, José.

The first one comes from the other thread, and regards works created in a period style that are unique (in the sense of not being copies of existing works). You suggest that my notion that it is simultaneously original and a fraud moves between the worlds of art and society, and that these interpretations are somehow not compatible.

One of the things I was pointing out is that art during the XX century fuzzed the boundaries between disciplines, or “worlds.” The concepts of artistic intention expanded to include economics, philosophy, social commentary, etc. The art factory replaced the hand of the artist in some works, and the questions of originality of image and attribution became the subject of certain movements. So my question was whether a creation that would be considered fraud in the social sense you mention (like forged banknotes) becomes something else when consciously done as an artistic gesture.

I do believe that the “imitation” of Renaissance style done today produces a different work than one executed then, even if the artist mixes the pigments and media by hand according to old recipes, and uses hand-woven canvas. The concept behind the work is different. The artist brings knowledge of the intervening centuries. The intention is different. And the molecules are not that old, so it is chemically different no matter how you try to make it the same.

The second issue you raise is about my comment that “imitation is a necessary condition of both forgery and reproduction”. I didn’t say imitation is always “bad.” To the contrary, Holbrook’s performance was excellent. Your notion of biological imitation being different from artistic imitation is interesting. I am not sure about that, or even whether either is different from a photocopier. Perhaps we can table that discussion until December, after the conference here on Mirron Neurons (see http://www.interdisciplines.org/mirror for details).

The final questions you raise are about my *Theory of Museum Finish*, and whether it explains why you did not have an aesthetic experience with the Giotto reproductions, but felt you might if faced with the originals. Perhaps it was that the illustrations of the works lacked the surface treatment that gives the originals “Museum Finish.”

It also may be that you are sensitive to the vibes that only exist in the original objects touched by the artist. Some people feel the vibes and others don't, or are not conscious of them. Are those vibes transmitted through the ordering of the molecules in the Work? Do the subtle manipulations of matter create the vibrations like a stylus on a vinyl record, or a laser on a cd? Or do they get transmitted through the unconscious using materials that are not on the periodic table of elements? Do cords of energy connect the unconscious to objects through time and space, such as those called Aka in the Hawaiian system of Huna?

Reply to Guijarro's other question: More on The Theory of Museum Finish
Richard Minsky
27 oct. 2004 22:30 UT

Regarding *The Theory of Museum Finish*, José wrote: *I would like to know what you understand by "the power of the object" and what do you consider to be a "metaphor" (of what?). Are there describable cognitive relationships that show how they function the way you say they do?*

The phrase I used is "the artist balances the power of the object, the image and the metaphor." This means that the artist pays attention to the relative intensity, presence, force, and attraction, of each of these three elements. The "power of the object" comes from its visual and tactile qualities, the nature of the material, the method of working it, and the formal properties of its structure. The "power of the image" relates to its subject, composition, and color. The "power of the metaphor" is influenced by the significance of the underlying unnamable concept that is referred to by the Work, its universality, and its relevance in the culture of the intended audience.

I use the word *metaphor* as it is normally used in literature or visual art, as that which is used to suggest or denote a subject or characteristic in something else, directly or by inference, without making a comparison.

This system was prepared for practical application as a tool for visual artists and critics. It enables one to judge where one's work is weak while creating it, or to analyze existing works (as when doing critique sessions at art schools).

For example, a sculpture that looks like a tomb, made of plaster and paint, with images and words on it, may cause a viewer to experience emotions relating to their own experiences with tombs, or matters of life and death. The power of the material (its physical presence as substance) draws the viewer to its property of being just plaster and paint—it is not a tomb, it is not that which it represents, but it is physical matter arranged into form by someone—it is an illusion. The image that it represents, a tomb, the pictures on it, and the words, again stimulate personal recollections or emotions in the viewer, and the cycle repeats. This occurs many times per second, creating an illusion of simultaneity, and the characteristic "flickering" or "shimmer" of space around the object.

Sometimes the material itself supports the metaphor. That is an area of particular interest to me. As a bookbinder, I often choose materials that support the metaphor within the text. Examples would be my 1973 binding of *Pettigrew's History of Egyptian Mummies* in strips of linen, which you can see at http://minsky.com/3.htm or the Trilateral Commission's 1975 book *The Crisis of Democracy*, which I bound in 1980 in sheep, gold and barbed wire, at http://minsky.com/crisis.htm .

Erratum +
Richard Minsky
28 oct. 2004 10:22 UT

In yesterday's 21:21 intervention there is a typographical error: "Mirron Neurons" should read "Mirror Neurons."

Also, I did not mean to imply that the “art factory” was a XX century invention, nor that earlier art excluded social commentary. Rubens and many others had factory-like studios. Social commentary and philosophical notions appear in artworks of all periods.

The intention of my comment, which goes back to John Zeimbekis’ thread, was that a copy, whether a forgery, a reproduction, or a work “in the style of,” may also be an original conceptual work of art, and, in terms of Greg Currie’s paper, may not be “a bad thing.”

Greg concludes “The creation of the copy is not the creation of any work; it is the creation of a copy of an existing work. The aesthetic equivalence between original and copy is not the aesthetic equality of two works. Rather, it consists in the fact that the copy is as good a means of appreciating the aesthetic properties of the work as the original canvas is.”

If the copy is an “appropriation” of the work, it is a new work. Is there anything to distinguish the copy from the original other than the word of the copy artist? If the original work was made on a photocopier and the copy artist used an identical machine, is the copy artist in a philosophically untenable position? How about a photograph of a photograph? Is the word/intention of the copy artist all that matters?

Getting back to “the original canvas,” José’s point was that he did not have a powerful emotional experience with the Giotto reproductions. I continue the position that the copy does not contain all the information of the original canvas, no matter how good the copy, because it lacks “museum finish,” which was applied by the hand of the artist, and includes such factors as the microscopic manipulation of paint at the brush tip and the chemical composition of the paint. In the process, the artist transmits subconscious information to the Work, whether it is through the alignment of paint molecules or through other energies. Those energies may not have quantifiable English nomenclature, may be like the Hawaiian “Aka,” and may be involved with mirror neurons.

The effects of time also make the original different from the copy. Time is a dimension.

Copies and power and mind I.
Ana Leonor Rodrigues
21 oct. 2004 12:41 UT

The classical copy, leaves me indifferent, if not amused, as it creates problems in a sphere which is not exactly the artistic or the aesthetical one. It creates a problem to the specialist, to the as abused owner or mislead, to all the systems that surround and profit from art and the artist and displace the real subject of artistic creativity and experience to an economic or ethic problem. It amuses me because it disturbs areas of power that are seldom touched by anything.

One mind stealing from another mind, is more disturbing, particularly to a mind shaped in the Occidental culture. Authorship is still a question of recognition and power, we abhor to be dissolved in the group and need individual recognition, and this need of the mind to get recognition for its products is what interests me as cognitive/psychological puzzle; where is the mind-group and where is the mind-individual? Are conscience and recognition so inter-webbed or is it a cultural question?

Replicative forgery

John Zeimbekis (Université de Grenoble)

(Date of publication: 2 November 2004)

I argue that there is no distinction between allographic and autographic representations. One consequence of this is that replicative forgeries have the same aesthetic and artistic value as originals, and are accurate records of actions. I end with some reflections on the pragmatic structure of forgery.

I.

In *Languages of art*, Goodman formulates what he calls 'the critical question' with respect to authenticity: is there an aesthetic difference between two pictures if I cannot tell one from the other? Or, is it possible that some difference I do not discern by simply looking at two pictures constitutes an aesthetic difference between them? (Goodman 1990, 138) I agree that this is the critical question concerning replicative forgery, in the sense that aesthetic, ontological or semantic considerations of authenticity should be guided by a requirement to answer it. Otherwise, the issue will no longer be the authenticity of works in particular as distinct from, e.g., objects auctioned because they are the personal effects of personalities. The bottom line for grasping this difference is that among the properties an object happens to have, not all are relevant to its functioning as an artefact, or else some may allow only exapted functions.

How does this question square with the well-known distinction between autographic and allographic works and arts? The distinction claims that a work is "autographic if and only if the distinction between the original and the forgery of it is significant", allographic if otherwise (Goodman 1990, 147). This would be circular as a definition, not to mind an explanation, of why forgery matters and is possible in some areas: it does not answer the 'critical question' above, and does not tell us why fakes matter. What we need is to know *whether* the distinction between original and forgery is significant. The autographic-allographic distinction is in fact a heuristic device that leads to an inquiry into density and discreteness in representation, which is Goodman's main concern in the area of authenticity and replication. If the difference between original and forgery turns out to be significant, then there is an aesthetic difference between two pictures even when I cannot tell one from the other. If it is not significant, then there is no aesthetic difference.

Specifically, if the inquiry can be used to claim that forged paintings do not have the same value as originals, the claim will have to be grounded on a denial that original and fake are *synonymous*, since according to Goodman all functions and cognitive effects of works either boil down to meaning or can be described as forms of meaning. This is not quite as restrictive as it sounds. *Languages of art* stretches meaning beyond what we have in mind when we take natural language semantics as our model, to include such things as demonstrations, exemplifications and various other unconceptualizable and 'je ne sais quoi'-type experiences (as Leibniz called certain nonconceptual categorizations), which we want the forgery to be able to cause.

Beyond this, we can break out of the restriction to meaning in another way. I will try to capture it with an imaginary example. If I attempt to make Madame de Recamier's portrait look ugly by variously tweaking her facial features, I will necessarily change the picture's meaning: on any attempt, the picture will *mean* different facial features. But the converse does not hold: although I change the features, I might not manage to turn a beautiful face-picture into an ugly one. Changes in perceptible shape do not necessarily result in changes at the level of aesthetic or stylistic properties, but they necessarily result in changes of meaning. In other words, synonymy (replicability of meaning) is a good test case for replicability of other functions, such as aesthetic and stylistic functions.

II

Now, let's try to answer the question whether the difference between original and forgery is significant—given an extremely wide definition of meaningful syntactic difference, and therefore an extremely strict definition of synonymy. The closest Goodman comes to substantially answering the question is his denial that pictures "not only practically but theoretically" can be finitely differentiated (Goodman 1990, 172). Put in the simplest terms possible, this is a way of stating the familiar idea that 'in a picture, any change matters'. But for present purposes, we have to understand finite

differentiation a little better than this. Finite differentiation can be thought of as the existence of zones of indifference within which type-indifference can be established for the purpose of grouping pictures together under a single value (in the same way that we can group graphic variations of "EXIT" under a single type as synonymous). If we cannot make such groupings, no two pictures will be strictly equivalent.

As I see it, two obstacles prevent the absence of finite syntactic differentiation for pictures from amounting to a stricture on synonymy. One militates against Goodman's claim that synonymous pictures are theoretically impossible, the other against the claim of practical impossibility. Here is the first. On a substantivalist conception of space, for any two *given* points that do not coincide, there is a region of space between them occupied by points. To apply this to pictures, imagine that two outline drawings A and B with different sizes are superposed on a sheet of paper, so that different possible outlines can be drawn in the space between their outlines. Then take any outline picture C between the two; this will be different to both A and B, allowing (on the same principle) a picture D which is between A and C and a picture E between B and C, and so on. Differentiation of the possible pictures into picture-*kinds* will be regressive as long as any two pictures are different by stipulation. However, none of this entails that two numerically distinct outline shapes on different sheets of paper *cannot* have identical shape or length—independently of how, or whether, they are perceived or measured. Therefore, lack of differentiation into types does not entail theoretical impossibility of replication. If replicability is claimed to be theoretically impossible, it will have to be for reasons of a different kind to those Goodman claims.

The second obstacle is epistemic, and more practical in scope. We have to be able to describe any differences between the spatial properties of pictures as *syntactic* differences. The concept of a syntax is already epistemic and implies that spatial differences can be picked up as such by a system, so that they can be converted into differences of function or value. Either we can think of a system as something that gives in to the lack of differentiation which putatively characterizes the ontological level, in which case there will be no meaning and no system; or we can think of the system as picking up information, which means picking up segments coarser than anything we can imagine slicing logically. Thus, in addition to the theoretical feasibility of replication, two numerically distinct pictures will *not* necessarily have different meanings. This does not fall foul of the non-transitive nature of indiscriminability. Since in replicative forgery we are concerned with the forger's perception of two pictures, one of them the original, we are in the case described by McDowell as indiscriminability "from the indicated sample", and not in the case of indiscriminability "from something else that counts as having" the same properties as the indicated sample (McDowell 1996, 170). Indiscriminability "from the same sample" has also come in for criticism, on the grounds that it cannot serve as a ground for individuation of concepts (Dokic & Pacherie 2001). But in the case of replicative forgery, we do not need to individuate concepts: all we need to do is ensure identity relative to a context.

It may appear that problems raised by fineness of colour grain could be solved in a way similar to the problem raised by fineness of spatial grain. However, it is possible that there is no objective (physicalistic) sense in which two colours are the same, in the way that two lines can be said to be the same length or shape. If this is so, then the argument presented in favour of the theoretical possibility of replication does not apply to colour. On the other hand, the epistemic points concerning perceptual discrimination and identity relative to a context still apply.

Neither the epistemic point, nor the previous ontological point, redresses the lack of finite spatial differentiation in pictures that Goodman points out. Instead, it is just that lack of finite differentiation does not imply lack of replicability either in principle or in practice, albeit for different reasons. (I stress that non-finite differentiation itself remains intact, because it is also necessary for the specific kind of meaning pictures can possess.)

III

Once the obstacle to forgery presented by non-finite differentiation is overcome, many other obstacles collapse: those obstacles presented by the replication of any properties dependent upon the perceptual traits of the object. This includes aesthetic properties (for example, the gracefulness of a sculpture of the Three Graces) and stylistic properties (for example, the kind of gracefulness that is shared by a given series of frescoes of the Three Graces by the same artist, or by his assistants).

Changes in such properties supervene on changes in perceptual traits: no aesthetic or stylistic properties can change without changes in the picture's perceptual traits, taken as the base on which the former properties supervene. (See the Madame de Recamier example in section I.)

This allows us to re-draw the basic conceptual distinction concerning forgery. There are no autographic arts or representations, and therefore there is no sense in talking of allographic arts or representations either. Instead, there are just dense and discrete representations. Now to the 'critical question' of whether there is an aesthetic difference between two pictures if I cannot tell one from the other. Here, the answer has to be that there is no aesthetic difference. There is probably a difference in shape between the two pictures (in outline, occlusion and shading shapes, barring some extraordinary coincidence), but it is not a *syntactic* difference, so it cannot prevent synonymy, and it is not a perceivable difference, therefore it cannot prevent both pictures from causing the same aesthetic and stylistic effects.

IV

What kind of terminology could capture the relative status of forgeries and originals? Perhaps appropriate concepts can be found in another area that analyses artefacts: philosophy of language. An initial, but inadequate, approximation consists in describing an original and a replicative forgery as different tokenings of the same work. This is true, but, as it stands, unfair to the forgery. A replicative forgery is a perfect *record* of the original work taken as the result of a sum of actions carried out by an agent in a given situation. For it to be a perfect record, it has to exemplify all the formal properties on which representational functions, aesthetic effects and art-historical evaluations can supervene, something we have tried to ensure. But if the forgery works as a record of an action, then it is not just an occurrence coming under the same *type* as the earlier token, but something *closer* to the original tokening—since a tokening of an expression is, precisely, an action. This can make it tempting to describe the forgery as a quotation of the original work. Whether forgeries of paintings are pictorial quotations depends on whether we take quotations to exemplify types or tokens. I will assume that at least some quotations exemplify tokens, and therefore refer to a prior tokening. Then, presenting a forgery is like saying (1), but it is not like saying (2):

(1) In the context of production, the artist did this '___'.

(2) After you left, he made this gesture '___'.

In both cases, '___' *is* a case of the same gesture previouly made. But in (2), '___' also refers to the prior occurrence of the gesture. In (1), it is clear that '___' cannot refer to what the artist did: for the replicative forgery to refer to the original, the forger would have to admit that the forgery is not the original. Of course, the forgery can be *used* to make a quotation in the full sense of (2): reference plus replication. But if it is so used, the forgery no longer functions as a forgery but as a replica. Therefore, what defines replicative forgery is the highly unusual pragmatic predicament in which the forger finds himself. Although he presents an object that is a quotation (that is type-identical to a prior token and refers to that prior token), he has to present the object as if it *is* that prior token.

I hope that this helps to clear the moral ground of objections to forgeries. Objections to replicative forgery cannot be based on a claim that a forgery necessarily produces either inaccurate records of actions, or somehow inferior aesthetic experiences. So they have to find some more basic ground, independent of artistic and aesthetic value.

References

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Discussion

Forgery and quotation
Gloria Origgi
2 nov. 2004 12:00 UT

I don't understand clearly what John Zeimbekis has in mind when he says that forgery may be described as a pragmatic act of « quotation ». How can this help us to answer to the main question of the paper, that is, whether there is a difference between forgery and replication? In the linguistic case, one can quote various aspects of an utterance, its meaning, its intonation, the accent with which it has been pronounced…. The case of the "perfect" quotation is as ambiguous as the one of the perfect copy, and I don't think it solves the problem. There is a sense in which Van Meegeren quotes Vermeer and another in which Picasso quotes Ingres. I have the feeling that the pragmatic of quoting is even more complex of that of forgeries, hence it is not a good *explanans* for the case we are discussing.

> **clarifications**
> John Zeimbekis
> 2 nov. 2004 13:40 UT
>
> I think this misses my point. I agree that it would be extremely difficult to replicate an utterance, even if criteria of type-identity could be found. But my point is that paintings can be replicated as to all the properties we seem to consider relevant (meaning, aesthetic properties, style). The pictorial *equivalents* of the aspects of a linguistic utterance mentioned by Gloria Origgi (meaning, intonation, accent)—ie, the representational, aesthetic and stylistic properties of paintings—are replicable.
>
> Gloria says, "How can this help us to answer to the main question of the paper, that is, whether there is a difference between forgery and replication?" I answer this question in sections I-III. The discussion of quotation does not claim to be an 'explanans'; the explanans is given by the arguments further up.
>
> Besides, I do not clam that replicative forgeries are quotations, only that being replicas, they can be *used* to pictorially quote (and that if they are so used, then they no longer work as a forgeries because they have to refer to the original, and blow their cover).

Some problems
Jose Luis Guijarro
6 nov. 2004 17:19 UT

I hate to admit my lack of intellectual level to fully understand John Zeimbekis' paper. Therefore, my message will be probably stressing the wrong issues. But seeing that, after a week, nobody has anything to say about it, I hopefully like to think I might be travelling in company after all.

The idea of a replica as a sort of quote seems, at least, original and, probably, worth thinking over. However, I think I understand Gloria's misgivings about such a metaphor and John's clarification insisting on new metaphors (i.e., "meaning" à "representational", "intonation" à "aesthetic", and "accent" à "stylistic properties") strikes me as being too loose to have really some sort of descriptive power.

I am able to make sense of the "meaning/representational" pair, though it may need some further refinement to make it totally accurate. But I can't see any relation, apart from John's postulating it, between the two other pairs. In what sort of world would the concept INTONATION be coupled with AESTHETIC, I wonder? And how could we think of ACCENT as a STYLISTIC property?

The trouble with the first of two these pairs lies in the quite accurate meaning of INTONATION in Linguistics and the evanescent meaning of AESTHETIC. In my mental world, the concept AESTHETIC is related to beauty (not necessarily artistic), but I have seen it used in a strict artistic context, referring to the art experience itself. If, as Norman M. Weigenberg suggests in a Scientific American paper, "Music and the Brain" (October 25, 2004), some people who " (…) can make no sense or recognize

any music (…) cannot distinguish between two melodies no matter how different they are. Nevertheless, [they have] normal emotional reactions to different types of music", this would imply, at least in my mental world, that AESTHETIC experiences are pre-cognitive. Does the coupling of these two concepts, INTONATION and AESTHETIC values refer to that precognitive state?

But although I have found a possible meeting point for that pair of concepts, I am totally at a loss with respect to the last one. In my world, the concept STYLE refers to the way I willingly present something to the others in order to share it publicly. An ACCENT, on the other hand, is hardly a matter of willingness. So, what's the possible link here?

As I said, these are probably the wrong comments to offer in this discussion. But, I insist on my poor understanding of John's … STYLE (?!) … in presenting his ideas. My interested suggestion would be to ask kindly for a schematic account of the relevant issues and their development in the paper, phrased in a less specialised language. But I am aware that this might be asking too much after all.

Reply to Jose Luis Guijarro
John Zeimbekis
7 nov. 2004 22:24 UT

First of all, Jose Luis, thanks for taking the trouble to read paper and responses critically. On some of the points you make I agree, not on others. I agree that the three things (meaning, intonation, accent) would not correspond one-to-one to (representational, aesthetic and stylistic properties). Simply, Gloria Origgi mentioned meaning, intonation and accent as various aspects of an utterance that one can quote. Accordingly, I mentioned repesentational, aesthetic and stylistic properties as various quotable aspects of a work of art. I don't mean that the concepts correspond one-to-one, only that the latter list are quotable aspects of a work of art.

For linguistic quotation, we seem to take the bare minimum to be identity between the quoted sentence and the quotation, that is, sentence-identity. 'Thanks a lot' gives 'He said "Thanks a lot"'. But this doesn't tell you that he shouted it sarcastically while waving his fist, which are all relevant aspects of the utterance.

My point was that a replica of a painting is necessarily richer in this respect than a type-identical sentence quotation. As I argue in the paper, if there are no changes in the formal traits of a painting that give it its meaning, there cannot be any changes in its aesthetic properties either. In language, there is no such relation: I can say 'Thanks' sarcastically, gracefully, clumsily, softly or gently, but there seems to remain a standard ('literal' or 'type') meaning for 'Thanks' however I say it. Now think of a portrait. If you make a pretty face look insipid, you will change its features. Say you change the edges of the mouth. Then, the picure will also have a different meaning—not 'mouth until here' but 'mouth until there'. So if I want to quote the aesthetic and stylistic properties of a pictorial work, a replica will do the job well.

However—for the record—quotation is not the main point of my paper but a corollary. The main point is that the meaning, the aesthetic properties, and the stylistic properties of pictures can be replicated, and that therefore I can see no difference between the aesthetic and artistic value of a painting and its forgery.

Interesting issues
Jose Luis Guijarro
8 nov. 2004 12:24 UT

Interesting issue

That's the way I understand what a "debate" should be like! When people react to what one has written, the proper thing to do is to answer back and try to clarify issues that have been erroneously criticised or poorly understood. I am, therefore, very thankful to John Zeimbekis for taking the trouble to answer to what, in all probability, he may think is not a very important aspect of his argument.

Two things seem to be of interest here. The first is that, as he states in the last paragraph of his response, there are “no differences between the aesthetic and artistic value of a painting and its forgery”. I have always thought so, although I am unable to couch my thoughts in such a learned and stringent chain of arguments as he seems to have done. I really will try to decipher what, for me, is almost a hieroglyphic text, for I want to be aware of his ideas.

The second thing is a little more problematic: i.e., the idea about “quotes”. Granted that, after reading it, and acknowledging Gloria’s misgivings, I thought it could begin to explain why my reaction to Giotto’s paintings (in life-size photocopies) which I saw last Summer at La Sorbonne in Paris, although very pleasing and interest triggering, was certainly not what I presume would have happened had I seen them in Assisi. “Of course”, I said to myself. “They are QUOTES from the original paintings and, as such, they produced a different cognitive reaction”.

But on second thoughts, I began to wonder whether that was really a possible incipient explanation. Is it not a mere terminological interchange between the word “copy” and the word “quotation”? I might as well have said: “They are COPIES from the original paintings and, as such, they produced a different cognitive reaction”.

If that is so, then, unless I finally decipher John’s text, I am afraid I haven’t got the beginning of that explanation I am so eagerly seeking. [Incidentally, in the previous discussion, Richard Misnky proposed another explanation for my problem, namely, the museum finish idea, which I also had difficulty in grasping (maybe, as I seem to lack interpreting power in these matters, I should just leave the discussion through the back door!) … until I saw his binding work (in photographs and in my computer). I don’t know whether or not I agree with that half-understood idea of his, but I think I grasped somehow the context (i.e, mental ground) from where he probably developed it. Unluckily for me, I am no artist, and it is very difficult to put myself in a similar context to be able to fully understand the import of the “museum finish” idea].

The scope of the explanatory tools borrowed from the theory of language
Nicolas Bullot
8 nov. 2004 13:47 UT

Mutatis mutandis, the content of the debate on D. Dutton’s article applies to the discussion of John’s (inverted) claim (see in particular D. Sperber’s and J. Levinson’s comments and their discussion). I shall not ‘replicate’ the debate. I cannot endorse John’s main thesis because I accept Jerrold Levinson’s thesis*, and that the theses are not compatible. As a result, I understand Gloria’s trouble about the use of the notion of quotation.

My question is in fact about general methodology. Even though John’s article objects to a distinction introduced by Nelson Goodman, I think that John agrees and follows an aspect of N. Goodman’s method, which is to apply some explanatory tools of the theory of language (theory of meaning, pragmatics, notation, quotation etc.) to the analysis aesthetical phenomena. This may explain the high number of linguistic metaphors in his article. I may be wrong however. Therefore: (i) Are you actually endorsing this kind of Neo-Goodmanian approach? (ii) Would you think it appropriate to explaining most aspects of aesthetical phenomena (including causal/historical properties of the artwork and nonconceptual contents of aesthetic experience)?

* For memory, Levinson’s thesis is roughly: “There is some aesthetic/artistic virtue in an original painting that is not possessed by even a perfect, that is, perceptually indiscernible, copy or duplicate of it.” “(…) the original "puts us in touch" with the artist in the way the duplicate cannot, because of the different causal/historical properties of the two, those non-observable, extrinsic, relational properties to which Sperber, Pouivet and Bloom allude in different ways.”

the tools are not borrowed from language
John Zeimbekis
9 nov. 2004 10:36 UT

Nicolas, you state that Goodman applied linguistic tools to works of art. This is a false. Goodman tried to explain how *non* linguistic symbols could have meanings, and his theory is

one of the few studies of meaning *outside* language—in such areas as analogical representation, exemplification and pictures. Therefore, talking about the symbolizing functions of works (just as we talk about the symbolism of artefacts from alien cultures) does not imply any linguistic analogy. Of course, even if you approach visual arts with the most open mind imaginable, there is a chance you will find *common* foundations for meaning: for example, a simple value of reference. Finally, Nicolas uses the expression “neo-Goodmanian approach.” I’m not aware of any such approach currently, and I’m certainly not going to start one. Like anyone else practicing analytic philosophy, I borrow any tools that seem relevant to my problem as long as they have not been discredited for the wrong reasons. In this paper I tried to criticise one of Goodman’s distinctions that has the status of orthodoxy, the allographic-autographic distinction.

Next, Nicolas takes up Jerrold Levinson’s point: a replicative forgery and an original cannot have the same value because they have different causal histories. I’ve already posted a couple of replies to Jerrold (first discussion on Dutton: “Objects and actions”, “Do we ever appreciate the token action”). A quick answer is this: since we do not learn the causal history of the painting by looking *at* the painting, but by learning facts *about* the painting, causal history cannot affect the way we see the painting. The proof is that if I see the original and a replica side by side, I will not know which of the two has ‘the right’ causal history. In addition, the part of appreciation that comes from factual knowledge about the painting wll be borne out by each last detail of the replica. That is why I think a replicative forgery has the status of a record of the artist’s action.

Reply to John Zeimbekis: Symbols?
Maria Rossi
14 nov. 2004 12:17 UT

“Nicolas, you state that Goodman applied linguistic tools to works of art. This is a false. Goodman tried to explain how *non* linguistic symbols could have meanings, and his theory is one of the few studies of meaning *outside* language—in such areas as analogical representation, exemplification and pictures.”

I don’t consider the truth of this claim to be so obvious; maybe one could substitute ‘content’ for ‘symbol’. You have mentioned “non linguistic symbols”. Even though qualified by the adjective ‘non-linguistic’, the use of the term “symbol” strongly suggests considering artworks as symbolic elements which are vehicles of meaning. However, it may be controversial to claim that interacting with (pictorial or abstract) artworks is reducible to interpreting symbols. The literature on nonconceptual content (e.g., Cussins, 2003; Gunther, 2003; Peacocke, 1992) may help refine this debate and disentangle the understanding of the perception of objects of art from the understanding of language. But I cannot enter into the details here.

Cussins, A. (2003). Postscript: Experience, thought, and activity (2002). In Y. H. Gunther (Ed.), Essays on Nonconceptual Content (pp. 147-159). Cambridge: MA: MIT Press.

Gunther, Y. H. (Ed.). (2003). Essays on Nonconceptual Content. Cambridge: MA: MIT Press.

Peacocke, C. (1992). Perceptual concepts, A Study of Concepts (pp. 61-98). Cambridge, MA: MIT Press.

Reply to John Zeimbekis (2): Medium, causal traces and the history of art objects
Maria Rossi
14 nov. 2004 12:42 UT

“Next, Nicolas takes up Jerrold Levinson’s point (...) A quick answer is this: since we do not learn the causal history of the painting by looking *at* the painting, but by learning facts *about* the painting, causal history cannot affect the way we see the painting.”

Objection: I think that in standard cases (without taking into account deviant causal chains) we can learn about the causal history of the art object by perceptually examining the relevant clues. If you repudiate this idea, you are simply going to bypass the role of the medium for any external art representations/objects, for examining the medium allows us to obtaining information about the causal lineage of each token object of art. Overlooking the role of the medium may cause considerable difficulties if one intends to study the art-objects which (i) make use of the principle of jointly manipulating the medium and the pictorial content or (ii) abandon any pictorial content whatsoever, for instance in the abstract arts. When looking at a painting or a piece of sculpture (or for that matter at any ‘allographic' artistic apparatus) one can pay attention to many traces of its causal history and that can help keep track of its *unique* history.

Some artists have designed experiments involving objects of art that manipulate the traces of the causal history. For example, consider the viewer-interactive “10 x 10 Altstadt Copper Square” by Carl Andre at the Guggenheim Museum, (http://www.guggenheimcollection.org/site/artist_work_lg_3_4.html) where causal modifications are defined by both the work and the spectator who is free to walk across it and thereby is in a position to causally alter its surface in a visible way. Thus, once again, one can perceive some (visible and touchable) traces of past events of its causal history *on* its surface. This does not relate to the symbolic status of the object.

[Carl Andre's sculptures: see http://www.artcyclopedia.com/artists/andre_carl.html .]

From Original to Copy and Back Again

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Abstract:
Historians have been largely silent during the debates on Goodman's claim that one cannot distinguish an authentic from a forged painting by "merely looking". This paper opens the limited dialogue between art history and aesthetics by exploring the ways that forgeries, copies, and originals function within historical and critical discourse. Instead of comparing authentic and inauthentic, I will be comparing imperceptible difference with immediately obvious difference.

Within aesthetics, exchanges about the nature of forgery have been made largely in response to Nelson Goodman's *Languages of Art* (1968). In his original scenario we are invited to contemplate two paintings: one is authentic and the other is forged, but we cannot distinguish them by "merely looking," without the aid of X-Rays and chemical analyses. Our different aesthetic response to the two paintings is used to argue, in the words of Joseph Margolis, that "there are bound to be aesthetically relevant considerations in the appreciation of art that are not directly accessible to perception or to any other relevant mode of experience."[1] Historians have been largely silent during the debates on Goodman's claim: not, I think, because it is not an important problem, but rather because what matters to historians are the imperceptible considerations in any given case, rather than the simple existence of such considerations.

Here I would like to open the limited dialogue between art history and aesthetics on this subject by exploring the ways that forgeries, copies, and originals function within historical and critical discourse. I hope to show that the art historical way of ordering such concepts intersects the distinctions made by aestheticians in a mutually illuminating manner. A useful step in this direction has been taken by Crispin Sartwell, who has questioned the assumption that it is possible to distinguish authentic and inauthentic artworks.[2] In place of the dichotomy authentic / inauthentic he has proposed a twenty-one-step sequence of works from those that are most clearly inauthentic (and to which we would want to attach the label "forgery") to those that are most clearly authentic.[3] The list, which need not be repeated here, begins with copies that are represented as originals, and ends with paintings that have been cleaned. Enroute it passes through such problematic intermediaries as the "masterful copy of a particular painting to which a [forged] signature has been added by another hand."

What I would like to do here is propose that Sartwell is not quite correct in historical terms when he says that "the authentic and the inauthentic are continuous with one another." The fact that "cases can be adduced to any desired degree of intermediacy" (Sartwell's next sentence) does not imply that there is a mathematical continuity between the poles of authenticity and inauthenticity. I agree that it does not make sense to speak of authenticity as a well defined term, but I think that the path from one to the other is made in discrete steps. Those "steps" are historically determined categories and habits of thought. Such criteria are relevant here since history has given us the terms "authentic" and "inauthentic" to begin with, and it is therefore plausible that it has given us other terms as well. To rephrase Sartwell's claim: there are only a few "degrees" that we know how to "desire," and historical practice has given us their names. Sartwell's twenty-one steps, in other words, cannot be made infinite upon demand.

The particular sequence that I will propose has a second difference from Sartwell's continuum and from Goodman's dichotomy. Instead of comparing authentic and inauthentic, I will be comparing imperceptible difference with immediately obvious difference. The reason for this change also lies in the realm of historical response. In most cases, historians and critics do not find themselves wondering about nuances of forgery: the intention to deceive is far less common than the intention to emulate.[4] Goodman's problem, of seeing the difference between two apparently identical paintings, may never have taken place. Even when Van Meegeren's paintings were thought to be Vermeers, there was no single painting that matched any of Vermeer's authentic works. In saying this I do not mean that Goodman's problem might not be a good test case: simply that to achieve it, he has to leave the realm of history. The history of art is marginally concerned with forgery, but centrally informed by the progression from the student working "in the manner" of the master to the

independent artist working “in his own manner.” Questions of the aesthetic component of imperceptible differences arise whenever one looks at a second- or third-rate picture, and wonders if it is sufficiently different from its models to be experienced as an original in its own right. Forgery, in this view, is a special case of all artistic work, a perversion or failure of the normal process of increasing distance between student and master.

So I wish to point out that forgery itself may be defined as a special case of a more general phenomenon, and that the sequence from original to copy is not continuous but proceeds in historically determined stages. My third contribution to this question is to point to the fact that the sequence forms a loop. Original and copy are not well-defined terms: not only because they are linked by intermediaries, as Sartwell says, but because they contaminate one another by forming a cycle that repeats through history. We can begin with the “given term,” the original work itself.

1. In art history, an *original* work may or may not be an authentic work. The *Virgin of Vladimir,* an early Russian icon, has been overpainted at least five times in the last seven centuries. Art historical texts tend to assign the concept of originality to works that possess one or more of three further properties: originality (which I will call *originary* to avoid confusion with the general term *original*), primacy, and uniqueness.

Originary works are those that appear to be without antecedent. The smile in Leonardo’s *Mona Lisa* (1503-05) inaugurates a tradition of enigmatic smiles from Leonardo’s pupil Bernardino Luini onward (as in the *Madonna of the Rose* in the Brera in Milan), but Mona Lisa’s smile can be understood as part of a tradition that goes back to Leonardo’s teacher Verrocchio and ultimately to archaic Greek sphinxes (6th c. B.C.).[5] Note that originality is constrained by the contextual uses of tradition, and not by a fixed definition. If we search for the history of polka-dots, we can trace them to a nineteenth-century dance, or all the way back to panthers.[6] According to George Steiner, whatever is *primary* refers mostly to itself, and whatever is secondary refers mostly to what is primary. Hence Shakespeare is primary, and Shakespeare criticism is secondary.[7] An originary work might not be primary, and vice versa. *Uniqueness,* the third nonessential property of originality, distinguishes originals clearly from certain kinds of copies, but only blurrily from most terms we will be discussing. None of the three properties of originality taken individually is sufficient to distinguish originals from copies, and we will see traces of each idea in the categories that follow.

2. Next are *strict copies,* works made in exact emulation of originary works.[8] In the strictest sense the strict copy is a modern invention. Baroque copies were seldom line-for-line, and Baroque academies had a well-developed aesthetic of copying that did not involve “photographic” copywork.[9] A *reconstruction* is a particular kind of strict copy, in which the work is made again in the same way it was originally made, perhaps including layers of gesso, *imprimatura, grisaille,* body color, glaze, and varnish. It need not look any different from a strict copy that only mimics the surface layer of paint (as is done in contemporary conservation). The different aesthetic responses provoked by reconstructions and strict copies mirror the responses provoked by Goodman’s original and forgery, except that the successful reconstruction casts doubt on the original by making us wonder if all the layers were really necessary.

Given the large literature on forgeries[10] and the extensive investigations of the Van Meegeren case, [11] I will make only two points here regarding forgery. First, though it is true that in this sequence most forgeries are strict copies and reconstructions, pictures called “forgeries” can also be found among the looser copies we will consider below.[12] Forgery, in effect, is dispersed through several categories and loses its primary value: as is appropriate for a category that is historically marginal in comparison with originality and copying. And second, the detailed historical study of forgeries might augment Sartwell’s twenty-one examples.[13] Much of this literature has gone unnoticed in the philosophic debates, though some philosophers have stressed the historical aspect of our understanding of forgeries.[14] The historical material demonstrates that the problem of forgery is seldom equivalent to the problem of achieving an indistinguishable strict copy. For example, a strict copy might refer to a forgery, without being a forgery. Adrian Kovacs has made copies of Cézanne’s self-portraits, which he calls self-portraits of himself.[15] But the artist’s advertisements show that he refers to forgery rather than exemplifying it. This is like the difference between the word forgery and the word “forgery,” and it opens the possibility of reorganizing the topic of forgery in terms of quotation rather than intention or authenticity.

3. A *reproduction* is a copy that is readily distinguished from the original, but is meant to reduce or avoid an appearance of originality. Hence reproductions are works that are taken to have the same absence of originality that strict copies have, but with an appreciable formal and aesthetic distance from the originals. A reproduction is therefore one step further from an original than a successful strict copy. In art criticism there is a well-developed dialogue on originals and reproductions, and among its claims is the idea that the "age of mechanical reproduction" has served to bring out the concept of originality latent in previously "unreproduceable" works.[16] Reproductions can be mechanical or hand-made, and that difference can be used to modify the reference to originality. An example of the former is Sherrie Levine's photographs after Walker Evans photographs. By rephotographing from copies of Evans' negatives, Levine avoids the issue of skill and stresses the theme of copying: as she sees it, everything is a copy, and the world is "filled to suffocating" with copies of copies. The open, obvious, unskilled nature of her copies is meant to underscore the universal impossibility of originality.[17] Handmade reproductions may be exemplified by Mike Bidlo's paintings of Picasso paintings. Since he actually paints them, his works are more complicated than Sherrie Levine's (and arguably also more aesthetically confused): he also refers to the skill and the genius that went into Picasso's works as well as to the difficulty, irrelevance, undesirability, or unimportance of originality. A work such as Bidlo's *Not Duchamp* (1984), which is an assemblage designed to mimic Duchamp's earliest ready-made, the *Bicycle Wheel* (1913), shows how complex these references can be.

Some reproductions approach or depart from originals in terms of quantity rather than skill or quality. *Multiples* and *limited editions* are sets of objects that preserve the idea of the original's uniqueness. Joseph Beuys's multiples have this double reference: in part they are anonymous works of machine production, and in part they are originals. In paleography this is known as the difference between signatures and autographs. The latter are unique, and are by the signator's hand; the former can be reproduced or signed in absentia, by contracting a surrogate signator. Ottoman *tughras,* elaborate calligraphic Sultans' signatures, were drawn by calligraphers and sometimes also signed in autograph with the calligrapher's name, as one might sign a painting.

4. *Imitation* is a word introduced by the poet Robert Lowell; he used it to describe his poems "imitated" from originals rather than translated in the usual sense.[18] Part of the concept of imitation is the construction of a new entity comprised of the imaginative fusion of the new and old works or artists. *Emulation* and *free copy* are synonyms, since such works are typically engaged in "conversations" with the original that paradoxically involve alteration in the name of greater fidelity. Imitations evoke the Italian distinction between imitation (*imitazione*) and imagination (*fantasia*).[19] In historical terms, Lowell's position is that a work must be re-created by the *fantasia*, rather than slavishly imitated, in order to be "really" or "truly" true to the original. The opposite to this, and the privileging of *imitazione,* is provided by Vladimir Nabokov's translation of Pushkin's *Eugene Onegin.* Nabokov stresses absolute precision in transcribing individual words and his translation is frequently unreadable; in our terms his is an attempt at a strict copy.[20]

5. *Variations, variants,* and works "after" or "in homage to" an original are allied to imitations but stress divergence from the original rather than the affinity. The catalogue of an exhibition held at Nuremberg in 1980 lists over one hundred variations on Dürer's work, many shown side by side with the originals.[21] Though they span the full range of styles that were possible in Germany in the 1970's, most are primarily variations, as the catalogue suggests: they argue that style and relation to the past are independent. Picasso's works based on Old Masters are the best-known modern examples of variations. He made variations on Velasquez's *Maids of Honor* (1656), Jacques-Louis David's *Rape of the Sabine Women* (1799), Eugene Delacroix's *Women of Algiers* (1849), and others. Caricature, parody, and persiflage (light-hearted parody) are variants because they do homage in an ironic or critical fashion. A pastiche, for example, is an uncomprehending or simplifying variation. Canaletto and others made *capricci* for tourists which are pastiches in this sense: if someone visited Italy and wanted mementos, but could not afford separate paintings of each city, he could buy Canaletto's paintings, which juxtaposed monuments from different places.[22]

6. Moving farther along we come to *version,* which is almost an independent work: almost a second original. A famous example is Manet's *Dejeuner sur l'herbe* (1863), whose original may be said to be Marcantonio Raimondi's engraving of *The Judgment of Paris*.We do not always think of Marcantonio when looking at Manet, but we are invited to experience a divided consciousness: on the one hand, Manet's painting is an original, and on the other, it is not. *Le Dejeuner sur l'herbe* is poised between primary and secondary, imitation and original. It is this ambiguation that is partly the reason

for Manet's fascination to art historians: his works occupy a particularly difficult position in the sequence we have been following.[23]

Several of Sartwell's twenty-one entries belong here (especially numbers 10 through 21). His number 20 includes the states of a print, which are partly determined by earlier states but not necessarily dependent on them. When Rembrandt erased the figure of Tobit and the Angel from a landscape by Hercules Seghers, he made a pure landscape scene out of a Biblical scene. The ghostly outlines of the Angel and Tobit remained, but they are too faint to be noticed except by a close examination executed by someone who knows where to look. Again we meet Goodman's dichotomy in a setting that refigures its meaning: in this case, the aesthetic appreciation of both original and version is changed.

7. We now approach the place where we began, since works that edge the original entirely out of mind approach the status of original. But to be exact, this seventh category is not equivalent to the first category, because the existence of a seventh category (of "originals" derived from orginals, to use the quotation marks I employed in speaking about reproduction) undermines the possibility that the first category might be pure and distinct from those that follow. In light of the seventh category, originals must be redefined as works related to and derived from copies, and copies must be also reconceived as originals *in statu nascendi.* This is historically appropriate and exemplary of the conceptual reduction that is required in order to contrast authentic and inauthentic works.

Though each of these stages suggests further problems, I think that the underlying, and most interesting, question is the conceptual role of history itself, which appears from our vantage to have dictated the terms of the debate itself by providing us with these stages. For artists and aestheticians alike, these possibilities show the restricted meanings that obtain in the apparently trackless domain between original and copy. An artist is not free to work toward independence by achieving "any desired degree of intermediacy." One does not make an original by sliding away from a great work, but by proceeding in stages from apparent slavery to apparent independence, and it is ultimately the interplay of stages in this sequence that provides the richness of meaning that painted traditions routinely achieve. That structure of our understanding, it seems to me, can enrich philosophic exchanges by fleshing out the harsh difference between original and forgery.

This article was first published as "From Copy to Forgery and Back Again," *The British Journal of Aesthetics* 33 no. 2 (1993): 113–20.

Notes

[1]J. Margolis, "Aesthetic Appreciation and the Imperceptible," *British Journal of Aesthetics* 16 (1976): 305.
[2]Another useful text is W. E. Kennick, "Art and Inauthenticity," *The Journal of Aesthetics and Art Criticism* 44 (1985): 3 - 12. Kennick notes that Goodman writes "'forgery,' 'copy,' 'reproduction,' 'imitation,' and 'fake'… as if it made little or no difference which he used" (p. 3).
[3]C. Sartwell, "Aesthetics of the Spurious," *British Journal of Aesthetics* 28 (1988): 360 - 362.
[4]This point is also made by W. E. Kennick, *op. cit.,* 8.
[5]This is crucial in the critique of Freud's *Leonardo da Vinci and a Memory of his Childhood.* See Elkins, "Psychoanalysis and Art History," *Psychoanalysis and Contemporary Thought* 9 (1986): 261-98.
[6]See also H. Hutter, "Original, Kopie, Replik, Paraphrase," in *Original—Kopie—Replik—Paraphrase,* Bildhefte der Akademie der Bildenden Künste in Wien, Doppelhet 12/13 (Vienna, 1980).
[7]G. Steiner, *Real Presences* (Chicago, 1989).
[8]See J.-P. Niemeier, *Kopien und Nachahmungen im Hellenismus: Ein Beitrag zum Klassizismus des 2. und frühen 1. Jhs. v. Chr.* (Bonn, 1985), and B. S. Ridgway, *Roman Copies of Greek Sculpture: The Problem of Originals* (Ann Arbor, 1984).
[9]See D. G. Karel, *The Teaching of Drawing at the French Academy of Painting and Sculpture from 1760 to 1793,* PhD dissertation, University of Chicago, 1974, unpublished; L. Tavernier, *Das Problem der Naturnachahmung in den kunstkritischen Schriften Charles Nicolas Cochins d. J., Ein Beitrag zur Geschichte der französischen Kunstteorie* (Hildesheim, Zürich, New York, 1983), esp. 60 ff.; and P. L. Price, "Learning Through Imitation: Some Examples," in *Children of Mercury, The Education of Artists in the 16th and 17th Centuries,* exh. cat., Department of Art, BrownUniversity(Providence, Rhode Island, 1984).
[10]Among sources I have found useful are *The Forger's Art, Forgery and the Philosophy of Art,* edited by D. Dutton (University of California Press, 1983); H. Steele, "Fakes and Forgeries," *British Journal of Aesthetics* 17 (1977): 254-58; M. P. Battin, "Exact Replication in the Visual Arts," *Journal of Aesthetics and Art Criticism* 38 (1979): 153-58; D. F. Stalker, "Goodman on Authenticity," *British Journal of Aesthetics* 18 (1978): 195-98, M. Sagoff, "Historical Authenticity," *Erkenntnis* 12 (1978): 83-93; C. B. Cebik, "Forging Issues from Forged Art,"

Southern Journal of Philosophy 27 (1989): 331-46; M. Wreen, “Counterfeit Icons and Forged Paintings: Caveat Emptor,” *Analysis* 40 (1980): 156-51; and G. Bailey, “Forging Art,” *Southern Journal of Philosophy* 27 (1989): 319-29.
[11]For Van Meegeren’s technique see H. Van de Waal *et al.,* editors, *Aspects of Art Forgery* (The Hague, 1962), 44 ff., with references to W. Froentjes and A. M. de Wild in *Chemisch Weekblad* (1949), 267, and P. Coremans, *Van Meegeren’s Faked Vermeers en de Hooghs* (Amsterdam, 1948); *Imitationen: Nachahmung und Modell: Von der Lust am Falschen,* edited by J. Huber *et. al.,* exh. cat., Zürich, Museum für Gestaltung (Basel, c. 1989),140 ff.; and H. B. Werness, “Han van Meegeren *fecit,”* in *The Forger's Art, op. cit.,* 1 - 57.
[12]This is acknowledged in some philosophic work; see D. Dutton, “Artistic Crimes: The Problem of Forgery in the Arts,” *British Journal of Aesthetics* 19 (1979): 302-14.
[13]For historical information on forgery see first *Fake? The Art of Deception,* edited M. Jones *et al.*, exh. cat., British Museum (London, 1990), and then *Gefalschte Blankwaffen: galvanoplastische Kopien, Probleme der Authentizität,* ed. I. Baer *et al.* (Hannover, 1980); A. Andren, *Deeds and Misdeeds in Classical Art and Antiquities* , Studies in Mediterranean Archaeology, Pocket-Book, 36 (Partille, 1986); K. Turr, *Falschungen antiker Plastik seit 1800* (Berlin, 1984); *Falsifications and Miscreconstructions of Pre-Columbian Art: A Conference at Dumbarton Oaks, October 14th and 15th, 1978,* edited by E. H. Boone (Washington, D.C., 1982); and J. Hüber, *op. cit.*
[14]See I. Mac Kenzie, “Gadamer’s Hermeneutics and the Uses of Forgery,” *Journal of Aesthetics and Art Criticism* 45 (1986): 41-48, and see also U. Eco, “Fakes and Forgeries,” *Versus, Quaderni di studi semiotici* 46 (January-April 1987): 3 ff.
[15]For Kovacs see J. Adamovic *et al.,* editors, *FRA-YU-KULT, Collection of Franciscan Monastery,* exh. cat., Mazej, Museum Siroki Brijeq (Yugoslavia, 1990), 90 ff.
[16]For reproductions as the “repressed” condition for the existence of originals, see R. Krauss, “The Originality of the Avant-garde: A Postmodernist Repetition,” *October* 18 (fall 1981): 47 - 66.
[17]See P. Nagy, moderator, “From Criticism to Complicity: *Flash Art* Panel,” *Flash Art* 129 (summer 1986): 49, and Yve-Alain Bois, “Painting,” in *Endgame: Reference and Simulation in Recent Painting and Sculpture,* exh. cat., Boston, Instituteof Contemporary Art(Boston, 1986), 29; and J. Siegel, “After Sherrie Levine,” *Art Magazine* LIV / 10 (summer 1988): 141 - 44.
[18]R. Lowell, *Imitations* (New York, 1961),
[19]For imitations see also *The Counterpoint to Likeness: Essays on Imitation and Imagination in Western Painting,* edited by S. Howard (Davis, 1977), which is a selection of graduate students’ papers from a seminar on imitation; and *Imitation and Inspiration: Japanese Influence on Dutch Art,* edited by S. van Raay (Amsterdam, 1989).
[20]Nabokov, *Eugene Onegin: A Novel in Verse* (Princeton, 1990).
[21]M. Mende, *Dürer A-Z, Zeigenössische Dürer-Variationen von Anderle bis Zimmerman,* exh. cat., Stadtgeschichtlichen Museen Nürnberg and the Albrecht Dürerhaus Stiftung (Nürnberg, 1980).
[22]For Canaletto’s *cappricci* see T. G. Links, *Canaletto* (Ithaca, 1982).
[23]See J. Huber, “Imitative Strategien in der bildenden Kunst,” in *Imitationen: Nachahmung und Modell: Von der Lust am Falschen, op. cit.,* 144.

Discussion

one vs seven!
Jose Luis Guijarro
22 nov. 2004 19:05 UT

Forty two years ago, an Edinburgh linguist, Robert M.W. Dixon, published a book, LINGUISTIC SCIENCE AND LOGIC (1963, The Hague, Mouton) in which he speculated about the nature of science. For him, there were two levels involved, the level of what he called “substance” (which comprised the reality isolate one wanted to analyse and its localization in the world) and the level of “form”. He maintained that, although the scientific goal was to account for the existence and functioning of the chosen isolate in situation, the scientific effort took place at another level, namely, the “formal” one. This level, according to Dixon, was “a fully explicit mirror in which will be reflected certain scientific patterns (the “form”) of the raw material. All theoretic operations and descriptions take place entirely […] on the plane of the mirror” (pg.22). Dixon was, of course, describing the formal systems of thinking which have been used by human beings for over millennia, ever since simians became human beings. For instance, this is the way we think by means of our LANGUAGE. When I think of somebody, for example, I don’t need to put her in flesh and blood before (even less, inside!) me; I merely give her a given “form” which I store in my brain and then use it in connection with other

such forms to think or speak about her. This “formalising” power may be the reason for the ancient and still common notion that “to name is to create”, which we find in the first words of St John’s Gospel.

However, the idea of science that Dixon explored seems to me to be a little too vast (for it is the GENERAL way we have to go around in the World we live) or, paradoxically, too short (for it does not deal explicitly with the most important level I think science should attain: the level of EXPLANATION). This is probably unfair to Dixon’s general idea of science, but, as I say, the explanatory power of a science is not postulated as a necessary level of science.

As a linguist, I have read James Elkins paper with those contextual biases: (1) the formalising necessity one has in order to analyse something in “the mirror”. This he achieves in an interesting way with all the conceptual (i.e., “formal” in a way) distinctions about original objects and their numberless reproductions, and (2) the explanatory power of his theory which, according to my point of view, is totally lacking in his paper. On the contrary, instead of having one problem, the one that fakes in art pose for us, we have now seven problems for which we do not have any solutions. I am not saying that the present paper is invalid, how could I? What I am trying to point to is the fact that his naming of different categories of reproduction has added confusion to my mind. Probably it was Elkins’ goal to make us aware of that complexity, in which case, I must confess that he has achieved it (at least for me!) with this intellectual revulsive. I wish he could give us some explanatory answers as well!

connoisseurs and fakes
Noga Arikha
24 nov. 2004 20:02 UT

In response to Guijarro's misgivings about the usefulness of multiplying distinctions, I would argue that Elkins wants to show how historically grounded this debate actually must be, and how the notion that "the sequence from original to copy is not continuous but proceeds in historically determined stages" is intimately connected to the notion that "Original and copy are not well-defined terms".

A fake, after all, is not created out of the blue, as little as a work of art is created out of the blue. The "anxiety of influence", the notion coined by Harold Bloom within the context of literary criticism, does not always have to be anxiety; it can be a boon. The ability to trace influences is essential to the connoisseur's ability to make correct attributions, that is, to affix a name correctly to a work, just as the ability for an artist to absorb influences is what determines his greatness, and his value. But the value of the connoisseur's capacity is determined both by the prestige of "finding a Michelangelo" in a convent's dusty back-room and by the possible financial reward entailed by a great name. Without degrees of quality, that is, of transcended influence, and without degrees of fame, that is, of a hierarchy of names - and although quality is not a function of fame however much the market "sells" the latter as if it effectively were the former - there would be no need either for connoisseurship nor for the clear, intended fakes whose reward can result in nothing more prestigious than a jail sentence. Fakes represent the other side, so to speak, of the economics of art and of the human ambitions that underpin the making of art. In other words, in order to "create a fake", there have to be the cultural circumstances in which it is possible to create a "fake", that is, in which "fakes" are a clear category. To discuss fakes outside the categories created within the history of art (and of society) is indeed, as Elkins, I think, suggests in his paper, to create confusions rather than to clarify them.

In response to Noga
Jose Luis Guijarro
25 nov. 2004 10:22 UT

I agree wholeheartedly with all Noga Arikha says in her message. I have never doubted that the multilevel distinctions between different copies (i.e., copies of the real world, of another copy, of the style of another copy, an so on) is a cultural question which can be analysed and studied in an historical perspective to explain a lot of problems, such as the reason why in certain cultures, the important thing is not the authorship of X but rather the type of information people get from X. That is, as I see it, lots of issues which have been considered by the different authors and their correspondents of this seminar are ONLY cultural phenomena. To describe them, to

differentiate them, to examine them are naturally interesting intellectual activities which I would not dream criticising.

But, as I understand (perhaps wrongly) it, the aim of this seminar is not only to make interesting hair-splitting distinctions of culturally loaded phenomena. I had always thought that what interested the participants in this seminar was a move ahead this historical dissecting activity; that the real aim was to EXPLAIN why (possibly in all cultural environments) some X are considered valuable, while others are not. In other words, to describe the mental functioning of our relationship to what in our culture we call ORIGINAL ART (say, “A”) and our processing of what we consider FAKES (say, “F”) in our present environment.

If we are able to reach interesting conclusions in that level, we may then apply the “A” and “F” descriptions to other totally different social phenomena that happen to be culturally determined in another human group. And if that would result in interesting findings, we might then be able to explain why human beings are generally endowed with different ways of processing the information that reaches them in one way or another, how the cultural factors affect it, and what differences are possible in that frame.

If my intellectual capacity is not severely malfunctioning, the no doubt seven interesting “differences” James Elkins puts forward in his paper don’t seem to me to have anything to do with the type of EXPLANATORY “differences” I have just mentioned. As I said in my former contribution, they present, instead, seven new problems where we had only one before; and no solution to any of them whatsoever.

history and theory
Noga Arikha
25 nov. 2004 16:19 UT

José would like us to be able to "apply the “A” [original] and “F” [fake] descriptions to other totally different social phenomena that happen to be culturally determined in another human group."

But that takes us away from the issue at hand, I think, that is, from the question of why fakes matter IN ART. The problem with a general, theoretical preoccupation such as his is that it cancels out our starting point, which is at least an enjoyment, if not a valuing of art, in itself what allows the very existence of fakes; the goal is still to understand what IN ART makes fakes possible, in order then to understand why they matter so much to the viewer, buyer, collector, indeed evaluator. He writes that these issues "can be analysed and studied in an historical perspective to explain a lot of problems". But no, it is more than that: I think that the historical examples do not so much illustrate as constitute the story we are trying to tell.

The question of "why human beings are generally endowed with different ways of processing the information that reaches them in one way or another" is a good, important question but I don't think that is the question that Elkins, as an art historian rather than, say, a cognitive psychologist, is asking here. The art historian's perspective, however, is crucial in a debate on fakes, for the big, theoretical questions, when framed in the way Guijarro wants them to be, can at times be too vast, too abstracted from the objects of knowledge to be effective. In order to understand how we process information in this case, we have to look at what, historically, this information is. Art is not just any old information. It is a peculiar sort of information, created by us - and its power is a function of how much it bears the immediate trace of the creator's hand or vision. Of course a work is a creation even when we copy it, or create a fake - but then not quite in the same way. Copies and fakes are not the same sort of creation as originals, because they are mediated - and that is why it is important to identify the differences and the various levels of mediation, to define what originals are, in what ways a copy is not an original, and indeed how imitations differ from copies, how pastiches differ from imitations, and so on.

It is perhaps not theoretically adventurous merely to identify differences, but I would think it helpful to define one's terms and to explicitly ground them within the history and cultural context

in which they happen, only then to takw off and derive from this case bigger questions about human cognition.

Clarifying my qualms
Jose Luis Guijarro
26 nov. 2004 10:31 UT

Thanks, Noga, for clarifying your position. Let me try to clarify mine.

You say “The problem with a general, theoretical preoccupation such as [t]his is that it cancels out our starting point, which is at least an enjoyment”.

Well, does it? Why can’t we, for instance,

(A) DISTINGUISH between different sorts of processing in-coming information which could produce, say,

(1) a mystic experience, (2) a trance experience, (3) an ART experience, (4) an aesthetic experience, (5) an enjoyable experience, etc.,

(B) DESCRIBE those different cognitive states, their possible relations, ways of combining, etc.

(C) find a biological EXPLANATION for them, and

(D) then find CULTURAL CORRELATES that actualise in different societies those cognitive states?

If I understand you correctly, Noga, you prefer to start from (D) and move (more or less) upwards, which may be also a productive decision. What I am wondering about all the time, though, is not the direction of the possible research on these topics, but rather why a personal-cultural distinction should be considered relevant in any way to achieve a likely explanation of the phenomena we are discussing about without relating it to well described cognitive states.

Mechanical vs. hand-made reproduction
Gloria Origgi
28 nov. 2004 22:01 UT

I find Elkin’s distinctions very useful, because they give us a chance to test some of the main philosophical concerns of the debate on a larger sample than the authentic/fake distinction (or the much discussed triad authentic/replica/forgery).

But, taking his historical stance, I wonder whether his taxonomy can ignore the distinction between mechanical vs. hand-made reproduced works of art, which is only briefly mentioned in his discussion of the notion of reproduction.

The history of technical reproducibility is multifarious. Some arts were mechanical reproducible in the ancient Greece, such as founding and stamping bronzes, coins and terracotta, while some others have become technically reproducible only in the Middle Age (woodcut, engravings). Lithography is introduced at the beginning of XIX century and it is the XX century the “Age of Mechanical Reproduction” of works of art.

Our tolerance with copies depends in part of its mode of reproduction, that is, whether it is mechanical or hand-made. Walter Benjamin pointed to this problem when he wrote: *Confronted with its manual reproduction, which was usually branded as a forgery, the original preserved all its authority; not so vis à vis technical reproduction*

As we have seen in the previous debates on the aesthetic value of replicas and on the role of performance in the appreciation of a work of art, I think that we cannot ignore the impact of new technologies of reproduction (mostly in the XX century for figurative arts and music) in our understanding of the whole classification proposed.

Le premier original, la première copie
Clotilde Lampignano
1 déc. 2004 14:18 UT

A la suite des approfondies explications , l'auteur conclut,me semble-t-il, qu'il n'existe pas d'originaux, mais qu'on doit les definir comme des oeuvres qui se réfèrent a et qui sont le résultat des copies.. Puis il dit que les copies doivent être repensées comme si elles étaient des originaux *in statu nascendi*. Alors je crois qu'on doit se poser la question, si les originaux se réfèrent à des oeuvres qui se réfèrent à des copies etc. sur un chaine infinie, quel est le premier original, quelle est la première copie, ou vraiment existe-t-il le premier original, du moment que l'original est aussi une copie. Ce dinamisme infini de la copie à l'original e viceversa apporte les mêmes doutes que la dichotomie

authentique/inauthentique.

What about a nominally inauthentic but expressively authentic forgery?
Gloria Origgi
1 déc. 2004 21:18 UT

Yasushi Inoué's novel *The Counterfeiter* is about a journalist who is commissioned to write the biography of a famous painter but becomes fascinated by a man who produced forgeries of the painter. He reconstructs the sad life of the counterfeiter, a former friend of the painter. At the end of the novel, the writer takes some rest from his researches, and spends some days in the countryside, looking at the mount Amagi. Looking at that perfect landscape and thinking about the forgeries he has seen in the counterfeiter's poor home, the writer wonders why those paintings have to be considered as forgeries. They are about the reality around him, he concludes, and continue to live and represent world in which fake and authentic are meaningless words.

It is a beautiful novel that made me think about the bizarre status of forgeries that I can try to restate in the terms of Denis Dutton's distinction between nominal authenticity and expressive authenticity. A forgery is of course nominally inauthentic, but it can be expressively authentic, as in the case of Hara Hosen's paintings in Inoué's novel.

What about our aesthetic judgement's in the case of a nominally inauthentic but expressively authentic forgery? Wouldn't it be just snobbery or, worse, conformism, to consider it is in some sense "less" than the original?

> **Insecure and/or Poor Artists**
> Richard Minsky
> 2 déc. 2004 13:34 UT
>
> Not *bad* artists. Artists who are not wealthy. There have been many "forgeries" of famous artists' works that have "originality," by at least one of the criteria James Elkins describes. Gloria is correct. Using a fictional reference for this, however, gave me the same unease as Sartwell's implication that partial differential equations could be applied to a historical model, or the notion of a hypothetical "perfect copy" in earlier discussions.
>
> If intention is a determinant of content in art, then the state of mind or finances of the artist may be a factor in attribution of originality. Perhaps the minds of artists are not built the same as those of art historians or philosophers. Perhaps their eyes are sensitive to different wavelengths or amplitudes, or their interpretive faculties have different levels of rejection of visual information at a threshold that inaugurates conflict with accepted philosophical or art historical truths.

Belief structures are significant components of what we see. From Charles Mackay's 1841 *Memoirs of Extraordinary Popular Delusions* to the 2004 USA Presidential election we can note numerous examples. Art is no different.

A skilled artist who is more concerned with paying the rent and feeding the kids may create a beautiful original painting in the style of a famous artist, including the artist's signature, and sell it for much more money. An insecure artist with low self-esteem may attribute work in another's style out of a feeling that his or her own work is not "original" or "good" enough, and may also be in error about that. The attribution of it to the artist who is emulated might be a psychological crutch to raise self-esteem, if the art community accepts it.

The above non-sequiturs notwithstanding, we are left with Gloria's enigmatic question: is it *in some sense "less" than the original?*

How do we quantify "more" and "less" in valuing a work of art? Since the "forger" did not create the "original" style, we may say it is less. But what if the "fake" has a more powerful metaphor, expressed with a stronger image? Or at least is a strong enough painting to be regarded by the art community as an excellent example of the master's work, until it is discovered to be a fraud? Do the acts of criminalizing the artist and reattribution of the work change its aesthetic value? That brings us back to the cultural aspects of perception and valuation that have been involved throughout this symposium. We have returned to our moderator's starting point.

Did reading *The Counterfeiter* inspire the creation of this conference?

Figurative art, style and temperament
Gloria Origgi
3 déc. 2004 13:34 UT

Richard writes: *How do we quantify "more" and "less" in valuing a work of art? Since the "forger" did not create the "original" style, we may say it is less. But what if the "fake" has a more powerful metaphor, expressed with a stronger image?[...]Do the acts of criminalizing the artist and reattribution of the work change its aesthetic value? That brings us back to the cultural aspects of perception and valuation that have been involved throughout this symposium. We have returned to our moderator's starting point.*

Inoué's novel is about a special case of art, that is, figurative art. It is special because it holds a double relationship with what it is supposed to represent and the author's intentions in representing that particular aspect of reality. Emile Zola said that a work of art is a corner of nature seen through a temperament. It is probably faking this temperament that we find unacceptable in forgeries.

Faking a style is another matter. Artists are very often under the cultural pressure of following the style of their epoque. A too dramatic departure from the canons of style usually results in exclusion from the art community. Take the case of Van Gogh's copies of Millet, which reprent the same scenes, but with such a different "temperament" that they were rejected by his contemporaries.

Abstract or figurative, it can be a fake original
Richard Minsky
4 déc. 2004 0:14 UT

Gloria, I do not understand the distinction of figurative art as "special because it holds a double relationship with what it is supposed to represent and the author's intentions in representing that particular aspect of reality."

When I look at the work of Morris Louis, Clyfford Still, Mark Rothko, Franz Kline or Kandinsky there is also at least a double relationship, and perhaps it is more multidimensional than double,

as I might also say about figurative paintings that are more representational of landscapes or humans than of paint and spirit.

The spiritual aspects are present in the old masters as well, and their abstract compositional elements no less pictorial. In Kline's work, for example, I see the pictorial representation of edges that has always fascinated me, and the reality of border conditions intrinsic to a brush stroke establish relationships no less clear or significant than those of Manet's *Dejeuner sur l'herbe*. Perhaps the "push-pull on the image plane" as the subject of a particular aspect of reality attracts me because I was born in Manhattan in 1947.

It's most interesting that you include "faking a style" as a determinant of artistic form within an epoch in the context of Economics, or perhaps it is social exclusion or ostracization you refer to. The result is the same. It caught my eye in the context of this conference because that makes original works into fakes. It is similar to what I referred to in an earlier intervention as "faking your own style" or producing work in similar form due to the demand of the art market.

Clarification
Richard Minsky
4 déc. 2004 10:09 UT

In the language of the current paper, work by an artist who is producing redundant baubles for the marketplace might can be described as unique, but neither primary nor originary.

That feature enabled such works, complete with provenance, to be used as chips in certain art world poker games. Whether there is any cultural value to such items is another issue.

Art vs. science or beauty vs. truth
Mario Borillo
16 déc. 2004 13:54 UT

Let us try an unusual - and difficult - but maybe fruitful parallel. For a long time, at least in the Western societies, art was associated with the idea of beauty. Beauty was the traditional dimension for evaluating/judging art production. Beauty itself was a multiple-dimensional notion, often both referring to the physical beauty of what was reproduced - the model - and also, in some historical periods, to its "moral" appraisal. But beauty was essentially correlated with the skill of the artist in doing it (beautiful way of writing, of painting, etc.). Obviously, the limitations of such a view in art have become more and more patent along the 20th century, after the Cubism, Futurism movements and Dada especially.

It is worth noticing that it has been the converse in science, where truth and falsity play an essential role, while aesthetic considerations (when they can be defined) are secondary. Potential empirical falsifiability of theories (and their current non-falsity) has been the main criterion in experimental sciences, while logical coherence has been at the core of the foundations of mathematics and of scientific reasoning. In most cases, the expressive "economy" and the mental clarity of the explanatory power of the scientific theories are the main constituents of their so-called "beauty".

However, especially from a cognitive science point of view, putting the question of art judgment in terms of falsity/truth become relevant, and not only for discussing the status of fakery and forgery, following the usual criteria. But then, falsity/truth with respect to what? Dutton (5)'s distinction between nominal falsity / authenticity and expressive falsity / authenticity indeed agrees with our intuition that falsity can be encountered in art not only in forgeries, but also in a pretended piece of art without a sufficiently deep and personal commitment from its producer shall probably lack some kind of subjective authenticity. However, from a "symmetrical" point of view, a true "forger" might be still an authentic artist in some sense, one who is able for instance to create a piece that some famous artist could have plausibly produced.

Another noticeable difference between science and art is the great importance of the sociological dimension in the recognition of artists and the use of art as a code for mutual recognition in socio-economical groups (Bourdieu and Darbel (2), Dubuffet (4), Moulin (10), Heinich (8)), even if sociological behaviors may also have a role in science at a smaller scale.

Forgery
Mario Borillo
16 déc. 2004 13:56 UT

Let's consider forgery first. It is usually presented as a new piece of art, e.g. a painting, which is produced in the manner of a well-known artist, and is falsely claimed to have been realized by him/her (origin criterion). More exceptionally, it may happen that after time, it is no longer obvious to determine what is the original piece among several quite similar copies (historical criterion). At least in the West, it is largely admitted that certified originals have greater values than copies, and pieces of art with an uncertain origin (financial criterion). This shows the importance given to the originality, the uniqueness of the piece, and to the authorship. This has been questioned in different ways:

- The inventions of forgers as fictional characters of novels (with detailed biographies and paintings), as done by William Boyd (3), or before by Max Aub (who wrote the life of Jusep Torres Campalans and produced his paintings (1)), who have never existed, but who were presented has genuine (and genial) ones. There is also a tremendous number of examples of fake writers in the history of literature (9).

- Another interesting 'variation' along this line is the work done by « appropriationist » artists (Mike Bidlo, Sherrie Levine, Richard Pettibone, Sturtevant, Philip Taaffe ; see (11)), who "acknowledged" fakery by re-producing paintings and other pieces of art by Matisse, Picasso, de Chirico, Duchamp, Mondrian, Pollock, Johns, Beuys, and many others, and by signing them with their own name, and titling them « Not a Picasso », « Not a Kandinsky », or « Without Title (After Kasimir Malevitch) », or directly by the name of the author of the piece copied, emphasizing in fact slightly different attitudes and messages, in their « appropriation » works.

- Another unusual question arises for instance with the flowers painted by Mondrian (he was very good at that, and he did it somewhat reluctantly for earning his living). Are they true Mondrians'? Can an artist make a forgery of its own work? A strange, but maybe interesting attitude.

Authenticity
Mario Borillo
16 déc. 2004 13:58 UT

Now, let us come to the main issue: expressive falsity/authenticity. What does Dutton not explicitly point out is the fact that an artist comply – explicitly or implicitly – with different kind of rules or prescriptions. In particular - rules or conventions defining the possible contents of artwork (« semantic aspect »), associated with the role of art and its socio-cultural context (not the same at the time of the cavern art, or in medieval religious art, …), leading for instance, for a long time in Occident, to figurative pieces of art only; - rules defining structural constraints on artworks (« syntactical aspect »), i.e. rules characterizing 'styles'; - pragmatic rules deriving from the material conditions in which the artist is embedded (e. g. compliance with real constraints such as size limitations or technology capabilities: to-day, the so-called numerical art); - rules rooted in the bio-cognitive characteristics of visual (or auditive, tactile, olfactive) perception (e.g. perspective laws, physical properties of colors in painting, of flagrances in perfumes,…) - artist's own rules, his/her specific "signature".

To be short: The first three sets of rules refer to a semiotic view of art (Eco (6)) and depend on the society context, the fourth one depends on human perception and brain, while the fifth set, the crucial one in contemporary art, refers to characteristic features that often conflict with rules in the first three sets. The artist should emphasize his/her differences, but also some links with 'tradition' to make what is proposed understandable (and valuable) to other people (if the artist wants to be recognized as such). A complex association of inner coherence, originality, and subtle relevance are maybe the main criteria for appreciating this last set of rules. Besides, it is clear that the appraisal of a piece of art is also conditioned by the perception model of the one who judge, who may not be able to "recognize" some piece of art that (s)he cannot understand since s(he) does not even know the underlying rules.

Clearly in modern art, the two first sets of rules and conventions play a smaller role, while the artist's own "idiosyncrasies" play a essential role. This differentiation is also in agreement with the fact that art has been for a long time anonymous (not signed), still, for instance, at the time of schools around

great painters. It is remarkable that the emergence of individuals has also facilitated the emergence and the extension of the forgery problem.

In the framework of all these rules, conventions (and some of their transgressions), a piece of art may be examined and evaluated, its coherence with other productions checked, and if all the applicable rules and criteria are sufficiently satisfied, the piece of art may be declared meaningful, authentic and valuable. It is remarkable that other important issues in art analysis could benefit from some of the questions raised above: for instance, the definition of the relations and differences between art tendencies and schools, or at an individual level the recognition of a piece of art as belonging to some established tendency…

Evaluating art in terms of beauty seems to be purely “emotional”. It is note-worthy that trying to appreciate it in terms of expressive falsity/authenticity opens the way to make the sources of emotions induced by art somewhat explicit.

Mario Borillo, Catherine Gadon and Henri Prade1

Drawing in the Social Sciences: Lithic Illustration

Dominic Lopes (University of British Columbia)

(Date of publication: 23 May 2005)

Abstract: Images are used in science to present data. Taking the physical and life sciences as paradigms, one might suppose that machine-made images always serve this purposes better than hand-made images. A broader view of the use of scientific images, which includes the social sciences, shows this supposition to be false. Archaeological drawings of human artifacts, such as stone tools, are preferred to photographs. The explanation of this is that the human drawing system is an observational tool with special powers.

Images are used in science to present data. Taking the physical and life sciences as paradigms, one might suppose that machine-made images always serve this purposes better than hand-made images. A broader view of the use of scientific images, which includes the social sciences, shows this supposition to be false. Archaeological drawings of human artifacts, such as stone tools, are preferred to photographs. The explanation of this is that the human drawing system is an observational tool with special powers.

A teeming variety of images serves a variety of scientific purposes. Scientific images include geometrical diagrams, Doppler radar images, bubble chamber photographs, fMRI scans, Feynman diagrams, and maps of adaptive landscapes. These and other images are used to formulate hypotheses, conduct proofs, describe experimental set ups, confirm hypotheses, present and analyze data, test and calibrate instruments, and communicate data, results, or hypotheses. One might expect that in any given scientific context, some kinds of images will serve some purposes significantly better than other kinds of images. Thus a full taxonomy of scientific images will reveal systematic connections between (1) image types, (2) purposes, and (3) contexts of use. However, philosophers of science (unlike historians of science) have focussed on two small corners of the ground such a taxonomy should cover – diagrams used in mathematical reasoning, on one hand, and, on the other, machine-made images used to present data in the physical and life sciences. A look at images falling outside these areas of focus is needed for progress towards the full taxonomy; it also affords an opportunity to reexamine some standard historical and philosophical accounts of scientific images.

Lithic Drawing

Archaeological illustrations fall into two main classes. Receiving greater attention are 'reconstruction drawings,' which depict imagined scenes of prehistoric hunting, foraging, campfire life, childrearing, and other such activities. According to Stephanie Moser, these drawings were used by archaeologists to argue in a distinctively visual manner for or against conferring hominid status on fossil specimens of australopithecenes and other primates (Moser 1996, 1998). While the drawings neither constitute nor present evidence for or against hominid status, they seem to have been persuasive, and so make for a rich case study of images in scientific rhetoric. Receiving less attention are images of archaeological finds. Settlements and other sites are mapped and buildings are rendered in architectural plan view. Surprisingly, though, artifacts (such as tools) are almost invariably drawn – photographs appear only in popular, non-professional books (e.g. Waechter 1976). Why are drawings preferred? What roles do they fill in archaeological practice? These questions might have different answers for different types of artifact, so consider the case of flaked stone tools.

The authors of a recent handbook written for archaeologists and illustrators feel obliged to explain why drawing is preferable to photography in archaeology. They write that whereas photography "has the disadvantage of being unselective," "a drawing can convey much more relevant and comparable information and can be edited more easily than a photograph" (Adkins and Adkins 1989: 6). Adkins and Adkins give two explanations of the importance of selectivity in representing artifacts. First, "a good drawing selectively portrays the details that the reader needs to see and edits out irrelevant details, so that the illustration can be understood much more easily" (Adkins and Adkins 1989: 7). Second, "the purpose of the illustration is to convey not only information but also an interpretation of that information" (Adkins and Adkins 1989: 5). The first explanation is in tension with the fact that photographs are common in popular publications, where easy understand would seem to be paramount. The second explanation raises questions about the role and nature of interpretation in archaeology.

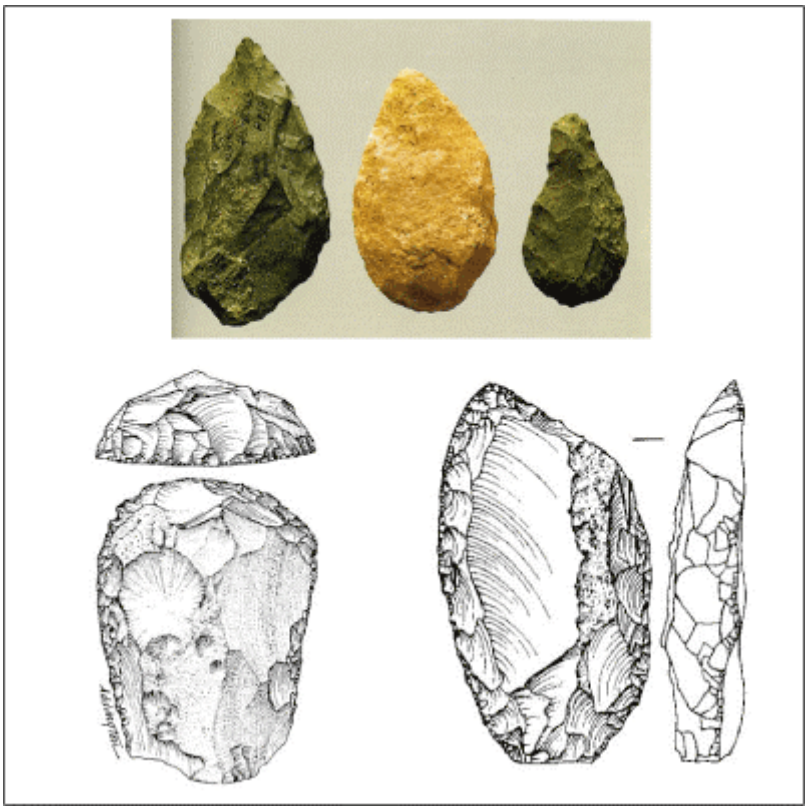

Figure 1

To find out what an artifact is for, examine the details of its making: this principle works just as well for artifacts that are drawings of artifacts as it does for artifacts that are drawn.

Luckily, the details of lithic drawing are lucidly and systematically set forth in Lucile Addington's manual for archaeological illustrators (Addington 1986). Flaked stone tools are drawn either by an archaeologist or, more often, by a collaborator with specialized training in lithic drawing. Addington is such an illustrator, and her manual is intended to be read by illustrators-in-training and also by archaeologists seeking explicit instruction on the principles of lithic drawing.

As the manual makes clear, lithic drawings must serve two needs. First, a "universal language" is needed to ensure that drawings from any lab can be compared easily and reliably (Addington 1986: ix, xiii-xiv). Note that photography answers this need perfectly well. Second, the drawings should convey all and only relevant information about the objects represented. What is relevant is determined by the nature of the object represented and the role of the representation in its context of use. Lithic drawings represent artifacts and they represent them as artifacts – as objects whose form is largely the result of deliberate human activity – in a context where they are objects of study for their artifactual characteristics. As Addington remarks, "properly drawn artifacts are invariably more informative than photographs in illustrating a prehistoric knapper's workmanship as well as an artifact's form and diagnostic features" (1986: ix).

Knapping involves repeatedly striking a stone, usually flint, in order to remove flakes. Either the flakes themselves are knapped further or the stone from which they are taken is knapped to leave behind a useful pattern of flake scars. The stone is thus 'retouched' in order to achieve a finished form, and it may subsequently be subjected to reuse or weathering that changes its shape or surface. Flaking stone is not a way to pass the time. Stone tools were made for use as knives, cleavers, scrapers, arrowheads, and hammers. Thus a lithic drawing must show how a stone was knapped and with what

purpose in view. To do this, it must show: scale; the pattern, sequence, direction, and force of blows to the stone; the bulb and platform of percussion; areas of retouch, snapping, and truncation; areas of grinding, battering, or abrasion; fractures caused by heating; the effects of materials; and pitting and sickle sheen. Fossils, variegated coloration, patina, seams, banding, and crystalization are not shown.

Since relevant features of each stone must be shown in a way that allows for reliable comparisons, lithic drawing conforms to several conventions. The object is illuminated from 45° in the upper left. The ventral surface of the stone is placed at the bottom of the drawing and, when multiple views are shown, profile views are shown next to the edges they display. The scale is fixed at 1:1. Paper types, drawing instruments, and techniques of pencilling and inking are standardized. Finally, a well-defined representational vocabulary is strictly followed. Stippling indicates the cortex of the stone, with greater density indicating greater roughness. Outlines show flake scars in their sequence of making. Lines imitating the ripples caused by flaking indicate the direction of a blow and, by their thickness, its approximate force. They are shown as attached to only one side of a flake scar and to two sides of a bulbar surface. Curved direction lines indicate snapped surfaces and thermal fractures are shown by spider lines, spoked lines, or swirls. Moreover, stippling and direction lines are dual purpose, since they convey the volume of the stone as well as its surface details. This vocabulary of marks helps replicate the look of the object. It is supplemented by a non-mimetic vocabulary. Arrows point to bulbs of percussion. Dashed lines show where broken fragments of a stone fit together. Tick marks coordinate multiple views so that key features of the stone can be matched up.

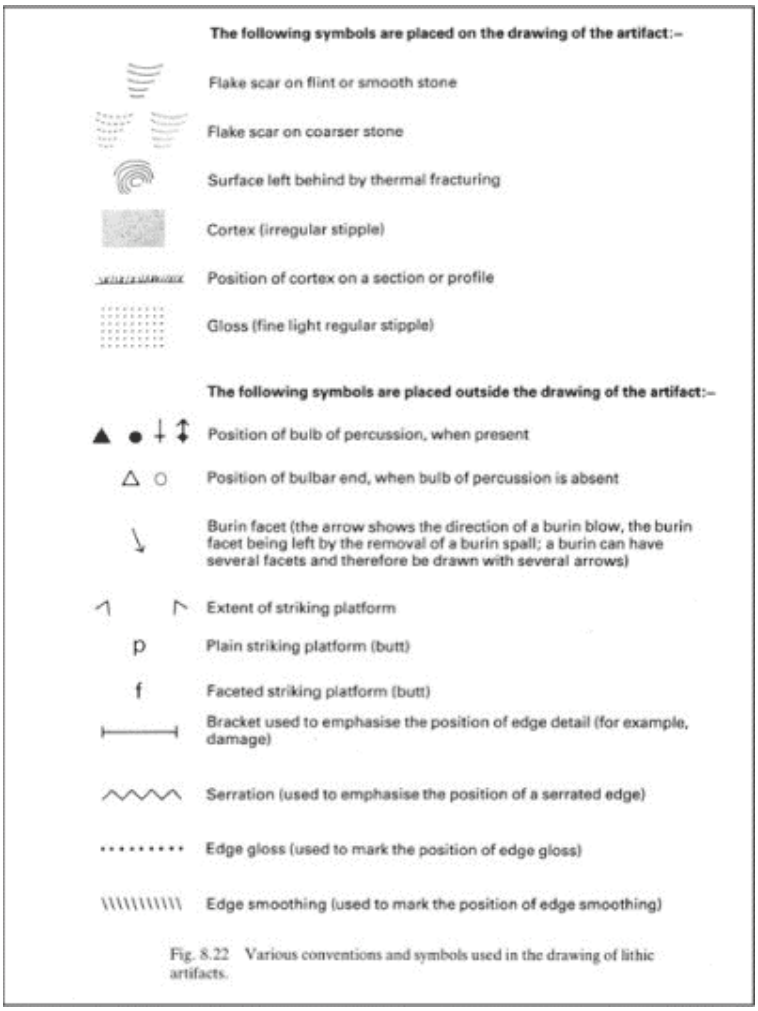

Fig. 8.22 Various conventions and symbols used in the drawing of lithic artifacts.

Figure 2

In order to make a drawing complying with these rules, an illustrator must learn to 'interpret' or "read an artifact's surfaces" (Addington 1986: 2). He or she must have an eye for flake scars and what they indicate about their making; for the different types of flaking, including retouch, snapping, truncation, grinding, and cleaving; for the effects of materials (flint is most common but other types of stone are also knapped); for the effects of weathering and reuse, which must be distinguished from the effects of knapping; and finally for the practical challenges facing a knapper who is knapping a particular stone, since every stone is different and not all 'readings' of a stone are consistent with what a knapper can do with it. An archaeologist who is not also an illustrator is capable of the same reading, but only an illustrator is able to embody the reading in a drawing.

Drawing in History and Philosophy

Lithic drawings challenge several generalizations that have been made about scientific images and thereby raise some interesting philosophical questions.

According to one standard history (see Topper 1996), early scientific images are for the most part drawings that represent types – botanical and anatomical drawings are prime examples. In many cases, what is represented has features never found in a particular specimen – it is an idealization. In other cases, the object represented has the features of a particular specimen, but it is understood that we are to abstract from its particularity and see it is as standing for a type. Later, with the invention of mechanical imaging, many scientific images are made that represent particulars and not types: we are not to abstract from their particularized features since they present evidence about the particulars themselves. They fix observations for later examination.

Exceptions to this history are hand-drawn images used in mathematical proofs and the presentation of theorems or hypotheses, from Euclid to Feynman. However, these exceptions confirm the explanation of the history: drawings represent types; the representation of particulars comes along later, with mechanical imaging. Thus drawings still have a place in the age of mechanical image-making because they represent types. David Topper writes that "despite the invention of photography about a century-and-a-half ago… the artist still has a role to play in illustration, for the camera captures an individual specimen (the particular) whereas an artist can depict the archetype" (1996: 234).

There are other exceptions. Galileo's drawings of the moon, which were made during the early period, represent a particular. The discovery that the moon's dark spots are shadows caused by mountains required meticulous attention to the specific features of the spots. In the later period, Wegener used drawings of particulars in his accounts of continental drift. Again, the exact details of the shape of continental coastlines was needed in order to establish that the continents were once parts of a single land mass. Nevertheless, the main idea stands. Had they been available, photographs would have worked at least as well and probably better for the purposes of Galileo and Wegener.

Another element of the history is worth mentioning. It is natural to consider scientific images as art, when they are drawings, especially when they are drawings from life and most especially when they are old. At least, it is natural in these cases to put in play the questions of whether they are art and thus of the links between art and science. So one might think of the history as gradually putting a distance between artistic images and scientific images. The point is not that scientific images come to lack aesthetic value – on the contrary, they often have great aesthetic value. Rather, the point is that any aesthetic value they have is merely a side effect of their primary function.

Twinned with the standard history is a standard epistemology. The epistemology has two forks, both concerning what counts as good evidence for empirical generalization. The first fork is the claim that good evidence for an empirical generalization should issue from a reliable process. Add the assumption that processes are the more reliable as they rely on mechanical means of taking measurements, and we have a good epistemic reason to prefer photographs to drawings when we wish to capture data for empirical generalization. The second fork is the claim that good evidence for empirical generalization should issue from an objective process. Add the assumption that data-imaging processes are objective only if they produce images with non-conceptual content. An image has non-conceptual content just in case its having the content it has does not require that whoever or whatever makes the image possess concepts of the properties represented (Lopes 1996: ch. 9). If photographs, unlike drawings, have non-conceptual content, then we have another good epistemic

reason to use photographs rather than drawings to capture data for empirical generalization. Note that the forks are linked if objectivity promotes reliability.

This epistemology bolsters the thesis that drawings largely give way to machine-made images in science as images come to be used to capture data for empirical generalization. Data capture must be reliable and objective; machines have both virtues; draftsmen have neither. It also explains the observation that machine-made scientific images and artistic drawings follow diverging paths: the epistemic strictures on scientific image-making trump the aesthetic concerns that give the rule to art.

Lithic illustrations are drawings, but they do not represent types; instead, they capture data about particulars which are used in making empirical generalizations. While the drawings are often elegant, visually interesting, even beautiful, that is incidental to their primary purpose. Moreover, lithic drawings are relatively new, postdating the invention of inexpensive and reliable photography: there is a real preference for them over machine-made alternatives. In these ways, they are exceptions to the standard history of scientific images and the explanation of that history. And they challenge the standard epistemology, too. They are made by hand, which seems relatively unreliable, and their contents depend on the conceptual resources of the illustrators who make them, so they would seem not to be objective. If they are reliable and objective, taking into account the purposes for which they are made, then some account is needed of how that can be so.

Drawing, Reliability, and Objectivity

One might say that lithic drawings fail to capture good data for empirical generalization. Assume that this is not the case. Making this assumption has a price, of course. We must make sense of the epistemic quality of lithic drawings.

Images belong to systems. Each system is individuated by rules that set out how images in the system are made, by marking surfaces mechanically, through digital processing, or by hand-drawing, such that selected determinable features of objects or scenes are represented. Some systems are hierarchically nested within others. For instance, colour photography is nested within black and white photography, since colour photography is black and white photography with the addition of resources for depicting hue and saturation. Stopping at this point, of course, leaves us with a very coarse-grained conception of imaging systems, and a great deal more could be said – and has been said (e.g. Lopes 1996, Kulvicki forthcoming). But this is enough for now.

An imaging process outputs images belonging to a system. It is reliable in representing some determinable property of the world just in case the contents of images in the system are counterfactually dependent on the determinates of the determinable. An image's content is counterfactually dependent on trajectories just in case it represents objects' trajectories and would also represent the trajectories were they different. An image's content is counterfactually dependent on the outline shape of a stone just in case it represents its outline and would also represent the outline were the stone a different shape. Imaging processes which generate images whose contents are counterfactually dependent in this way are reliable.

One way to ensure that an imaging process is reliable is to build a device that makes images mechanically. Not all imaging devices work reliably, of course, but many do. Film-based photography is highly reliable for many determinables, for example. However, it is a mistake to assume that the only way to ensure counterfactual dependence is through mechanical causation. A system of images made by drawing is reliable in representing, say, shape, so long as the drawings represent an object's shape and would have represented it as having a different shape were its shape different. Do drawing systems meet that condition? Some do.

The conventions and techniques of lithic drawing ensure its reliability with respect to a selection of determinables, such as the scale of a stone, its overall shape, the shape and location of flake scars and retouch, and the location of features like thermal fractures. That is, an illustrator who follows the rules will produce drawings which show the relevant features of stones and which would also have shown those features as different, were they different. This counterfactual dependence is not a fluke; it is a consequence of compliance with the conventions and techniques of lithic drawing; and that is why lithic drawing is a reliable process.

This explanation of why lithic drawing is reliable might appear to preempt its objectivity. After all, in complying with the conventions of lithic drawing, an illustrator must exercise concepts such as 'flake scar,' 'retouch,' and 'thermal fracture.' He is not merely drawing the look of the stone; he is drawing its look given a reading of it in terms of those concepts. (You and I would make poor lithic illustrators, no matter what out drafting abilities, because we lack the required concepts.) However, we have assumed that drawing, to be objective, cannot require that an illustrator possess concepts of the features depicted. So lithic drawing, for the very reason that it is reliable, is not objective.

A perfectly reasonable move at this juncture denies the assumption that image-making processes are objective only if they are nonconceptual. Objectivity is consistent with observations or data being theory-laden, otherwise few if any processes of data capture are objective. Moreover, mechanical image-making processes are selective in what they represent and designing selective mechanisms usually involves the exercise of concepts of the selected determinables. Nobody designs fMRI systems without making use, as part of the design process, of the concept of neural activation. Nevertheless, as reasonable as it may be, this move fails to address what is worrying about lithic drawing. Lithic drawing requires the illustrator to 'read' or interpret each stone as part of rendering it on paper. The rendering therefore presents an interpreted data point, and the trouble is that one cannot establish a good empirical generalization on the basis of individually interpreted data points.

What we need, if lithic drawings are objective, is a notion of objectivity that is consistent both with the penetration of data by theory and also with the fact that lithic drawings issue from individual interpretations of stones.

Consider what is special about lithic drawing. Lithic illustrators must 'read' a stone's surface and then represent their reading using the conventionalized vocabulary of the system. But anyone who is familiar with flint knapping and with the vocabulary of lithic drawing – any archaeologist, for example – can do as much. The illustrator does something more and it is something that archaeologists who are not illustrators cannot do. The illustrator *draws* the stone. That is to say, the illustrator has an eye and hand that are able to make marks on the paper that capture the finely detailed contours and textures of the stone. These features are not ones of which the illustrator must have concepts. Thus, the content of a lithic drawing is not wholly a product of interpretation.

Something more is needed for an explanation of how lithic drawing is objective in a useful sense. The proposition that the contents of lithic drawings are not wholly products of interpretation does not erase the worry that some contents of lithic drawings are the products of interpretation and that the contents in question are exactly those that are meant to furnish evidence for archaeological hypotheses.

The worry is misplaced because the nonconceptual contents of lithic drawings interact with their interpreted contents in at least two significant ways. First, ‘readings’ of a stone are constrained by the fine features that are captured nonconceptually, so that the nonconceptual contents of the drawing constrain their conceptual contents. Second, the conventions of lithic drawing clearly differentiate between interpreted and non-interpreted elements, so that it is possible, when inspecting a lithic drawing with a trained eye, to see what alternative interpretations of the artifact are possible. That is to say, an imaging process captures data objectively when the products of the process, to the extent that they are also products of interpretation, have nonconceptual content that constrains interpretation and provides a basis for alternative interpretation.

This is a moderate objectivity and it may be worth having. Topper comments that "theory-ladenness in depiction… is a double-edged sword: on the one hand, concepts aid us in seeing what may otherwise be missed; on the other hand, they can also impede us in recognizing something that does not fit the given categories but which may, in fact, be sitting in front of our noses" (1996: 223). Lithic drawings embody interpretations that are very difficult to achieve from inspecting photographs, and this is what makes them so useful in archaeology. At the same time, however, they do not prevent our seeing alternative interpretations. In this, they possess a kind of objectivity – one that may be worth having, in particular, in social scientific inquiry, when interpretation is both ineliminable and positively desirable.

An important issue has been skirted so far. What weight is borne by the fact that the representations of stone tools used in archaeology are *images?* The answer is not merely that images show what the artifacts look like. That is news to nobody. Rather, the images show what the stones look like and also

capture data about their making in a format that is lends itself to empirical generalization. The case of lithic drawing shows that the two functions are not as far apart as might appear. Counterfactual dependence and non-conceptual content are two marks of the mimetic. They are also, in this case, marks of good evidence.

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FRENCH VERSION

Le Dessin dans les Sciences Sociales : L'illustration Lithique. Dominic Lopes

Résumé : Les images sont utilisées en sciences pour présenter des données. En prenant les sciences physiques et biologiques comme des paradigmes, on pourrait supposer que les images faites à la machine servent cet objectif mieux que les images faites à la main. Une approche de l'usage des images scientifiques, dont l'extension est plus large, au point d'inclure les sciences sociales, montre que cette supposition est fausse. Les dessins archéologiques d'artefacts humains, comme les outils de pierre taillée, sont préférés aux photographies. L'explication de ce fait est que le système humain de production de dessins est un outil observationnel doté de pouvoirs spéciaux

(Traduction de l'original en anglais de Nicolas J. Bullot)

Les images sont utilisées dans les sciences pour présenter des données. En prenant les sciences physiques et biologiques comme des paradigmes, on pourrait penser que les images réalisées par des machines servent toujours ce but bien mieux que les images dessinées à la main. Un examen plus large de l'utilisation des images scientifiques, prenant en compte les sciences sociales, montre que cette supposition est fausse. Les dessins archéologiques d'artefacts réalisés par des hominidés, comme les outils en pierre, sont préférés aux photographies. L'explication de ce fait est que le système humain de production de dessin est un outil d'observation possédant des pouvoirs spéciaux.

Une variété foisonnante d'images sert une variété d'objectifs scientifiques. Les images scientifiques comprennent les diagrammes géométriques, les images radar Doppler, les photographies de chambre à bulle, les scannérisations fMRI, les diagrammes de Feynman ou les cartes de paysages adaptatifs. Ceux-ci et d'autres images sont utilisés pour formuler des hypothèses, effectuer des démonstrations, décrire des installations expérimentales, confirmer des hypothèses, présenter et analyser des données, tester et calibrer des instruments, et communiquer des données, des résultats ou des hypothèses. On pourrait s'attendre à ce que dans tout contexte scientifique donné, certains types d'images servent mieux certains buts que d'autres types d'images. Ainsi une taxonomie complète d'images scientifiques révélera des connexions systématiques entre (1) les types d'image, (2) les buts, et (3) les contextes d'utilisation. Toutefois, les philosophes des sciences (contrairement aux historiens des sciences) n'ont mis l'emphase que sur deux petits coins de l'ensemble du terrain qu'une telle taxonomie devrait recouvrir – les diagrammes utilisés dans le raisonnement mathématique d'une part, et, d'autre part, les images faites à la machine utilisées pour présenter des données dans les sciences

physiques et biologiques. Une prise en compte des images tombant en dehors de ces zones de fixation est nécessaire pour progresser vers une taxonomie complète; cela apporte aussi l'opportunité de ré-examiner quelques analyses historiques et philosophiques standard des images scientifiques.

Le dessin lithique

Les illustrations archéologiques se distribuent dans deux catégories principales. Parmi ceux qui reçoivent la plus grande attention, il y a les "dessins de reconstruction" qui décrivent des scènes imaginées de chasse préhistorique, de recherche de nourriture, de feu de camp, d'élevage d'enfant et d'autres activités semblables. Selon Stéphanie Moser, ces dessins sont utilisés par les archéologues pour donner des arguments d'une manière spécifiquement visuelle en faveur ou en défaveur de l'attribution du statut d'hominien à des spécimens fossiles d'australopithèques ou à d'autres primates (Moser 1996, 1998). Alors que les dessins ne constituent ni ne présentent une preuve pour ou contre le statut d'hominien, ils semblent avoir été persuasifs, et ils constituent par conséquent une étude de cas fructueuse des images dans la rhétorique scientifique. Les images de découvertes archéologiques ont reçu moins d'attention. Les installations et les autres sites sont cartographiés et les bâtis sont décrits en fonction de vues reproduites à partir d'un plan architectural. De façon surprenante, toutefois, les artefacts (tels que les outils) sont presque invariablement dessinés – les photographies ne sont présentées que dans des ouvrages populaires, non professionnels (voir par exemple Waechter, 1976). Pourquoi les dessins sont-ils préférés? Quels rôles replissent-ils dans la pratique archéologique? Ces questions pourraient avoir différentes réponses pour différents types d'artefact, considérons ainsi le cas des outils de pierre taillée.

Les auteurs d'un manuel récent écrit pour les archéologues et les illustrateurs se sentent obligés d'expliquer pourquoi le dessin est préférable à la photographie en archéologie. Ils écrivent que là où la photographie "a le désavantage d'être non sélective", "un dessin peut véhiculer beaucoup plus d'information pertinente et comparable et peut être édité plus facilement qu'une photographie" (Adkins and Adkins 1989:6). Adkins and Adkins donnent deux explications de l'importance de la sélectivité pour représenter les artefacts. Premièrement, "un bon dessin décrit sélectivement les détails que le lecteur a besoin de voir et élimine les détails non pertinents, de telle sorte que l'illustration peut être comprise beaucoup plus facilement"(Adkins and Adkins 1989:7). Deuxièmement, "le but de l'illustration est de véhiculer non seulement de l'information mais aussi une interprétation de cette information" (Adkins and Adkins 1989:5). La première explication est dans une relation problématique avec le fait que les photographies sont communes dans les publications populaires, où une compréhension facile semblerait primordiale. La seconde explication conduit à des interrogations sur le rôle et la nature de l'interprétation en archéologie.

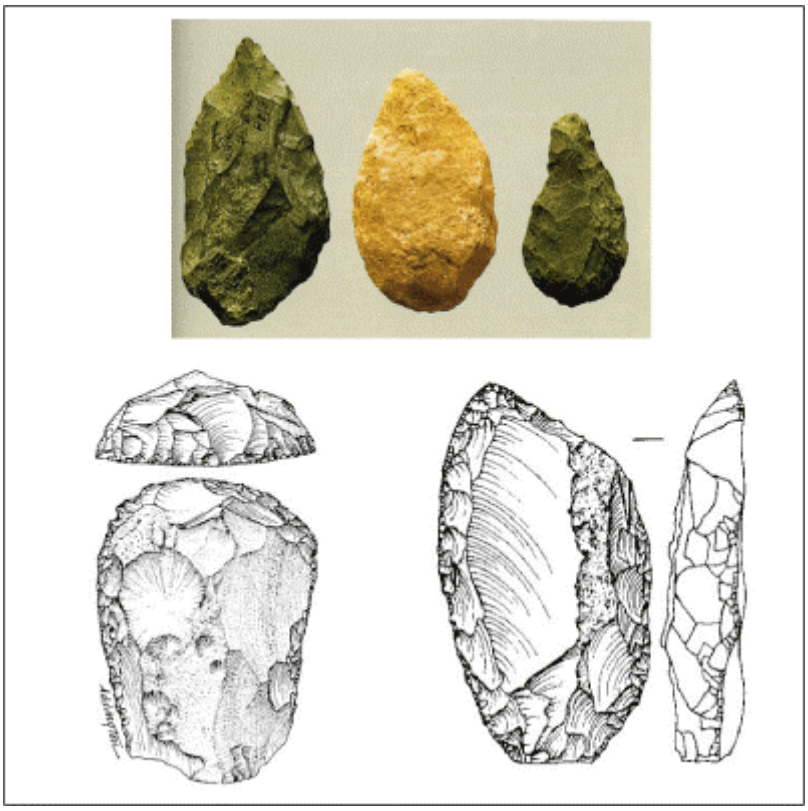

Figure 1

Pour trouver à quoi sert un artefact, examinez les détails de sa fabrication : ce principe fonctionne aussi bien pour les artefacts qui sont des dessins d'artefacts que pour les artefacts qui sont dessinés.

Heureusement, les détails dans le dessin lithique sont lucidement et systématiquement mis en avant dans le manuel pour illustrateurs archéologiques de Lucile Addington (Addington, 1986). Les outils de pierre taillée sont dessinés soit par un archéologue ou, plus souvent, par un collaborateur avec une formation spécialisée dans le dessin lithique. Addington possède une telle formation d'illustratrice, et son manuel est fait pour être lu par des illustrateurs en formation et aussi par des archéologues cherchant des instructions explicites sur les principes du dessin lithique.

Comme l'indique clairement le manuel, les dessins lithiques doivent servir deux fins. Premièrement, un "langage universel" est nécessaire pour garantir que les dessins de n'importe quel laboratoire puissent être comparés aisément et d'une manière fiable (Addington 1986:ix, xiii-xiv). Notez que la photographie répond parfaitement à ce besoin. Deuxièmement, les dessins doivent véhiculer toute l'information pertinente, et uniquement celle-ci, sur les objets représentés. Ce qui est pertinent est déterminé par la nature de l'objet représenté et le rôle de la représentation dans son contexte d'utilisation. Les dessins lithiques représentent des artefacts et ils les représentent comme des artefacts – comme des objets dont la forme est largement le résultat d'une activité humaine délibérée – dans un contexte où ils sont des objets d'étude pour leurs caractéristiques "artefactuelles". Comme le remarque Addington, "des artefacts correctement dessinés sont invariablement plus informatifs que les photographies pour illustrer un travail préhistorique de tailleur de pierre tout comme la forme d'un artefact et des traits caractéristiques" (1986:ix).

La taille implique de frapper de façon répétitive une pierre, généralement un silex, pour enlever les éclats. Ou bien les éclats eux-mêmes sont taillés à nouveau ou la pierre d'où ils sont pris est taillée pour finir par laisser un pattern utile formé de cicatrices d'éclats. La pierre est ainsi "retouchée" pour aboutir à une forme finie, et elle peut ensuite être soumise à la réutilisation ou à l'érosion qui change sa forme ou sa surface. Ecailler une pierre n'est pas une façon de passer le temps. Les outils de pierre servaient de couteaux, de fendoirs, de grattoirs, de pointes de flèches et de marteaux. Ainsi un dessin lithique doit-il montrer comment une pierre a été taillée et dans quel but. Pour ce faire, il doit

montrer : l'échelle; le pattern, la séquence, la direction et la force des coups sur la pierre; le bulbe et la plate-forme de percussion; les zones de retouche, de rupture, et de troncation; les zones d'affûtage, de martèlement ou d'abrasion; les fractures causées par l'échauffement; les effets des matériaux; et le piquage et le reflet en forme de faucille. Les fossiles, la coloration variée, la patine, les veines, les raies et la cristallisation ne sont pas montrés.

Puisque des caractéristiques pertinentes de chaque pierre doivent être montrées d'une façon qui permet les comparaisons fiables, le dessin lithique se conforme à plusieurs conventions. L'objet est illuminé de 45° dans la partie supérieure gauche. La surface ventrale de la pierre est placée en bas du dessin et, quand des vues multiples sont montrées, les vues de profil sont montrées près des bords qu'elles déploient. L'échelle est fixée à 1:1. Les types de papier, instruments de dessin, et techniques de crayonnage et d'encrage sont standardisés. Finalement, un vocabulaire représentationnel bien défini est strictement suivi. Le pointillé indique le cortex de la pierre, avec une plus grande densité indiquant une rudesse plus grande. Des tracés montrent des cicatrices d'éclats dans leur séquence de fabrication. Des lignes imitant les rides causées par l'écaillage indiquent la direction d'un coup, et, par leur épaisseur, sa force approximative. Ils sont montrés attachés à seulement un côté de cicatrice d'écaille et à deux côtés d'une surface bulbaire. Les lignes directionnelles incurvées indiquent des surfaces cassées net et les fractures thermiques sont montrées par des lignes en forme d'araignée, de rayons ou par des spirales. En outre, les pointillés et les lignes directionnelles ont un but double, puisqu'elles transmettent le volume de la pierre aussi bien que ses détails de surface. Ce vocabulaire de marques aide à répliquer l'aspect de l'objet. Il est augmenté par un vocabulaire non mimétique. Les flèches pointent vers les bulbes de percussion. Des lignes brisées montrent où les fragments cassés d'une pierre se correspondent. Des coches coordonnent les vues multiples de telle sorte que les caractéristiques clé de la pierre peuvent être mises en correspondance.

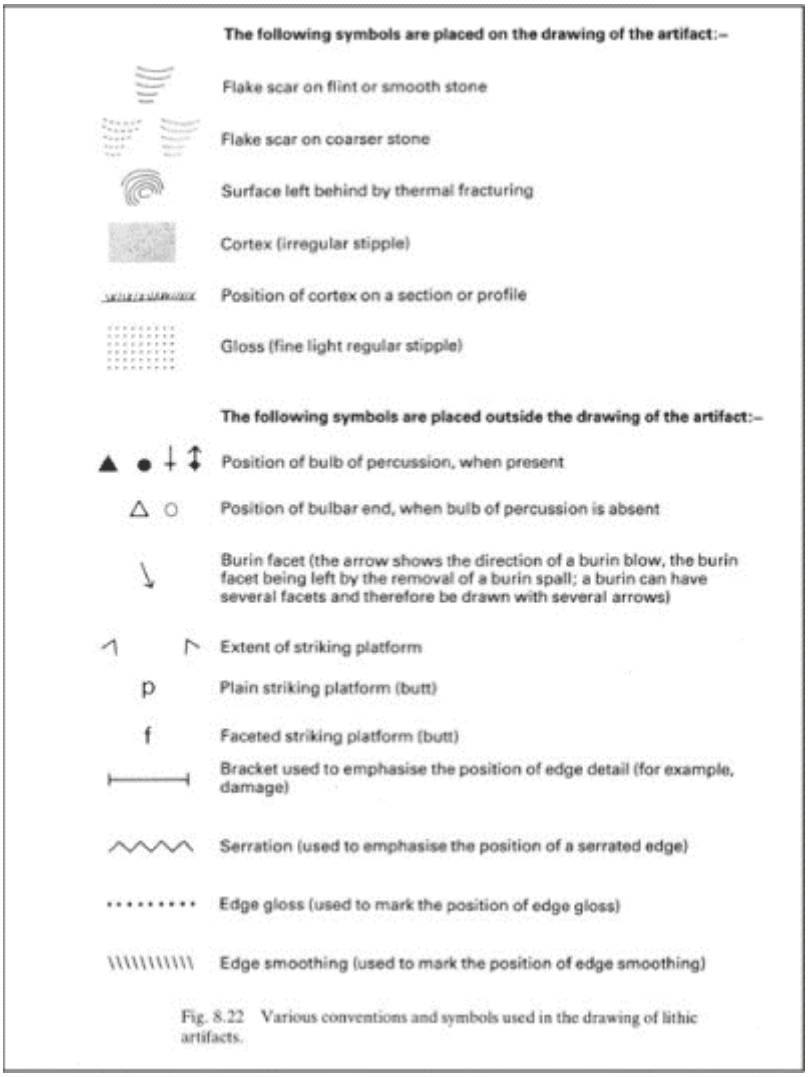

Fig. 8.22 Various conventions and symbols used in the drawing of lithic artifacts.

Figure 2

Pour rendre un dessin conforme à ces règles, un illustrateur doit apprendre à 'interpréter' ou "lire les surfaces d'un artefact" (Addington 1986:2). Il ou elle doit surveiller les cicatrices d'éclat et ce qu'elles indiquent sur leur fabrication; sur les différents types d'éclats, y compris retouche, cassement sec, troncation, aiguisement, et fente; sur les effets de l'érosion et de la réutilisation, qui doivent être distingués des effets de la taille; et finalement sur les défis pratiques qui se posent au tailleur qui taille une pierre particulière, puisque chaque pierre est différente et que toutes les 'lectures' d'une pierre ne sont pas cohérentes avec ce qu'un tailleur peut faire avec elle. Un archéologue qui n'est pas aussi illustrateur est capable d'une telle lecture, mais seul un illustrateur peut englober la lecture dans un dessin.

Le dessin dans l'histoire et la philosophie

Les dessins lithiques sont un défi pour plusieurs généralisations qui ont été faites sur les images scientifiques et de ce fait soulèvent plusieurs questions philosophiques intéressantes.

Selon une histoire standard (voir Topper 1996), les premières images scientifiques sont pour la plus grande partie d'entre elles des dessins qui représentent des types – les dessins botaniques et anatomiques en sont le meilleur exemple. Dans beaucoup de cas, ce qui est représenté a des caractéristiques qu'on ne trouve jamais dans un spécimen particulier – c'est une idéalisation. Dans d'autres cas, l'objet représenté a les caractéristiques d'un spécimen particulier, mais il est entendu que nous devons nous abstraire de sa particularité et le voir comme représentant un type. Plus tard, avec l'invention de l'imagerie mécanique, de nombreuses images scientifiques réalisées représentent des individus particuliers et non plus des types : nous n'avons pas à nous abstraire de leurs caractéristiques particularisées puisqu'elles présentent la preuve des individus particuliers eux-mêmes. Elles fixent les observations pour un examen ultérieur.

Parmi les exceptions à cette histoire, il y a les images dessinées à la main utilisées dans les preuves mathématiques et la présentation de théorèmes ou hypothèses, d'Euclide à Feynman. Toutefois, ces exceptions confirment l'explication de l'histoire : les dessins représentent des types; la représentation d'individus particuliers vient plus tard, avec l'imagerie mécanique. Ainsi les dessins ont-ils encore une place à l'âge de la fabrication d'images mécaniques parce qu'ils représentent des types. David Topper écrit: "en dépit de l'invention de la photographie il y a un siècle et demi (…) l'artiste a encore un rôle à jouer dans l'illustration, car la caméra capture un spécimen individuel (le particulier) là où un artiste peut décrire un archétype" (1996:234).

Il y a d'autres exceptions. Les dessins de la lune qui ont été faits par Galilée, qui furent réalisés pendant la première période, représentent un individu particulier. La découverte que les taches noires de la lune sont des ombres causées par les montagnes a requis une attention méticuleuse aux caractéristiques spécifiques de ces taches. Dans la période récente, Wegener a utilisé les dessins de particuliers dans ses descriptions de la dérive continentale. Ici encore, les détails exacts de la forme des lignes côtières continentales était nécessaire pour établir que les continents étaient autrefois des parties d'une seule masse de terre. Néanmoins, l'idée principale tient. Si elles avaient été disponibles, les photographies auraient fonctionné au moins aussi bien et probablement mieux pour les objectifs de Galilée et de Wegener.

Un autre élément de l'histoire mérite d'être mentionné. Il est naturel de considérer les images scientifiques comme de l'art, quand ce sont des dessins, spécialement si ce sont des dessins d'après nature et plus spécialement encore s'ils sont anciens. Du moins, il est naturel dans ces cas de mettre en jeu les questions de savoir s'ils sont de l'art et ainsi des liens entre l'art et la science. Ainsi pourrait-on penser de l'histoire qu'elle met graduellement une distance entre les images artistiques et les images scientifiques. Le point n'est pas que les images scientifiques en viennent à manquer de valeur esthétique – au contraire, elles ont souvent une grande valeur esthétique. C'est plutôt que la valeur esthétique qu'elles ont est simplement un effet secondaire de leur fonction première.

Jumelée avec l'histoire standard se trouve une épistémologie standard. L'épistémologie a deux fourches, les deux concernant ce qui compte comme étant bonne preuve pour la généralisation empirique. La première fourche est l'affirmation qu'une bonne preuve pour une généralisation

empirique devrait sortir d'un processus fiable. Ajoutez le postulat que les processus sont le plus fiable quand ils dépendent de moyens mécaniques de prises de mesure, et nous avons une bonne raison épistémique de préférer les photographies aux dessins quand nous voulons capturer des données pour une généralisation empirique. La seconde fourche est l'affirmation qu'une bonne preuve pour une généralisation empirique devrait être issue d'un processus objectif. Ajoutez l'hypothèse que les processus de données images sont objectifs seulement s'ils produisent des images avec un contenu non conceptuel. Une image a un contenu non conceptuel juste dans le cas où avoir le contenu qu'elle a ne requiert pas que celui ou ce qui fait l'image possède des concepts des propriétés représentées (Lopes 1996: ch. 9). Si les photographies, contrairement aux dessins, ont un contenu non conceptuel, alors nous avons une autre bonne raison épistémique d'utiliser les photographies plutôt que les dessins pour capturer des données pour la généralisation empirique. Notez que les fourches sont liées si l'objectivité promeut la fiabilité.

L'épistémologie renforce la thèse d'après laquelle les dessins laissent largement place à des images faites à la machine en sciences quand les images sont utilisées pour capturer des données pour la généralisation empirique. La capture des données doit être fiable et objective; les machines ont ces deux vertus, les dessinateurs n'ont aucune des deux. Cela explique aussi l'observation que les images scientifiques faites à la machine et les dessins artistiques suivent des voies divergentes : les restrictions épistémiques sur la fabrication d'images scientifiques l'emporte sur les préoccupations esthétiques qui donnent la règle à l'art.

Les illustrations lithiques sont des dessins, mais ils ne représentent pas des types; au contraire, ils capturent des données sur des particuliers qui sont utilisés à faire des généralisations empiriques. Alors que les dessins sont souvent élégants, intéressants visuellement, même beaux, ceci est accessoire par rapport à leur but premier. De plus, les dessins lithiques sont relativement nouveaux, post-datant l'invention d'une photographie bon marché et fiable: il y a une réelle préférence pour eux par rapport aux alternatives faites à la machine. De cette manière, ils sont des exceptions à l'histoire standard des images scientifiques et l'explication de cette histoire. Et ils défient l'épistémologie standard, aussi. Ils sont faits à la main, ce qui semble relativement peu fiable, et leur contenu dépend des ressources conceptuelles des illustrateurs qui les font, ce qui nous amène à penser qu'ils ne semblent pas être objectifs. S'ils sont fiables et objectifs, en prenant en compte les buts pour lesquels ils sont faits, alors une explication doit être donnée.

Dessin, fiabilité et objectivité

On pourrait dire que les dessins lithiques échouent à capturer les bonnes données pour une généralisation empirique. Supposons que ce n'est pas le cas. Faire cette affirmation a un prix, bien sûr. Nous devons démontrer la qualité épistémique des dessins lithiques.

Les images appartiennent à des systèmes. Chaque système est individué par des règles qui précisent comment les images sont faites dans le système, en marquant mécaniquement les surfaces, par un processus numérique, ou par le dessin manuel, de telle sorte que des caractéristiques sélectionnées déterminables d'objets ou de scènes sont représentés. Certains systèmes sont hiérarchiquement inclus dans d'autres. Par exemple, la photographie en couleur est incluse dans la photographie en noir et blanc, puisque la photographie en couleur est la photographie en noir et blanc avec l'addition de ressources pour dépeindre teinte et saturation. Arrêter à ce point, bien sûr, nous laisse avec une conception grossière des systèmes de production d'images, et beaucoup plus pourrait être dit – et a été dit (voir Lopes 1996, Kulvicki à paraître). Mais ceci est suffisant pour le moment. Un processus de production d'image génère des images appartenant à un système. Il est fiable en représentant certaines propriétés déterminables du monde uniquement dans le cas où les contenus des images dans le système dépendent "contrefactuellement" des déterminés du déterminable. Le contenu d'une image est "contrefactuellement" dépendant de trajectoires uniquement dans le cas où il représente des trajectoires d'objets et représenterait aussi les trajectoires si elles étaient différentes. Un contenu d'image est "contrefactuellement" dépendant du contour d'une pierre uniquement dans le cas où il représente ce contour et représenterait aussi le contour si la pierre avait une forme différente. Les processus de production d'image qui génèrent des images dont les contenus sont "contrefactuellement" dépendants de cette manière sont fiables.

Une manière de s'assurer du fait qu'un processus de production d'image est fiable consiste à construire un dispositif qui génère les images mécaniquement. Tous les dispositifs d'imagerie ne fonctionnent pas de manière fiable, mais beaucoup le font. Une photographie fondée sur un film est hautement fiable pour beaucoup de déterminables, par exemple. Toutefois, il est erroné de supposer que la seule manière d'assurer une dépendance contrefactuelle passe par une causation mécanique. Un système d'images fait par le dessin est fiable pour représenter, disons, une forme, tant que les dessins représentent une forme d'objet et l'auraient représenté avec une forme différente s'il avait une forme différente. Les systèmes de dessin remplissent-ils ces conditions? Certains le font.

Les conventions et techniques du dessin lithique assurent sa fiabilité par rapport à une sélection de déterminables, comme l'échelle d'une pierre, sa forme globale, la forme et l'emplacement des cicatrices d'éclat et les retouches, et l'emplacement de caractéristiques comme les fractures thermiques. C'est à dire qu'un illustrateur qui suit les règles produira des dessins qui présentent les caractéristiques pertinentes des pierres et qui aurait aussi montré ces caractéristiques comme différentes, dans le cas où elles étaient différentes. Cette dépendance contrefactuelle n'est pas un pur hasard; elle est une conséquence de la conformité aux conventions et techniques du dessin lithique; et c'est pourquoi le dessin lithique est un processus fiable.

Cette explication des raisons pour lesquelles le dessin lithique est fiable pourrait ébranler son objectivité. Après tout, en se conformant aux conventions du dessin lithique, un illustrateur doit exercer des concepts comme 'cicatrice d'éclat', 'retouche', et 'fracture thermique'. Il ne dessine pas simplement l'aspect de la pierre; il dessine son aspect étant donné une lecture de celui-ci dans les termes de ces concepts. (Vous et moi ferions de mauvais illustrateurs lithiques, quelque soient nos capacités de dessiner, car il nous manque les concepts requis). Toutefois, nous avons supposé que le dessin, pour être objectif, ne peut requérir qu'un illustrateur possède des concepts des traits caractéristiques décrits. Par conséquent, le dessin lithique, justement parce qu'il est fiable, n'est pas objectif.

A cette jonction, il serait parfaitement raisonnable de rejetter l'hypothèse d'après laquelle les processus de fabrication d'images sont objectifs seulement s'ils sont non conceptuels. L'objectivité est compatible avec des observations ou des données chargées de théorie, autrement, peu de processus de collectage de données sont objectifs. De plus, les processus mécaniques de fabrication d'image sont sélectifs dans ce qu'ils représentent et la conception de mécanismes sélectifs implique en général l'exercice des concepts des déterminables sélectionnés. Personne n'utilise des systèmes fMRI sans utiliser, comme une partie du processus de conception, le concept d'activation neurale. Néanmoins, aussi raisonnable qu'elle puisse être, cette démarche échoue à prendre en compte ce qui est problématique dans le dessin lithique. Le dessin lithique requiert de l'illustrateur qu'il 'lise' ou interprète chaque pierre comme une partie du travail d'inscription et de rendu sur le papier. Le rendu de ce fait présente un point de données interprétées, et le problème est qu'on ne peut pas établir une bonne généralisation empirique sur la base de points de données individuellement interprétées.

Ce dont nous avons besoin, si les dessins lithiques sont objectifs, c'est d'une notion d'objectivité qui soit cohérente avec la pénétration des données par la théorie et aussi avec le fait que les dessins lithiques sont issus d'interprétations individuelles des pierres.

Considérez ce qui est spécial dans le dessin lithique. Les illustrateurs lithiques doivent 'lire' une surface de pierre et ensuite représenter leur lecture en utilisant le vocabulaire "conventionnalisé" du système. Quiconque est familier avec la taille du silex, ou avec le vocabulaire du dessin lithique – tout archéologue par exemple – peut en faire autant. L'illustrateur fait quelque chose de plus et c'est quelque chose que les archéologues qui ne sont pas illustrateurs ne peuvent faire. L'illustrateur *dessine* la pierre. C'est à dire que l'illustrateur a un œil et une main qui sont capables de faire des marques sur le papier qui capturent les contours finement détaillés et les textures de la pierre. Ces traits caractéristiques ne sont pas de ceux dont l'illustrateur doit avoir des concepts. Ainsi, le contenu d'un dessin lithique n'est pas totalement un produit d'interprétation.

Quelque chose de plus est nécessaire pour une explication de la manière dont le dessin lithique est objectif dans un sens utile. La proposition d'après laquelle les contenus des dessins lithiques ne sont pas pleinement des produits d'interprétation n'efface pas le souci d'après lequel certains contenus de

dessins lithiques sont les produits d'un travail d'interprétation et que les contenus en question sont exactement ceux qui sont supposés fournir des preuves pour des hypothèses archéologiques.

Le souci est mal placé car les contenus non conceptuels des dessins lithiques interagissent avec leurs contenus interprétés au moins de deux manières significatives. Premièrement, les 'lectures' d'une pierre sont contraintes par les traits caractéristiques fins qui sont saisis non conceptuellement, si bien que les contenus non conceptuels du dessin contraignent leurs contenus conceptuels. Deuxièmement, les conventions du dessin lithique font clairement la différence entre les éléments interprétés et les éléments non interprétés, de telle sorte qu'il est possible, quand on inspecte un dessin lithique avec un œil entraîné, de voir quelles interprétations alternatives de l'artefact sont possibles. C'est à dire qu'un processus de production d'image capture les données objectivement quand les produits du processus, dans la mesure où ils sont aussi des produits de l'acte d'interprétation, ont un contenu non conceptuel qui contraint l'interprétation et fournit une base pour une interprétation alternative.

C'est une objectivité modérée et il pourrait être valable de l'avoir. Topper commente en affirmant que "la charge théorique dans l'image (…) est une épée à double tranchant: d'une part, les concepts nous aident à voir ce qui autrement serait manqué; d'autre part, ils peuvent aussi nous gêner pour reconnaître quelque chose qui n'entre pas dans des catégories données mais qui peut, en fait, se trouver juste devant notre nez" (1996: 223). Les dessins lithiques incorporent des interprétations qui sont très difficiles à effectuer par l'acte d'inspecter des photographies, et c'est ce qui les rend si utiles en archéologie. En même temps, toutefois, ils ne nous empêchent pas de voir des interprétations alternatives. En ceci, ils possèdent une sorte d'objectivité – qu'il peut être utile d'avoir, en particulier, dans l'enquête en sciences sociales, quand l'interprétation est à la fois inéliminable et positivement désirable.

Un problème important a été contourné jusqu'ici. De quel poids est le fait que les représentations d'outils de pierre utilisées en archéologie sont des images? La réponse n'est pas simplement que les images montrent à quoi ressemblent les artefacts. Cela n'est nouveau pour personne. Plutôt, les images montrent à quoi ressemblent les pierres et également capturent des données sur leur fabrication dans un format qui se prête lui-même à la généralisation empirique. Le cas du dessin lithique montre que les deux fonctions ne sont pas éloignées comme elles pourraient le sembler. La dépendance contrefactuelle et le contenu non conceptuel sont deux marques de la mimétique. Ils sont aussi, dans ce cas, marques d'une bonne preuve.

Discussion

design properties, perceivable properties and inferred properties
John Zeimbekis
May 24, 2005 9:35 UT

Suppose that a region R of a stone's surface has the determinate grainy property G. A drawing of the stone will have a corresponding region R* possessing a certain graphic property F. What will R* 's possession of F represent?

First, it will represent R's possession of the property of being grainy, though not R's being G specifically; in fact, a photograph would represent a narrower determinable of graininess/roughness than the drawing does. Second, F will also represent something else, which is the result of interpretation—for example, it will *also* mean "is the result of knapping", as distinct from "is the result of a some later erosion or accident". Call this represented (and inferred) property 'P'.

The question arises whether the design or graphic property F of the drawing can represent both G and P at the same time. The lines on R* that represent P will occupy space that the lines representing G would otherwise occupy. Therefore the drawing can represent the inferred artifactual properties only *at the expense* of G (or at the expense of other perceivable properties).

So if the interpretation (of G as being P) turns out to be false and we have to re-interpret G, and all we have to go on is the drawing, we will no longer have any representation of G to go on. A few such mistakes in the right places can skew an entire theory (eg about the migrations of a certain group); and it's quite plausible that all the finds from a certain area would be drawn and catalogued by the same illustrator committing similar errors. So for epistemological purposes, the photograph is a better record than the drawing: it preserves data throughout changes in interpretation.

So, why not add the 'discrete', on/off -type information (eg: 'intentional flaking' / 'non-intentional faking') by superposing region-outlines over a photograph, naming them with letters, and then adding the interpretive information in writing *outside* the photograph? This would combine analog and conceptual information, but now, if the conceptual information turned out to be false, we would not have lost the analog information.

there are two questions here

Dominic Lopes
May 26, 2005 17:27 UT

John is right: every image is selective. Photographs are in general less selective than line drawings. The question I raise and attempt to answer is why lithic drawings are nevertheless preferred to photographs of the stones. The answer is that they represent properties of the stones that the photographs cannot.

It is a consequence of this, as John observes, that some information is lost. It is possible that the information that is lost about a collection of stones could be the very information needed to prevent the adoption of a skewed theory about the stones. However, neither reliability nor objectivity require anything as strong as the impossibility of making mistakes such as this one. (We are fallibilists, aren't we?)

John's observation also raises the question, which I did not raise, of why some other imaging system is not used, such as photographs with marks drawn on them. That's a harder question to answer. I think we have to attend to the specific epistemic needs of archaeologists, as they see them, and I know of no discussion among them of these kinds of images. However, I have been told that digital imaging systems are being developed for lithic illustration. My understanding is that what they will output will look much like line drawings.

Addressing the key question: Why are drawings so good?

Roberto Casati
May 25, 2005 11:51 UT

Lopes' fine contribution gives a clear idea of the type of work that should be done at the interface of the study of art and the study of cognition; there is in the field the urgent necessity of focusing on small-scale case-studies, in order to ask the good questions, before trying the large generalizations.

I am just concerned about the fact that the opening issue does not seem to have been addressed completely. Lopes writes:

"Archaeological drawings of human artifacts, such as stone tools, are preferred to photographs. The explanation of this is that the human drawing system is an observational tool with special powers."

His argument is then mostly devoted to successfully rebutting objections to the effect that conceptual mediation in producing a drawing are not to be considered a necessary impairment to drawings' reliability. However, little is said about the "special powers" of the human drawing system that make it suitable for depicting stones.

On the one hand, a full account should explain why lines in line drawings are so good for the viewer, and why they are so hard to extract by a non-human system (there are no good algorithms so far for creating a convincing line drawing from a photograph).

On the other hand, in the long run drawings may well be replaced by other instruments. For instance, if a lithic drawing needs to convey not visual information in the first place, but information about the causal history of the stone (marks, etc.), in such a way that such information can be compared across different research groups, then it is likely that sets of purely propositional descriptors will be better, especially for the needs of running large scale comparisons and extract statistically significant data.

the machines may not be far behind
Shimon Edelman
May 30, 2005 19:22 UT

The two questions brought to the fore of the discussion by Casati -- why are line drawings so effective, and what other instruments they may be replaced by -- are useful elaborations on the issues raised by Lopes. I would like to offer one remark on each of these questions. First, Casati is right in observing that the algorithmic problem of turning an image into a line drawing is not yet solved. Significant progress is, however, being made: see this work by DeCarlo et al Second, concerning possible alternatives to drawings: I am not sure what Casati means by "propositional descriptors", but I suspect that not much good can come out of something as alien to vision (and to natural cognition) as formal logic. Luckily, massive data sharing and analysis can be supported by existing techniques in mathematical statistics of shape spaces, an area pioneered by David Kendall (for an introduction, see David Kendall)

Drawing as editing
Thomas Stoffregen
Jun 1, 2005 7:28 UT

Why is it that in some fields of science, drawings are preferred to photographs? This is true in archeology, and also in paleontology, where fossil remains often must be "reconstructed" to recover 3-d form from 2-d remains. A distant but related example is caricature, drawings that highlight personality traits by exagerating physical features.

In all these cases, I think, the value of drawing is not in the hand of the artist. Rather, it is in the artist's ability to appreciate meaning, and to distinguish meaning from simple form. The camera provides a record that is accurate, but completely non-discriminating. The camera records all details, but meaningful and irrelevant. The contribution of the artist is to detect those elements that are meaningful and to record only those, editing out the great mass of irrelevant detail (in the case of caricature, meaningful details are exagerated, but the principal of selective presentation is the same). Computer vision applications are capable of extracting meaning from optics only to the extent that meaning is programmed into them by people. Our ability to see meaningful features of things is (so far) much greater than our ability to quantify meaning in syntactic codes (e.g., programming languages) and that is why artists remain better than computers for this type of task. A crucial fact is that detecting the meaning of objects (artifacts, or fossils) often depends upon many years of training and experience. Moving this level of perceptual sophistication from the head of the artist into software is --at best-- a very challenging task. Increasing the sophistication of computational algorithms, by itself, cannot lead to success -- there is no algorithm for meaning.

Motor response theories and relevance theories: two possible answers
alessandro pignocchi
Jun 1, 2005 15:45 UT

Here are two possible answers to Casati's question, why are drawings so good?

A first answer is in terms of relevance theory (Sperber and Wilson, 1986): cognitive effects being equal, a drawing exacts lower processing costs relative to a photograph of the same object.

The second answer is in terms of a motor simulation theory of drawing perception: perceiving a drawing activates motor responses that mirror the way the drawing has been produced - even a very rudimentary knowledge of the way drawings are made can activate those responses. When we see a drawing, we immediately access the way it was made, hence we "master" it. (The two theories are, of course, in opposition in the way in which theory-theories of mind are opposed to simulation theories.)

Reply to Thomas Stoffregen

Gloria Origgi

Jun 1, 2005 16:42 UT

Stoffregen writes: "Our ability to see meaningful features of things is (so far) much greater than our ability to quantify meaning in syntactic codes (e.g., programming languages) and that is why artists remain better than computers for this type of task"

Still, lithic drawing as many other cases of "scientific" drawing relies on partly codified meaninigs, as the Lucile Addington's manual mentioned by Lopes shows. Only trained archaeologists can extract the relevant, meaningful features from the stones, and in this sense, they have been partly "programmed" through their archaeological studies.

There are so many manual abilities in many domains of science that are difficult to automatize, such as for example collecting butterflies for entomological purposes, but this is a much broader question and I don't see why it should be so obvious that in the case of lithic drawings manual abilities will always have an advantage on computer drawing.

Also, there are two questions that are often confused in this debate: one is why drawing should be better than photographs (and the general answer is: because it allows to discriminate relevant features that a photo, although accurate, is not able to make salient enough). The other question is: is manual drawing intrinsically superior to machine drawing (and for this second question, I have not seen a clearcut answer)

Motor Response Theories

Dominic Lopes

Jun 2, 2005 19:03 UT

Alessandro's suggestion that "perceiving a drawing activates motor responses that mirror the way the drawing has been produced" is intriguing. It's one way to model the connection between perception and action.

However, is it really the case that perceptions of artifacts activate motor responses mirroring the motor actions that produced the artifact? That's hard to believe as a general rule. So why would drawings be different from other artifacts?

Why not say instead that perceiving anything activates motor responses that mirror the motor actions that are part of using the object? First, this is not restricted to artifacts. Second, this is how so-called "canonical" mirror neurons are said to work.

What about drawings? Maybe perceiving a drawing activates motor responses that mirror the way the object depicted is used.

A « motor reading » of drawing. (reply to Dominic Lopes.)
alessandro pignocchi
Jun 3, 2005 14:29 UT

Dominic is certainly right saying that “maybe perceiving a drawing activates motor responses that mirror the way the object depicted is used”. However, my hypothesis concerns the drawing in itself, not what it represents. The goal is to try to give an original answer to the question “why a drawing is better than a photography”. This hypothesis (perceiving drawing activates motor responses that mirror the way the drawing has been produced) is quite speculative, but has some empirical support : See for exemple Freyd 1988 :
http://dynamic.uoregon.edu/~jjf/articles/babcockfreyd88.pdf

Freyd uses small hand-made abstract characters (that looked like chinese letters). Two versions of the same character were displayed: one of the constitutive strokes was drawn in two opposite directions. The final visual aspect of the character was in each case quite similar.

When naive subjects were asked to reproduce the character, they draw the stroke in the same direction as it had been drawn in the model. Subjects were also able to describe verbally the direction in which the strokes had been drawn, but less efficiently than in the implicit direct-action task. Replaced in the present debate about motor simulation in perception, it is tempting to say that Freyd’s subjects directly access motor action that “mirrors the way the drawing has been produced”. I hypothesize that the same happens with other dynamic information regarding the production of the drawing, like the speed of the hand, the pressure,... I think drawing is different from other artifacts because everybody knows how to use a pen, even in a rudimentary way. The hypothesis of a “motor reading” of drawing could partly explain why a drawing is “better” than photography. For example, a drawing, via ‘motor reading’, can attract attention on some original parts of the object it represents, and then “teaches us [something about] how to see stones”.

Epistemology and Archaeology
Gloria Origgi
May 25, 2005 12:05 UT

I am a bit puzzled by some passages of Dominic Lopes’ text concerning epistemology. He depicts a “standard epistemology” position as a default position about the role of images in science. According to Lopes standard epistemology claims: *“that good evidence for an empirical generalization should issue from a reliable process. Add the assumption that processes are the more reliable as they rely on mechanical means of taking measurements, and we have a good epistemic reason to prefer photographs to drawings when we wish to capture data for empirical generalization. The second fork is the claim that good evidence for empirical generalization should issue from an objective process.”*

I don’t think this corresponds to any serious position today in the epistemology of archaeology (see for example Alison Wylie’s book: *Thinking From Things. Essays in the Philosophy of Archaeology*University of California Press, 2002). The epistemology of archaeology emphasizes today the peculiarity of archaeological evidence, and the role of background and collateral knowledge in interpreting archaeological data as evidence. Drawing in archaeology may have the same role as writing in ethnology (cf. J. Clifford and G.E. Marcus *Writing Culture*1986, University of California Press), that is, they are irreplaceable interpretive means to access past and distant cultures. Of course in the case of Galileo’s drawings of the Moon, a photo could have been better evidence than a drawing, but nobody thinks that the constraints on evidence of astronomy should be of the same kind

of those of archaeology. That's why social sciences are an interesting and distinct epistemological subject.

A level issue
Roberto Casati
May 25, 2005 13:59 UT

Gloria Origgi's criticism may be addressed to a larger project that the one pursued by Lopes? There may be a large issue of the epistemological status of archaeology, and smaller issues of the epistemological status of some tools used in archaeology – such as drawings or narratives. For drawings, Lopes' account seems to be just right: it explains why drawings are preferred to photographs, and why they are preferred to verbal descriptions.

An aside: A general notion that is important to account for some drawings and scientific images (and equally important, but in a different sense, for accounting for some narratives) is the notion of token-tracking. Lithic drawings, as well as drawings of archaeological items, must track their token; there can be, of course, drawings that represent some sort of idealized piece of carved stone or idealized Greek vase, but in the norm we find token-tracking drawings. Of course, photographs are token tracking (and aren't they used in the social sciences? Surely one would not dismiss photographic evidence for recent events?)

Is the drawing token-tracking?
John Zeimbekis
May 25, 2005 15:28 UT

I'm not sure that the drawings are token-trackings. The token is tracked while the drawing is being drafted, but not necessarily when someone else then looks at the drawing. The stone tool as a token has determinate properties and it's important to represent them as accurately as possible precisely because they are the traces of the actions we want to reconstruct from them. Even a photograph cannot represent those properties perfectly, so it seems to be normative and approximative just how much the token can be tracked. But how – if tracking is defined normatively for knowledge purposes, AND photography is available as a tracking means – can the drawing count as adequate tracking ?

token tracking, accuracy, and detail
Dominic Lopes
May 26, 2005 17:36 UT

Why should token-tracking require accuracy and fine representational grain? It seems to me that a causal connection between stone and representation suffices. Of course, we need a way to see that the causal connection obtains. Archaeologists manage that by giving each artifact a tracking number and labelling both drawings and photographs accordingly.

Baffled questions
Jose Luis Guijarro
May 25, 2005 19:33 UT

At the end of last year's Summer course on "reference to objects" held at the isle of Oleron (France), the organizer, Roberto Casati, told me that the next such course would be on scientific images, an idea that made me, a mere chomskyan linguist, wonder what on earth this topic could really amount to.

Reading Dominic Lopes' first contribution to this virtual seminar (a preparation for next Summer course or a virtual Ersatz of it?) has indeed made clear to me what some of the problems of this topic look like, although his remarks and considerations, mighty interesting as they are to an absolute neophyte, have increased my bafflement on how many ways one is able to interpret reality in cognitive terms.

I am almost certain that any comment I might be able to produce will therefore look like the babblings of a child, if not worse, to most of the learned participants, but let me try to pinpoint some of them anyway.

1) It is certainly the case that, when we travels and try to collect images which will make us remember our trip, postcards have less evoking force than pictures taken with our own camera, for these add the interpretation power of our environmental recollections (i.e., a context) to the images. But if you have some sort of "artistic" capacity and you are able to take images of places by drawing them, this interpretation gives them a lot more evoking feeling whenever you return to them. What I want to ask is this: does that added evoking force also exist in lithic drawings, at least in the domain that Lopes calls non-conceptual? He seems to suggest that this aspect is, say, the objective counterpart of the interpretative rendering. Do we have to believe, then, that this added evoked contextual interpreting force that we seem to acquire in the "touristy" process I described above, does not work in the lithic drawings case? And if it doesn't, why should this be?

2) The consideration of scientific images as art at some point in History is, mutatis mutandis, the same that has turned, say, some war reports written by general Julius Caesar into examples of Roman literature, or, for that matter, pornographic intimate letters by James Joyce and Anaïs Nin into erotic literature. It has to do with the way the information presented is processed, which, according to me, has little to do with the aesthetic value some drawings may or may not have; moreover, it does not relevantly raise any interesting question on the relationship between art and science. This is surely another topic of debate, but I wanted to stress that it happens all the time with anything whatsoever, not just drawings that show good craftsmanship.

3º) If I understood Lopes accurately, lithic drawings are a mixture of what I term implicit representations, where the relationships of elements in the representation are analogous to the real object they represent (i.e., images, for short) and explicit representations (where these relationships have to be explicitly stated (i.e., propositions, for short) as, say, maps seem to be. Then, to meet John Zeimbekis' problem, why don't we use several images of the same object with different symbols standing for different features? In cartography this seems to be a normal policy.

4º) Stretching this similarity a bit further, for non conversant with Geography, a map is a lot better description of a place than an aerial photograph, which seems to match the advantage of drawings over photographical pictures in lithic illustrations.

The Draughtsman's Contract
John Kulvicki
May 25, 2005 22:43 UT

This is a fascinating case study, and most of the comments below are requests for some more detail.

Lopes misses an interesting epistemic feature of lithic drawings: they can teach us how to see stones. The markings that stand for a particular kind of chip dug out of the axe embody a hard-won archaeological result. How do chips of that kind look generally? Perhaps there is no obvious visible feature that they have in common. Perhaps the features are individually obvious but grouped in a highly obscure disjunction. These are two ways that regularities in nature can be difficult to discern. It would help to know more about these kinds of chips and how much the lithic drawings abstract from their spatial details. In either case, a drawing or photo that includes precise spatial detail cannot merely depict a chip of that kind: it will always tell us more.

But if the lithic drawings abstract from these details, how can they help us better to see such chips? First, couple lithic drawings with their corresponding depicta, or possibly high-quality photos thereof.

Because they are mimetic with respect to the outlines and shapes of stone's subregions, it is easy to pair parts of lithic drawings with the regions of the tools they represent. By guiding inspection of the tools, the pictures aid the development of recognitional abilities for these subtle or obscure perceptual kinds. This is not the way pictures are normally thought to promote recognitional prowess—cf. Schier 1986 and Lopes 1996—but these are not ordinary pictures.

This advantage seems to reveal just where lithic drawings can fall short of one's epistemic needs. Since they embody the results of archaeological research, they would be fairly useless tools for questioning those results. To question how scientists read the stones, one needs access to the details over which the lithic drawings abstract. As long as this is not one's goal, of course, the drawings seem perfectly useful and objective, as long as they are carefully made. In this regard I agree with John Zeimbekis's response, but I am less worried about the limitations.

Neither the advantage I mention nor the disadvantage plays a role in Lopes' paper, and it would help to have a clearer sense of purposes to which these pictures are put. What are the generalizations that receive support or the questions that are raised by looking at these representations? With that in mind it would be easier to get a sense of the significance of these images qua images, qua drawings that abstract from many details, etc. It just seems too general to discuss lithic drawings in terms of objectivity and whether they represent particulars or types.

One final note: the distinction between machine made and person made images seems not terribly significant. There are practical limitations on both sides. If a machine could make the lithic drawings, it would be surprising if one were to insist on the versions fashioned by hand. As long as the makers of these images are reliable, however, it's difficult to see why one's suspicions should be aroused.

The Draughtsman's Abstract
Dominic Lopes
May 27, 2005 23:52 UT

John is quite right to ask for details about how archaeologists use lithic illustrations. I don't yet have an answer. For one thing, some are published but many, many are not, so John's question can't be answered merely by reading the journals. Moreover, they appear to be used in many ways in the journals. In particular, they appear to be used as evidence in many ways. I doubt that there will be a simple story to tell. And if there's no simple story about uses in publication there's not likely to be a simple story about uses in the lab.

I'm not sure if I follow John's main argument about the advantages and disadvantages of abstraction (selection?). Grant that making a drawing that shows a flake as removed by a blow at point x requires abstraction. It seems to me that is consistent with showing enough detail for a scientist to contend, with good evidence, that the flake was removed by a blow at y. We need not think of abstraction as deleting all information that could challenge the interpretation that the abstraction implies.

Finally, John's point about the drawings training the archaeological eye is extremely important. We must not overlook this role of images in science.

A Suggestion on Taxonomy
Susrut Ray
May 27, 2005 6:35 UT

I would like to suggest an alternative taxonomy for graphics that diverges from the 'hand-made'/ 'machine-made' distinction used in Lopes' thought provoking article. The alternative, arguably, renders some of the issues discussed more palpable. The proposal is to have a graded taxonomy ranging from 'photographic' (P) to the 'non-photographic' representation (NP). The paradigm of P is,

predictably, photographs. The works of a skilled draughtsman attempting to emulate a camera is near the P end of the range. NP's vary widely, some examples being machine-made seismographs, man-made histograms or the works of Jackson Pollock. The 'lithic' drawing discussed is a hybrid, part P (fine contours) and part NP (symbols in Figure 2). Representations, other than those produced by camera, are often hybrids. Advantages of the suggested taxonomy stem from the recognition of hybrid representations. Hybrids will somewhat loosely be referred to as NP. P are produced typically without any specific criterion of relevance; NP, on the contrary, are made always with some criteria of relevance in mind (in the mind of the designer of the machine when the NP is machine-made). The relevance criteria drives selectivity of certain features and filtering of others. The selected features are depicted using a conventional symbol system. The cognitive system uses relevance/selectivity in a manner that we have not been able to fully understand. Our attempts to analyse how we select what is relevant and what not, is thwarted, amongst others, by the notorious "frame problem" discussed at length by Fodor, Dennett and others. This is a major limitation of AI. Lithic NP is better that lithic P is because a camera has no built-in archaeological criteria. Can a machine be designed to produce lithic NP of adequate quality? That depends first on our ability to transfer the archaeologist's expert knowledge (e.g. distinguishing flake scars and thermal fractures) to a machine. We do not yet have this ability, but that in itself is not the full story. If we could transfer of the expert knowledge the machine would still be deficient. When, for example, it encounters an archelogical fake (e.g. a plastic piece made to have surface characteristics of an axe), the machine with built-in expert knowledge may produce a lithic NP which does not indicate that the piece is a fake just because material constituent had not been specified as relevance criteria. The frame problem is about how to "frame in" all possible relevance criteria. P are always representations of tokens. NP can be representations of types (e.g. anatomical and botanical drawings) or of tokens (e.g. Galileo's moons). Hybrids like lithic NP are part representations of types (flake scars etc) and part of tokens (shape, exact location of flake scars within the shape etc). Epistemic considerations require that tokens be related to types, that a particular piece of stone be classified as an axe. A hybrid lithic NP does this by superimposing representations of type on representations of the token.

More Taxonomy
Dominic Lopes
May 27, 2005 23:34 UT

There are many ways to cut the pictorial pie, of course. In deciding where to make the cut, we should think about what task our taxonomy is helping us to perform. With that in mind, how useful is the spectrum that Susrut proposes, from purely photographic to purely non-photographic? What orders images on the spectrum is the degree to which relevance criteria drive the selection of some types of features and the filtering of others. Pure photographs are not made to conform to any such criteria. That's a very nice point. However, in scientific contexts, I would venture, images are always made to conform to relevance criteria. Consider lithic photographs. I don't know but I would bet that they are made in a way that conforms to some of the conventions of lithic drawing, especially those that set the type of light, the direction of illumination, the scale, the medium (e.g. film and focal length). I guess that not only because standards are needed for purposes of comparison but that some ways of photographing stones will fail to show what's relevant. So if we're interested in images in science then we're always interested in criteria of relevance.

Why Photograhic/Non-Photographic Taxonomy is Better
Susrut Ray
May 31, 2005 6:52 UT

How useful is the proposal of cutting the pictorial pie along photographic/non-photographic lines? Consider one of the main problems raised by Lopes. How are we to explain that lithic

drawings, even though hand-made, do better than machine-made photographs at data capture? The context is that empirical generalization, putatively the aim of lithic drawings, demands that data be captured reliably and objectively. When reliability is seen in terms of 'contrafactual dependence' and objectivity is taken to be the mark of the non-conceptual, machines are more reliable and objective data capture devices. The explanation offered can be paraphrased as follows. The lithic illustrator's work can be analysed in two parts (a) making 'marks on the paper that capture the finely detailed contours and textures of the stone', and (b) ' "read" a stone's surface and represent …(it) using the conventionalised vocabulary.' Part (a) provides reliability-objectivity whereas part (b) facilitates empirical generalisation. A camera provides part (a) better than humans, but skilled humans can do acceptably. A camera cannot provide part (b), but a human can. A lithic illustrator therefore combines the efficacies of the photographic and the non-photographic, whereas a camera cannot. That is why a lithic drawing does better than the camera. So, the photographic/non-photographic cutting of the pie explains the phenomenon, whereas the cut along 'hand-made/machine-made' distinction does not!

What is the drawing an analog representation of?

John Zeimbekis

May 31, 2005 10:58 UT

There remains for me a grey area at the centre of Dom's paper, and I think several of the contributions (Ray, Casati, Guijarro) implicitly address it.

If drawings are better than photos, it cannot be for fineness of grain (unless we had only very poor photographic devices—though even they may retain large-scale spatial relations better than a drawing would). Therefore drawings would *seem* to be better for representing 'conceptual' values and conveying on/off-type information (more precisely: representing properties that form discrete rather than continuous property-spaces).

But I'm not sure such a claim is explicit in Dom's paper, and perhaps with good reason: after all, the drawing is an analog representation (dense, more fine-grained, composite – but not discrete).

The question is then, if the drawing is an analog representation: *What* is the drawing an analog representation *of*?

It cannot be an analog representation of what the stone looks like, because if it was, then archaeologists should have no grounds to prefer drawings from good photographs. Moreover: what the stone looks like is the result of the actions carried out on the stone (as, for instance, a trace of ink is the result of an action). So it also seems to be excluded that the drawing is an analog representation of the *result* of the actions of knapping, honing etc.

So what is the drawing an analog representation of? Is it possible that it is a representation with general value but determinate spatial content – in other words, a fictional representation with instructive value, of the same order as the drawings of type-members of natural species in dictionaries? Or even the reconstruction drawings discussed by Moser?

Object-related epistemic seeing-drawing for tracking visible properties and tracking down bygone properties

Maria Rossi

Jun 3, 2005 0:36 UT

It seems that we owe an object-based account to Dominic Lopes' epistemology of lithic illustrations. His analysis is particularly helpful for (i) appreciating the powers of object-related (primary and secondary) epistemic seeing [1], understood here within a wider seeing-drawing cycle or system; and

(ii) distinguishing the varieties of epistemic functions of pictures with regard to the human knowledge of material objects/artifacts. For instance, lithic drawings seem to play a functional role for object-related abilities such as ('x' stands for a material object):

- keeping record of the identity of the depicted artefact/object x;

- directing visual analyzers to particular properties of x such as x's unique shape (cue selection, weighting and recognition);

- inferring by induction general properties of the things of x's kind;

- comparing x's selected properties with properties of tokens of the same type;

- inferring on the basis of x's perceptible (particular) properties a set of x's (non-perceptible) bygone properties;

- inferring and describing the particular actions performed on x in the course of x's history;

- inferring general or particular properties of the agents who have interacted with x (e.g. spatiotemporal location, cultural characteristics);

- inferring x's function or status within a social community.

Lithic drawings can thus contribute to the indirect anchoring and orienting of object-related thoughts and explanations. They possess an "epistemic quality" and a "moderate objectivity" for their organization fulfills the rules of a hierarchical "system" in which their non-conceptual parts can be analyzed conceptually. The key phenomenon is thus the interaction of non-conceptual content with conceptual content. However, how does this interaction take place? How do non-conceptual skills/contents relate here to conceptual knowledge? For instance, how can the cues of Fig. 2 be analyzed and retrieved from the drawing and finally matched to judgments and concepts about the drawing and the depicted object? The article does not study this issue in detail. If we were to develop it, a possible path of explanation would be to analyze how appropriate non-conceptual visual skills/routines [2] are recruited for detecting basic relevant cues by what is sometimes called 'proto-propositional' [3] and propositional content so as to develop visual reasoning and gather knowledge by visual examination of the drawing's details. Research on demonstrative selection and identification [4-7] seems to be relevant for this project.

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Est-ce une affaire de progrès technique ou bien de culture ?

Catherine Allamel-Raffin

Jun 12, 2005 21:12 UT

Les réflexions faites par D. Lopes quant à la préférence pour le dessin par rapport à la photographie dans le domaine des illustrations lithiques me font penser aux débats qui ont eu lieu lorsqu'on a commencé à recourir à la photographie dans des domaines scientifiques déterminés. Envisageons les termes du débat à partir de deux catégories : la visibilité et la lisibilité. Comme le montre bien D. Lopes, le dessin présente l'avantage de pouvoir mettre l'accent sur ce que l'on veut montrer via des codages précis propres à une discipline. La lisibilité de l'image est ainsi accrue par le fait que les informations jugées pertinentes sont sélectionnées par le dessinateur. En revanche, et ceci n'est pas souligné par l'auteur, le dessin ne peut être considéré comme une trace ou un indice de l'objet représenté, à la différence de la photographie, qui donne à voir ce qu'est l'objet à un moment donné et garantit qu'il existe bien à ce moment. La photographie permet souvent d'accroître la visibilité (il n'y a qu'à penser aux premières photographies de champs stellaires en astronomie : ce qui est invisible à l'œil nu, ou même en regardant dans un télescope, devient visible sur une plaque photographique dès lors que le temps de pose est suffisamment long). La lisibilité de l'image, quant à elle, n'est pas accrue par le simple recours à la photographie, puisqu'elle est non sélective quant à l'information qu'elle est susceptible de contenir. On peut ainsi affirmer schématiquement que ces deux types de représentations scientifiques – dessins et photographies – présentent des avantages et des inconvénients opposés (quant aux deux catégories évoquées ici) lorsqu'il s'agit de les insérer dans un processus de validation des connaissances. Dès la naissance de la photographie, l'évaluation de ces avantages et de ces inconvénients a été l'occasion de vifs débats au sein de la communauté scientifique. (On peut consulter sur ce point l'ouvrage d'A. Rouillé, La photographie en France, textes et controverses : une anthologie 1816-1871, Macula, 1989). Notons que dès la fin du XIXème siècle, Gaston Bonnier tente un compromis entre dessin et photographie : dans son ouvrage Flore complète en couleur de France, Suisse et Belgique, il recourt, sur un même support, aux deux : le contour des photographies est redessiné à la main afin de mettre en évidence les détails importants. La qualité des figures ainsi obtenues est telle, que l'ouvrage de Bonnier a été constamment réédité et constitue, sous une forme abrégée, le seul document admis lors de certaines épreuves à l'agrégation de biologie en France ! Comme le souligne P. Barboza (Du photographique au numérique, Paris, L'Harmattan, 1996), ce type de conciliation préfigure certains codages des images numériques. Ma question sera simple et va dans le sens des réflexions de J. Zeimbekis : le dessin lithique n'est-il pas une simple étape intermédiaire en attendant des traitements des images préalablement digitalisées, qui permettront d'accroître la lisibilité des photographies des pierres, tout en conservant l'aspect indiciaire de ces dernières ? En attendant, pourquoi ne pas recourir alors à la technique proposée par Bonnier et qui semble avoir été probante en botanique ? A moins que le milieu des archéologues ne soit peu enclin à se servir des possibilités offertes par certaines techniques photographiques ou numériques ? Dans ce cas, la préférence pour les dessins n'est-elle pas explicable avant tout en termes de culture propre à un domaine de recherche scientifique ?

Dual Recognition of Depth and Dependent Seeing

John Dilworth (Western Michigan University)
(Date of publication: 6 June 2005)

Abstract: An explanation of the seeing of depth both in reality and in pictures requires a dual content theory of visual recognition. In addition, there are two necessary conditions on genuine seeing of depth-related content. First, the right kinds of dependence relations must hold between a physical picture, its content and its perceiver, and second, the perceiver must be in an appropriate, functionally defined perceptual state.

Introduction

How is it possible to see depth in a flat picture, or a mass of water such as a lake in its dry surface? The purely physical characteristics of pictures, such as flatness and dryness, are often distinct from the represented properties of depth and watery mass that one can see in them. But how can this seeing of such typical pictorial content properties be genuine seeing, if what one sees is not, strictly speaking, actually being physically presented to oneself?

I won't rehearse standard attempts to handle such problems. Instead, I'll present a brief summary of one novel approach that is argued for in my just-published book *The Double Content of Art* 2005a (DCA), then outline a more comprehensive view that grows out of the DCA approach.

The Double Content of Art presents a sustained series of arguments to the effect that artworks must be identified with the representational content of concrete artifacts such as physical paintings, manuscripts, concrete musical performances and so on. But since clearly we can in general perceive artworks--e.g., see paintings, or hear pieces of music--it follows that we can genuinely perceive any of the kinds of content that are associated with artworks, including their conventional subject matters. A wide range of other arguments for the legitimacy of this straightforwardly perceptual view of our experience of artworks are also provided in the book--see also Dilworth 2005c.

Another central element in the argument of DCA is, as the title suggests, that artworks involve a double or two-level content structure. Artworks must be distinguished from their subject matters, so that the normal case is one in which a concrete artifact such as a physical picture represents the visual artwork itself, which in turn represents its subject matter, with two different kinds of representation being involved. Typically each kind of representation plays a complementary role, with the artwork itself being made up of stylistic, expressive and intentional content, which in turn represents in the conventional sense the subject matter of the picture. A comprehensive theory of these two kinds of representation--aspectual versus intrinsic--and of their relationships is also provided in the book.

I The Double Content of Normal Perception

However, even if all of these claims are accepted for artworks, a more complete understanding of how perception of content is possible would require a broader and more fundamental theory, in which perception of artworks or other representations were closely related to normal perception of non-representational objects. Also, the crucial issue of how exactly perceptual reference to content items is similar to, or different from, standard cases of perception of concrete objects or properties would also need to be fully explained. In addition, since on my account perception of artworks always involves a double content structure, it would be highly desirable to find similar double content structures in normal non-representational perception, as a preliminary to giving a unified account of perception generally.

Fortunately it is possible to show that normal perception must also have a double content structure, as demonstrated in two articles, Dilworth 2005b and 2005d. One basic argument for this view is that retinal or other sensory information is a mixture of information both about the properties of objects, such as their shape or color, and also information about various relevant aspectual conditions, such as ambient lighting, or the angle at which a surface is viewed. Thus perception of the color or shape of an object is achieved only after a decoding operation in which the intermixed object-related versus

aspectual information components are separated out, but both components persist in normal perception, such as when one simultaneously sees both the circular shape of a disc, and also the aspectual elliptical shape it has because of the perspective in which one is viewing it.

In terms of standard perceptual and psychological theory, this double content view is a natural extension to perception generally of theories concerning perceptual constancy phenomena, such as the fact that a surface can continue to look white even under different conditions of ambient illumination. The double content view generalizes the idea of 'looking a certain way' to include both aspectual and object-related appearances, which structured combination it argues to be ubiquitous in normal perception.

II The Close Relations Between Pictorial and Normal Perception

Enough theory is now in place to explain, or construct, close links between normal perception of non-representational worldly items, and the perception of artworks. To begin, a useful way to summarize the double content view of normal perception is that it provides an explanation of the extreme flexibility of perceptual recognition mechanisms, in that even very wide variations in sensory data with respect to a worldly property F can be explained as being caused by independent aspectual factors A, which the decoding procedure can appropriately compensate for. The bridge to artworks is provided by the fact that the concrete representational artifacts, upon which a viewer's gaze is directed when she perceives an artwork, also often have very different properties from the subjects that they represent. On the present view, our abilities to perceive artistic contents are based upon the same general-purpose perceptual mechanisms, which enable us to perceive the properties of ordinary objects even under extremely variable aspectual conditions.

For example, the ability to perceive a simple pencil drawing on white paper as having a person as its subject matter arguably is of a piece with our ability to normally perceive a faint outline on a foggy day, in which virtually everything looks white, as nevertheless being the outline of an actual person. Arguably the same, or at least closely similar, perceptual abilities are mobilized in each case. While at the same time, the very different epistemic conditions under which each perceptual episode occurs--with an actual person causing the foggy perception, versus the pencil drawing on white paper causing the perception of the subject matter of the drawing--can serve to explain why the former case is a case of perception of content, while the latter is a case of perception of an actual person.

At this point a critic might remind us of how much still needs to be accomplished, as follows. So far all that has been presented here is a more psychologically sophisticated recognitional theory of artwork perception than perhaps has previously been available. Broadly recognitional theories, such as those of Schier 1986 or Lopes 1996, claim that we recognize items in pictures in much the same way as we recognize actual items of those kinds. The double content view explains in addition the facility with which we are able to do this, and the continuity of artistic with normal cases, but the above account so far has provided no further epistemic, semantic or ontological insight, no matter how effective it might be as a theory of cognitive perceptual mechanisms. It remains paradoxical how one could genuinely see e.g. a lake in a picture when there is no actual lake that one sees, without illusion or error of some kind. Also, specifically ontological issues as to the existential status of the seen content, plus semantic issues as to how genuine reference is possible in such cases, also need to be addressed (though my book DCA, Dilworth 2005a, does address many of these issues).

III Semantic and Ontological Issues Resolved

It seems to me that the critic is right. A satisfactory resolution of these and related issues requires the construction and defense--at least in outline form--of a comprehensive, broadly naturalistic theory of perception, potentially capable of giving an adequate general account of any kinds of perceptual reference and intentionality in naturalistic terms. Such a theory should, among other things, also be able to explain how there could be cases of veridical perception of content without overstepping naturalistic boundaries. I shall now briefly outline the main concepts of one such theory that has recently been developed.

The basic idea is as follows. What is needed is a functionalist theory of perception that explains perception in terms of the three standard kinds of causal role invoked in any functionalist theory of

mind or cognition, namely the causal roles of sensory inputs, intervening internal cognitive or dispositional states, and behavioral or other action-related outputs. My version of such a functionalist theory could be called the reflexive theory of perception, in that according to it, an organism Z perceives a property F of object X just in case property F causes Z to acquire some F-related disposition, that is, some disposition to act toward object X in some specifically F-related way. For example, on this reflexive theory, a typical case of person Z veridically perceiving an object X to be red would produce a disposition in Z to engage in some red-related behavior toward X, such as by putting X into a bin reserved for red objects. Non-veridical perception of the redness would show itself in potential incorrect color-sorting actions with respect to object X. (For further discussion see Dilworth 2004 and 2005e).

On this reflexive account, perceptual reference is explained in terms of the closed causal loop between property F, that causes the relevant perceptual state S in person Z, and Z's F-caused and F-related dispositional state S, that potentially would result in some action by Z upon that very same property F itself. The more specific concept of perceptual intentionality would allow also for different ways in which person Z might conceive of property F, whether veridically via a red-related disposition toward F, or non-veridically via e.g. a green-related disposition toward that same property F, even though both intentional states would involve perceptual reference, as reflexively defined above, to the same worldly property F.

Now prima facie, such a naturalistic theory has no way in which to accommodate veridical perception of content items as such. In this reflexive theory, any perceptual reference is entirely explained in terms of closed causal loops between actual objects or properties, and dispositions directed toward those same actual items. But when one sees a lake in a picture, since there isn't any physical lake which causes one's perception of lake-related content when looking at the picture, at best a non-veridical or mistaken perception of a lake would apparently result, leading to an illusion or error theory of content perception.

However, fortunately a functionalist perceptual theory has resources not available in more standard perceptual theories, which can be used to circumvent such problems. On standard, purely input-based theories of perception, if a real lake didn't cause a perception of a lake, then the perception must be non-veridical. But a functionalist theory can also consider as relevant factors both intermediary, internal processing factors, plus behavioral output factors as well. With suitable adjustments in those factors, the fact that an actual lake didn't cause the perception can be accommodated as follows.

To begin, consider what is involved in normal, non-representational cases of non-veridical perception of lakes. For example, one might attempt to drink from, or jump in, something X that one perceives as a lake, only to find out from the behavioral results that one was mistaken. Thus non-veridical perception of an item X as being a real lake involves characteristic kinds of disposition-formation plus behavioral outputs.

Nevertheless, it is clear that normal perception of the lake-related content of a picture involves no such characteristic non-veridical dispositions or behavioral outputs--normally no-one is under any illusion that they are looking at a real lake when perceiving a picture of one, aside from trompe l'oeil cases, and so they are not tempted to act toward the picture as if were a real lake. But such characteristic differences in disposition-formation and behavior toward pictures, as opposed to toward objects that are perceived as being real lakes, provide enough relevant functional differences to warrant a claim that veridicality standards for perception of lake content, as opposed to perception of actual lakes, are independent of, and systematically different from, veridicality standards for perception of actual lakes. As another example, we take it for granted that a picture of a lake can be handed from one person to another, without our assuming that the lake in the picture has thereby shifted its position. But only a radically disoriented person would attempt to hand a real lake to another person, again showing that the relevant veridicality criteria for real versus represented lakes are systematically different in each case.

The remaining issues to be dealt with concern how it is possible to perceptually refer to content items in such cases, and the ontological status of the content item referred to. According to the purely naturalistic reflexive theory of perception being used, all genuine perceptual reference must involve causation by physical objects and properties F of dispositions that are themselves directed toward

those very same physical items. So somehow reference to content items must be explained in terms of reference to such actual physical items, if it is to be genuine, on the present account.

At this point, the previous distinction between perceptual reference and perceptual intentionality becomes salient again. In the case of normal perception of a physical property F of a non-representational object, both veridical and non-veridical perception of F involve reference to the same property F, but the intentionality can be different in veridical versus non-veridical cases. Hence there is some sense in which each perceptual content or 'intentional object'--such as that involved in seeing the redness of an object as red in veridical cases, or as green in non-veridical cases--is different in each case, even though both contents involve reference to the very same physical property. Or in broadly Fregean terms, the functional differences between veridical versus non-veridical perceptions of a given property F serve to define different 'senses' or modes of reference for each perceptual content, even though each of them has the same actual reference.

Now this point can be extended to cover the more radically different content cases, in which the basic criteria for veridical versus non-veridical perception have systematically shifted from those appropriate for real cases. Presumably pictures of lakes are such that some group F of their physical properties cause normal perceivers to see lake-related content, which seeing consists in the acquiring of dispositions directed toward those very same physical properties F of the picture. However, as just discussed, the perceptual content or 'intentional object' could be different, depending on whether the viewer veridically perceives the lake content or instead perceives it non-veridically as a picture of a bathtub, even though the reference would be the same in either case, namely those physical properties F of the picture that cause the recognition of the lake content.

A more straightforward functional characterization of the situation, free of traditional intentionality or Fregean concepts, could be provided as follows. On the present naturalistic reflexive theory, the concept of reference involves a fundamental kind of referential spread or bluntness, in that reference is explained in terms of overt behavioral outputs directed toward an object or property. But any physical realization of a relevant functional perceptual state inevitably involves a wide range of interactions of a person's actions with physical objects and worldly states, so that it is never the case that a perceiver interacts with just one worldly object or property. Thus conceptualizations of objects and properties are inevitably more precise than references to them, on the present account. But an implication of this fact is that it is semantically harmless to refer to objects or properties that strictly speaking don't exist as such--as ontologically independent content objects or properties--as long as any such references arise only as part of legitimate functional perceptual states in which concrete physical objects or properties F are causing the relevant states, and which states are themselves referentially related to those same concrete objects or properties F. Or otherwise put, perceptual reference to content items is legitimate as a kind of reference that is dependent on reference to the relevant physical items that normally cause such content-related perceptual states to occur.

As might be expected from this account, the most appropriate ontological view of content items on the reflexive view would be an irrealist one: strictly speaking there are no ontologically freestanding or independent content items, but perception of them, as explained above, can play a legitimate functional role in perceptual activities that exploit the double-content-based recognitional flexibilities of perceptual systems. Or as a related way of explaining content items, appropriate groups of physical properties of pictures give rise to emergent dispositional properties to cause appropriate content-related perceptual states in culturally trained perceivers, and the seeing of content items involves appropriate functional interactions, of the kinds previously described, by perceivers with those emergent physical properties.

To conclude, here is a series of near-platitudes showing why some theory roughly similar to the present one is likely to be correct. First, one certainly can genuinely see a lake when looking at a picture of a lake, because one can immediately recognize lakes in pictures just as easily as real lakes. Second, a theory that doesn't closely relate representational seeing to normal seeing of worldly items has no hope of explaining the basic recognitional psychological data--even pigeons can recognize objects in pictures, and hence see content items. Third, our extremely flexible recognition mechanisms require postulation of two different kinds or levels of content in order to explain their powers. Fourth, no illusion could be involved in the seeing of content by normal viewers, because none of the normal behavioral signs of confusion or misidentification are present in such cases. And fifth, presumably all

of the above points must be consistent with some rigorous naturalistic theory of perceptual intentionality, which explains content perception in terms of references to actual objects and properties, because virtually no-one these days seriously doubts that some such theory must both be true, and available to us if we look hard enough for it. The present reflexive view may not be correct in detail, but arguably something similar to its general outlines is theoretically unavoidable.

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Discussion

depth and reference
John Zeimbekis
Jun 7, 2005 15:47 UT

Something that puzzles me in John's paper -- as a treatment of contents in depth perception -- is the fact that he analyzes reference as being to properties, as opposed to places.

Thus, John writes: "...the functional differences between veridical versus non-veridical perceptions of a given property F serve to define different 'senses' or modes of reference for each perceptual content, even though each of them has the same actual reference".

Couldn't we argue that the part of a mental content that represents a property is descriptive, and not referential? Then we could say that it is reference that makes picture-perception non-veridical. For example, phenomenally, I see a boat a mile away when I look into a postcard, but in fact the boat is somewhere else.

> **Reply to John Z. on depth and reference**
> John Dilworth
> Jun 9, 2005 11:50 UT
>
> John Z's initial query is as follows: "Something that puzzles me in John's paper -- as a treatment of contents in depth perception -- is the fact that he analyzes reference as being to properties, as opposed to places."
>
> Initially, no more is involved in this than the commonsense distinction between a seen object or place--such as a lake in a picture--and the property that one sees it to have, namely that of being a (particular) lake. I'm just trying to adapt the same realistic, referential way we have of talking about seeing an actual lake and its properties to the seeing of a lake in a picture. To be sure, eliminativists such as Quine or Goodman would deny that we can genuinely see any

property of being a lake when looking at a picture of a lake, but I'm attempting to preserve and defend this intuitively natural view within an alternative naturalistic framework.

But the main thrust of John Z's query is in his question, "Couldn't we argue that the part of a mental content that represents a property is descriptive, and not referential?" On the functionalist theory of perception I attempt to give at the end of the paper, the relevant mental content is both descriptive and referential. Aside from the functionalist theory as such, this is actually a fairly common view, namely that normally one cannot refer to something without also describing it, or taking up some attitude or stance toward it, or interpreting it, and so on.

Just one further point. John Z adds "Then we could say that it is reference that makes picture-perception non-veridical." But I'm attempting to show how some picture perception could be veridical--e.g. that one could correctly see a lake, rather than a bathtub, in a particular picture--so I'd reject any attempt to make picture perception merely parasitic on the seeing of real cases of the properties in question.

That theory you asked for
Thomas Stoffregen
Jun 12, 2005 5:18 UT

Dilworth notes that "What is needed is a functionalist theory of perception that explains perception in terms of the three standard kinds of causal role invoked in any functionalist theory of mind or cognition, namely the causal roles of sensory inputs, intervening internal cognitive or dispositional states, and behavioral or other action-related outputs."

Such a theory exists, and has existed for some time. I refer to the Ecological Approach to Perception and Action, originated by James Gibson (e.g., 1979). Gibson developed a general theory of perception that does all of the things requested by Dilworth, and has done them with enough success that the theory is now a major player in scientific psychology and in various applications, such as interface design. Gibson dealt directly with issues of pictoral representation, and engaged in a published interaction with Ernst Gombrich on this subject. More recent scholars have also found the Ecological Approach to be helpful in understanding the perception of art. See, for example, Joseph D. Anderson's 1996 book, The Reality of Illusion, which presents a theory of film perception inspired by the Ecological Approach, or my own discussion of relations between perceptual theory and the perception of paintings, film, and depictions, in general (Stoffregen, 1997, in Ecological Psychology).

It would be helpful if Dilworth could relate his approach to the Ecological Approach, identifying areas of agreement and conflict.

Reply to Tom Stoffregen on Gibson
John Dilworth
Jun 14, 2005 3:11 UT

To begin with positive factors and similarities, I find Gibson's concept of an affordance to be extremely interesting one, that is well worth developing. According to my alternative reflexive theory of perception, all perception involves an organism acquiring X-related dispositions toward its perceptual cause X. But no restriction is placed on what those dispositions might be, nor to which properties of item X. Now in evolutionary terms, one would expect that maximum survival value would accrue to species whose perceptual activities become concentrated on those aspects of the world that would be most useful or valuable to the animal. Thus the reflexive view and Gibson's view could agree that much perception is focused on perceiver-related, instrumentally useful aspects of worldly objects and events, rather than its having a purely epistemic or scientific purpose of discovering and representing facts about the world.

Thus both views could agree that perceiver-relative meanings can be directly perceived in the world, and so to that extent both provide direct theories of perception. I might add that on the reflexive theory, the relevant acquisitions of perceptual dispositions could involve direct neuronal modifications in the brain, so that no extra cognitive representation of perceived items is required, which also is closely consonant with Gibson's view. In addition, we both view perception as being directly connected with action--as on Alva Noë's enactive view--in my case because, on the reflexive theory, to perceive X just is to acquire X-caused dispositions toward X, which typically often would be immediately manifested in X-related actions.

However, a significant difference emerges over the issue of Gibson's claim that perceivers can directly pick up invariant information from the environment. To be sure, the double content view of art and perception in my book The Double Content of Art, and in the Synthese and Phil Psychol. references in my article, arguably, like Gibson, starts with an ecological view of perception as being environmentally situated. But the lesson I draw from the contextual or aspectual nature of all perception is completely opposed to that drawn by Gibson. I argue that there cannot be any simple pickup of invariant information from the environment, because the inextricable mixture of aspectual versus object-related data provided by sensory input requires sophisticated decoding to extract two distinct levels of perceptual content. Thus, on my account there is an even more indirect and complex process of inferential decoding required to extract usable content from raw sensory data than is acknowledged either by Gibson, or by informational semanticists such as Fodor and Dretske. Thus, Gibson and I are poles apart on the issue of direct informational pickup.

Nevertheless, Gibson's general idea of an ecological approach to perception, as a systems-based approach to the perception and control of action as in your own work, seems to me to be a very valuable one. But I would argue that perception as such is only one factor in the control of action, and that both higher forms of cognition and planning, plus conscious experiential factors, are needed in addition to provide an adequate account. Thus I think it is probably a mistake to try to explain all intelligent action in purely perceptual terms.

ART? ... perception
Jose Luis Guijarro
Jun 14, 2005 16:25 UT

Dilworth’s paper offers some very useful ideas about perception and its relation to cognition. For instance, the idea that “conceptualizations of objects and properties are inevitably more precise than references to them” (and what follows till the end of the paragraph) is, for me, a novel and interesting consideration which, as it is described explicitly, clarifies certain intuitions that I think I had in a very vague way.

What I don’t understand is why Dilworth seems to be so eager to relate this dual recognition theory of his to (the concept? The word?) ART. Granted, one has to be able to perceive and to conceive in certain ways (in “human” ways, I would say) to be able to experience ART. To argue otherwise would be absurd. But the same absurdity would arise if we were to deny that to experience ART one has to be a living (human) being. However, trying to describe what life is in order to explain ART seems a puzzling endeavour.

Two questions related to this:

FIRST. If looking at a cloudy sky I happen to “see” twisted camels, bearded Santa Claus heads, lone trees and whatnot in the strange shapes of some cumulus or cirrus, am I perceiving in a typical dilworthian dual way, or is this kind of perception an “error”, although I know all the time that I am looking at clouds? I take it, from what I have been reading, that seeing the portrait of Aunt Martha in that cumulus over there is an intentional affair and thus, it would fall within the scope of this dual recognition theory. Am I mistaken?

SECOND. If I am looking at an abstract painting (say, Rothko or Pollock), does this dual perception story also operate? If so, does this imply that I see individual painted shapes as colour spots, lines or volumes AND also a composition in which they all interact with one another to build up a painting? If so, I don't see any difference in that dual perception and the one we have when we look at the real world where, not only forms and their ground interact, but also the forms and, arguably, the backgrounds among themselves all the time?

Re. Guijarro's ART? ... perception
John Dilworth
Jun 15, 2005 3:10 UT

Re. Guijarro's ART? ... perception

"What I don't understand is why Dilworth seems to be so eager to relate this dual recognition theory of his to (the concept? The word?) ART."

Answer: the dual recognition theory grew out of trying to understand both what artworks are, and how artistic style and expression are possible. In general artworks can't be concrete objects, because novels, movies and plays etc can have multiple copies. Yet 'type' theories of art are hopelessly platonistic, making artistic creativity impossible. If instead artworks could be represented by screen showings, music performances, concrete paintings etc, this would mean that the way in which they in turn represent their subject matters can explain artistic style and expression. This view inevitably involves two levels of representational content, or a dual recognition.

"FIRST. If looking at a cloudy sky I happen to "see" twisted camels, bearded Santa Claus heads, lone trees and whatnot in the strange shapes of some cumulus or cirrus..."

"SECOND. If I am looking at an abstract painting (say, Rothko or Pollock), does this dual perception story also operate?"

Answer: see secs I and II of my paper--I came to realize that normal perception must have a similar double content structure to perception of artworks, if only because of the ambiguities in incoming raw sensory data. Seeing a cloud as a twisted camel must involve perceptual decisions about how to interpret the relevant raw data, just as much as in the perception of a deliberate artwork.

"If so, does this imply that I see individual painted shapes as colour spots, lines or volumes AND also a composition in which they all interact with one another to build up a painting? If so, I don't see any difference in that dual perception and the one we have when we look at the real world where, not only forms and their ground interact, but also the forms and, arguably, the backgrounds among themselves all the time?"

Answer: the main difference is that looking at the real world involves interactions between aspectual versus object-related data, but that one doesn't represent the other, as in the case of artworks representing their subject matter. See my forthcoming Synthese paper "The Double Content of Perception" for more details on this difference. And thanks for these interesting queries.

Further questions
Jose Luis Guijarro
Jun 15, 2005 11:50 UT

Your prompt reply to questions should be the "normal" way to act in seminars like this one; but unluckily, it is not the usual practice, since the majority of main paper writers seem to be too

high up in one scientific world or another to condescend with poor mortals down here. Therefore, I have to thank you heartedly for trying to make some issues clearer to me.

However (there is always a “however”, I’m afraid!), I still need some help.

Did you attain your initial aim (i.e., “(…) understand both what artworks are, and how artistic style and expression are possible”)? To tell you the truth, even if I am more or less able to imagine how your theory could help you in the second attempt, I can’t begin to see how it would help you to understand what artworks are. You seem to be content with your “two levels of representational content, or a dual recognition”. But then, in answering my first question you repeat what you wrote in your paper, namely, “ (…) normal perception must have a similar double content structure”. I take it that, if double perception is a “general” phenomenon it can’t be of any help in explaining a “special” case of perception. Couldn’t ART perception (or rather, ART conceptualization) be a triple sort of recognition? What I mean is that, if human beings are able to process information in nested representations, why should ART be limited to one embedding instance only -as you suggest? The experience of ART seems to involve a superordinate representational process that somehow “frames” the object perceived, and forces us to treat the (probably, double) information we are able to extract from it in normal perceiving processes as being “displayed” in order to be evaluated in a certain way (which should be described by the relevant ART theory).

If I am right, then, this superordinate representation (i.e, the DISPLAYING one, if you see what I mean) would be the one responsible for treating the double information you suggest in one way (i.e., non artistic experiences) or another (i.e., ART perception). It would somehow trigger our disposition to watch for the results of both sorts of perceptions and use them in our appreciation.

Re: Guijarro's Further Questions
John Dilworth
Jun 29, 2005 12:50 UT

Here's how I attempt to answer your question in another paper--hope this helps:

It might be thought that on the double representation account of perception invoked here, perception of depictions would involve not two but four levels of perceptual representation, so that, after all, picture perception would have a significantly more complex structure than ordinary perception. For if perception of physical objects and their properties requires two kinds or levels of representation--aspect versus object--then it might seem as if those first two levels of representation would only succeed in identifying the specifically physical properties of the pictorial artifact itself.

On such a view, a further, specifically pictorial interpretation of that physical artifact would be required, which would itself distinguish additional aspectual versus object-related representational factors associated with that depictive artifact, in arriving at a perceptual recognition of its subject matter. Also, such a four-level view might seem to be strengthened by whatever persuasiveness a double representational analysis of the representational powers of pictorial artifacts themselves may have.

But what immediately collapses the postulated four levels into just the usual two generic perceptual levels or kinds is the evidence that pictorial recognition is just as immediate in the case of pictured Xs as it is in the case of real Xs. Indeed, there is some evidence that kinds of items and their properties are even more readily recognizable from pictures than from real examples. Thus there is no need to invoke any more than the usual two levels of representation in understanding such cases.

An epistemic and evolutionary argument to the same conclusion, that only two levels of representation are needed in understanding typical picture perception, could proceed as follows. In gazing around in her environment, a perceiver has no means of knowing in advance

that a particular object X upon which she turns her gaze is in fact a picture. Thus she could not know ahead of time that a sophisticated procedure involving four levels of representation, rather than the usual two levels, would be required in order to adequately perceive or decode this particular object's recognizable content, as opposed to that of more normal objects.

Thus, since the whole point of creating pictures was presumably initially--in the evolutionary timeframe of our cultural history over the last few thousands of years--to give viewers convenient access to their contents, those pictures which could be immediately recognized in the normal way via their contents must have culturally won out in their competition with more complex kinds that required instead a fourfold, non-standard kind of perceptual interpretation--if any such ever existed. Hence, it may be concluded, even the most sophisticated current artistic cases must be understood in the context of our default perceptual systems, for which the easiest and most convenient pictorial recognition cases must have set the norm or standard of perceptual interpretation, both in evolutionary terms and in terms of individual development within our culture.

Shall we follow dualistic or pluralistic divides of perceptual content and aesthetic experience?

Maria Rossi

Jun 15, 2005 16:39 UT

I have two questions about your summary of The Double Content of Art (DCA henceforth), which bear on the choice of dualism instead of pluralism (partially addressed in your reply to J.L. Guijarro).

First question, according to DCA “artworks must be identified with the representational content of concrete artifacts” and “the normal case is one in which [i] a concrete artifact such as a physical picture represents [ii] the visual artwork itself, which in turn represents [iii] its subject matter, with two different kinds of representation being involved” (the numbering is mine). This analysis seems to uphold that artworks are essentially – and not accidentally – representational (two-level) apparatuses. If this claim is supposed to define the essence of any artwork, the claim faces the problem of accounting for literalist and minimalist artworks discussed by C. Greenberg [1] and M. Fried [2], in which a presented physical individual (object or sound event = [i]) does not apparently depict anything like [ii] or [iii] although it entails miscellaneous cognitive effects. (Cf. readymades, minimal art introduced by Judd, Morris, Andre, Smith, Greenberg’s collection , land art, literalism in electro-acoustic music etc.) What is DCA’s account of this type of cases? Is literalism impossible or illusory?

Second question, as it as been shown, namely in the discussion of D. Lopes’ article on lithic illustrations, many types of pictures are used in many different (epistemic and aesthetic) ways in art and science. I think that we have to account for this variety at the level of directing philosophical principles: thus how can DCA account for this plurality if it is so strongly rooted in representational dualism (instead of pluralism)? In other words, is there not a risk that DCA overlooks a number of other epistemic and aesthetic functions of pictures? The same question could also be raised about DCA’s claim about ordinary object perception, for the distinction about object-dependent and perspectival/aspectual contents of perception may not account for the diversity of perceptual contents. For instance, what is the status of perceptual verifications of observational propositions?

1. Greenberg, C., Art and Culture. 1961, Boston: Beacon Press. 2. Fried, M., Art and Objecthood: Essays and Reviews. 1998, Chicago: The University of Chicago Press.

Reply

John Dilworth

Jun 29, 2005 14:10 UT

I'm unclear about your dualism versus pluralism distinction. I take it that the standard view about representation is a monistic one--there's only one kind of depiction, and it has only one level.

Also it's usually assumed that concrete objects such as paintings are themselves artworks, which I deny (as does Danto, by the way: he describes them as non-artistic 'mere real things'). So literalists would simply deny that any representation is involved in the concrete objects that they take to be artworks.

I don't have a well-developed view of such literalist cases, but since, on my view, normal perception of real objects also involves a double content structure, just as with the perception of artworks, I could simply say that literalist cases are those that exploit our perception of ordinary objects, rather than using specialized representational objects. This certainly seems appropriate with land art in particular. Thus the essence of art remains in specialized exploitations of the double content structure of perception, but, again, these could use normal perception of normal objects rather than objects constructed to be seen representationally.

As for your second question, certainly pictures can be used many different ways. This is no problem for standard monistic views of representation, and I don't see why it should be any more of a problem for my dualistic view--which in any case has many more theoretical resources for dealing with problematic cases than standard monistic views.

As for broader issues about the diversity of perceptual contents, eg what is the status of perceptual verifications of observational propositions, this gets into much broader perceptual and philos. of sciences issues, that in my view require a more comprehensive functionalist theory of perception (see my original paper for a summary), with double content issues being only a minor factor. Eg, I argue in another paper (ref below) that scientists can actually see the craters on a moon of Jupiter in a photograph of the craters, because as trained scientists they are able to acquire perceptual dispositions toward those craters themselves, whereas the average non-scientist only sees some photographic content in the photograph, because of their lack of training.

John Dilworth, "Perceptual Causality Problems Reflexively Resolved," Acta Analytica, in press.

Perspectival and factual vs Aspectual and intrinsic

Nivedita Gangopadhyay

Jun 17, 2005 16:44 UT

According to Dilworth "… normal perception must also have a double content structure" viz. aspectual and intrinsic. In this context we can draw an analogy with the thesis propounded by Noë who claims that perceptual content has two aspects viz. factual and perspectival (Noë, 2003). Thus Noë states "First, we experience not only how things are, but also how they look from here. We experience that the plate is round and that it looks elliptical from here." (Noë, 2003). Dilworth's theory of aspectual and intrinsic aspects similarly seems to highlight the fact that in perception the content is determined both by the object and by the perceiver's relation to the object. If this be the case then why not follow the terms "perspectival" and "factual" aspects instead of introducing the new terms "aspectual" and "intrinsic"?

Reference:

Noë, A. Causation and perception: the puzzle unravelled. *Analysis* 63.2, April 2003.

Re: Perspectival and factual vs Aspectual and intrinsic

John Dilworth

Jun 29, 2005 13:22 UT

One reason is that by the time Noë's short Analysis paper appeared, I already had a complete book manuscript providing a much more comprehensive account of a much broader distinction. For instance, the aspectual versus intrinsic distinction covers any kinds of perceptual

processing, including kinds not specifically depending on viewer position, such as the fact that yellow retinal light may be information about an intrinsically yellow surface, or about a white surface illuminated by aspectual yellow light, and so on. As a further point, the term 'factual' is potentially misleading in the philosophy of perception generally, in that eg. Dretske distinguishes between simple non-epistemic perception versus epistemically loaded 'factual' perception, and I wanted to avoid that dispute.

Aspect: narrow or large?
Anouk Barberousse
Jun 22, 2005 10:45 UT

According to John Dilworth, our understanding of pictures rests on two different kinds of representation, which he calls 'intrinsic' and 'aspectual'. Moreover, he insists on the extreme variability of the aspectual conditions. But how much exactly do these conditions encompass? Are certain elements of context part of 'aspect'? For instance, the meaning of a line drawing may heavily depend on its title, which may determine how it looks like for the viewer -- namely, it may determine its aspect. Or the interpretation of a computer made astronomical picture may depend on the viewer's knowledge about the picture making technique. So, how large should 'aspect' be conceived?

Re: Aspect: narrow or large?
John Dilworth
Jun 29, 2005 13:06 UT

My basic idea, as I would now express it, is that all perception involves initial processing in which aspectual versus object-related components of content are distinguished, and that this double content processing carries over also to the perception of artworks. Thus the meaning of a line drawing is both about aspectual content, such as the artist's intentions or expressive style, and about its conventional representational content or subject matter as well. As for your knowledge about picture making technique, that is part of what you use to correctly interpret the concrete artifact in that double-content way which, normally, is an interpretation also intended by the artist.

Scientific Reasoning, Mental Models, and Depiction

Laura Perini (Assistant Professor at Virginia Tech.)
(Date of publication: 20 June 2005)

Abstract: Figures seem to play an important role in scientific reasoning, but the nature of reasoning with images is not yet understood. In this paper I will draw on the literature on mental models to provide some insight into the reasoning that scientists apply when working with visual representations.

1. Introduction

Scientists use images frequently. Visual representations like graphs, diagrams, MRIs and electron micrographs appear in research reports, grant proposals, textbooks, lab meetings and lectures. The way scientific images are presented indicates that they are not merely objects of perception, but are also involved in scientific reasoning. Figures in published research reports, for example, contribute to the argument presented in the paper: sometimes by expressing its conclusion, and sometimes by providing evidential support. How are we to understand reasoning that involves figures?

One way to address the issue is to investigate the use of images in light of current ideas about cognition. The depictive character of mental models provides a perspective on human cognition from which to clarify why visual representations are so useful to scientists. This tactic provides a perspective on the images that goes beyond individual cases, but is not constrained to explain the reasoning involved solely in terms of properties of the images. I will give a brief explanation of mental models and then examine several different kinds of cases, in order to show how images are involved in scientific reasoning.

2. Mental Models

One general hypothesis about cognition is that mental representations may have a depictive character rather than the features of a linguistic representation. External linguistic representations are characterized by a serial format: the sequence of visible marks or sounds is sufficient to determine a meaningful sentence. Spatial relations among the marks, such as distance from one word to another, do not normally contribute to linguistic meaning. In contrast, a depictive external representation is one whose two-dimensional form is essential to its meaning: some spatial relations of the symbol refer to features of the referent. In scientific visual representations, the spatial relations of the figure might refer to non-spatial or even non-visible properties. For example, length in timelines represents duration, and graphs are used to represent relations among properties. For all visual representations, however, the relationship between symbol form and content correlates visible properties of representations with features of the states of affairs they represent.

Recent work in cognitive science suggests that mental representations have similar features. A mental model is a depictive mental representation, in the sense that it has the same structure of relations as their contents. This means that dimensionality of content will be reflected in the dimensional structure of the mental representation. This is similar to how external visual representations use spatial dimensions to represent spatial and temporal relations, and other dimensional properties like temperature. If internal representations are structured like their referents, then manipulation of structural elements of the internal representation that are shared with the referent will serve as a form of reasoning, because the forms of both the input and the output representations are correlated with the structures of their referents.

Thinking about mental representations as mental models contrasts with the view of mental representations as analogous to linguistic representations. If mental representations have a serial format rather than a dimensional, depictive format, then the structural form of mental representations would stand in an arbitrary relation to the content represented, just as the visible form of the marks comprising a printed sentence bears an arbitrary relation to the content expressed by the sentence. In this case mental processes would run like operations on serial symbols. Rules of inference could be

applied based on the form of a mental representation in a manner analogous to applying rules of inference based on the form of logical formulae, but not by altering or changing perspective on the form, as would be the case with mental models.

While the question about the nature of mental representation is not yet settled, there is a growing body of evidence in support of mental models. Problem-solving through mental imagery suggests that some cognitive processes are best described as manipulations of mental models rather than operations on serially structured mental content. For example, when we use visual imagery to think through a problem like counting the sides of a pyramid, we use a mental representation that shares some of the structural features of the object we are thinking about. Experimental results in which the amount of time subjects take to solve problems correlates positively with some dimension relevant to the problem (like an estimation of distance or shift in perspective) provide further support. Cooper and Shepard (1973), for example, test time required to rotate letters and other patterns.

3. Images & construction of mental models

The concept of mental models is very general. Can it provide any insight into scientists' use of figures? Understanding mental representations as mental models provides some criteria for what must be accomplished in comprehending an external representation. No matter how the external representation is formatted, its content must be given a depictive structure for the internal representation. This suggests that images provide an advantage, because the structural relation between external representation and referent may facilitate formation of an internal representation which also has a structural relation to the referent. The information figures convey is already formatted so that spatial dimensions map onto relevant parameters of the state of affairs represented. Any symbol must be perceived in order to be comprehended, but because visible features of the symbol are mapped onto features of the referent, visual representations are comprehended through a perception of the visible features of the symbol which have a built-in structural relation to the symbol's content. The cognitive convenience that visual representations offer would then provide an explanation of why figures are used when alternative forms of representation are available. The presentation of content in two-dimensional format provides a way to decrease the total effort to needed to form an internal representation with a spatial character.

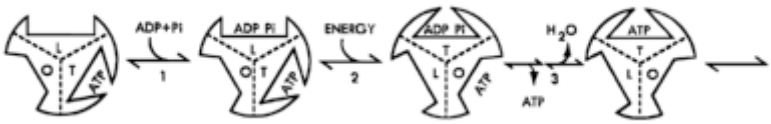

Figure 1: Diagram of the binding change mechanism of the ATP synthase. Jan Pieter Abrahams, A.G. Leslie, R. Lutter, and J. Walker (1994), "Structure at 2.8 A resolution of F1-ATPase from bovine heart mitochondria." *Nature*, 370 6491:621-8. reprinted with permission from Nature; www.nature.com.

Consider an image that conveys a scientific model. Figure 1 is a diagram of the mechanism by which an enzyme complex called the ATP synthase catalyzes the formation of ATP—the compound called the "energy currency of the cell" because it provides the immediate energy for most cellular functions. Most ATP is formed in mitochondria as the end result of a multi-step biochemical process called oxidative phosphorylation. In the last step of oxidative phosphorylation, the ATP synthase catalyzes the reaction of ADP and Pi to form ATP. Figure 1 presents a model of how the ATP synthase works, called the binding change model. According to this model, the ATP synthase has three subunits that can catalyze the formation of ATP. The catalytic subunits are chemically identical but take on three different conformations, each of which has different binding affinities for the inputs and products of the reaction (ADP and ATP, respectively). The three catalytic subunits of ATP synthase are each in a different one of the three conformations, and they all rotate through the same sequence of these conformations: first loose, which binds ADP, then tight, in which ADP and Pi bind to form ATP, and finally open, from which ATP is released.

The binding change model can be explained verbally, but it is more efficiently communicated with Figure 1. The wedge shapes refer to catalytic subunits of the ATP synthase enzyme complex (in open, loose and tight, conformations respectively). The different wedge shapes only refer to the different binding capacities of the different states; the binding change model does not include claims about the specific structure of the different subunit conformations. Contiguity of the wedge characters refers to co-location of subunits in the enzyme complex; spatial relations are used to convey this general feature of the complex, not the more specific structural claim that the catalytic subunits are contiguous. The horizontal double arrows refer to transitions between different states of the enzyme, in which the same subunit changes to a different conformation—for example, from loose to tight—the diagram doesn't represent the complex as rotating. The linguistic terms in the figure have their usual chemical referents; their position at the ends of hooks represents the addition of those items from the complex during transition from one state to another, and their position at the ends of curved arrows represents the deletion of those items. Positioning terms in the concave part of the subunit symbols means that the item is bound to a subunit in that conformation.

The diagram provides a concise way to express the model, and even more significantly, the diagram uses visible features like contiguity relations to express the key explanatory features of the model. The relations between states of the catalytic subunits, and the coordinated transitions of these states over time, are effectively expressed by spatial relations among parts of the diagram. This formatting aids the production of mental models: comprehension of the binding change model requires generation of a mental model structured in terms of the same key relations which the diagram conveys through spatial relations. For this reason, there is a cognitive advantage to using diagrams rather than verbal expressions of hypotheses. Thus diagrams that express models or hypotheses might provide a very effective way to communicate because they aid in generating the mental model at which reasoning is directed.

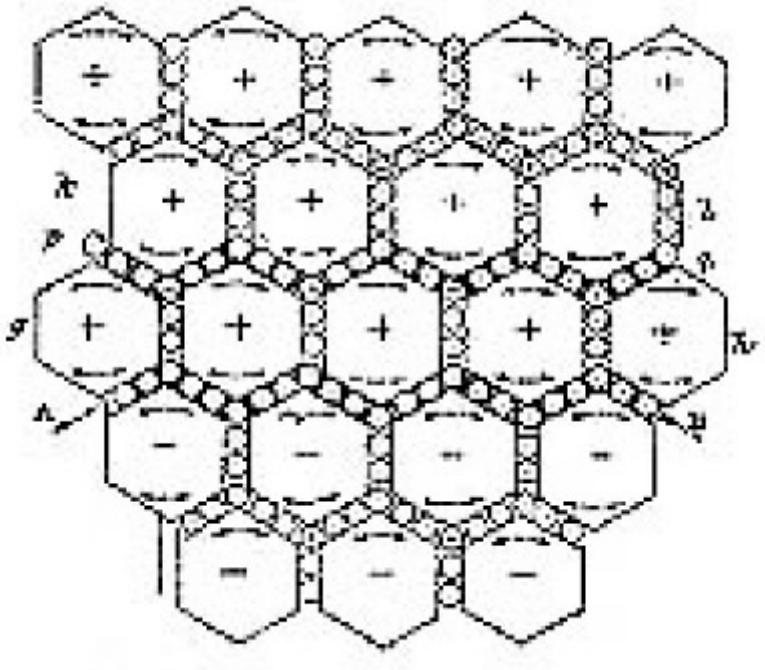

Figure 2: Clerk Maxwell's "physical analogy", Maxwell 1890.

Visual representations may do more than provide an efficient way to communicate due to facilitating the construction of a mental model. Nersessian (1992) explains Clerk Maxwell's use of a figure to present an analogy between electromagnetic forces and vortex-idle wheel systems. The analogy involves a mental mapping between key relations from the source domain—a mechanical system—to a target domain—electromagnetism. Perception of the figure involves comprehension of a structure of relations that is shared between the two domains, and by the visible features of the figure. So

perception of the figure involves comprehension of this structure. The visual representation thus provides an effective way of communicating the analogy. Nersessian's discussion suggests that images can do even more. The physical analogy is constrained by the mechanics of the situation: if vortices spinning in the same direction were arrayed in direct contact with each other, they would impede each others' rotation. This is a feature of the situation that is only implicit in a verbal description of an array of vortices spinning in the same direction. This constraint is met by the inclusion of idle-wheels between the vortices, as shown by Maxwell's figure (figure 2). Presenting the analogy with a visual representation means using the spatial features of the image to represent features of the source domain: in this case, the rotation of adjacent vortices. This makes explicit higher-order relations like the effect of one spinning element on an adjacent element; you would see, if the array consisted only of vortices spinning in the same direction, that they would impede each other (see Nersessian 1992 figure 3A for her rendition of such an image). The use of a visual representation to convey the analogy, by making the effects of adjacent rotating elements explicit, ensures that the full vortex-idle wheel system serves as the source domain for the analogy, rather than just an array of vortices.

The examples above show how images aid the generation of mental models, and suggest that they can play significant roles in the elaboration of mental models. The visual representations discussed used spatial relations to express conceptual relations important to the model. In these cases, the advantage the images provide is that they make these relations explicit by communicating them through spatial relations. Verbal descriptions of the models can convey the same information, but higher-order relations are left implicit and thus impose a higher cognitive load to comprehend, and increase the risk that critical higher-order relations will be overlooked.

4. Reasoning with mental models and graphs

Graphs also explicitly convey higher order relations: they are designed to represent relations among properties. Reasoning with graphs can include drawing inferences about an even higher-order relation, such as a comparison between two different rates of change of one property with another. Gattis and Holyoak (1996) present a study of the how graphs facilitate such inferences which shows that simply using spatial dimensions to represent properties is not sufficient for effective reasoning with graphs. Gattis and Holyoak(1996) investigate how perceptual operations on graphs are used to draw inferences about the relations the graphs depict. They note various studies in which visual representations not only decrease problem-solving time, but also increase accuracy, which suggests to them that "diagrams highlight the information used in reasoning with internal mental models and provide external support in managing the relations in working memory" (Gattis and Holyoak, 232.) The depictive character of images explains this highlighting: the information is presented via a two-dimensional structure. It is the structure that is used in reasoning with internal mental models. The depiction makes this structure explicit and perceptually available. Graphs offer an additional advantage: they integrate information from two conceptual dimensions. This is a demanding cognitive task, which graphs make easier by representing relations between properties through spatial relations; the conceptual dimensions are integrated through visual perception. Because of the mapping between spatial properties of the image and conceptual properties, the relation between variables can be understood with far less cognitive load.

Gattis and Holyoak show that the cognitive value of graphs requires more than the general feature of using spatial relations to represent relations among properties. The specific way relations between variables are plotted has a significant difference on accuracy of reasoning. It is standard graphing practice to use the horizontal axis of a graph to represent the independent or causal variable, and the vertical axis to represent the dependent variable. This convention—which Gattis and Holyoak call the "slope-mapping constraint"—is a graphing technique which produces a relationship between the slope of a graphed line and the rate of change: a steeper line corresponds to a faster rate of change in dependent variable with change in the independent variable. They assess the cognitive value of the slope-mapping constraint by testing for accuracy of reasoning with graphs formatted with, and without, following this constraint. The authors asked about the rate of change of one variable (the dependent variable) with respect to another (the independent variable), in cases in which the dependent variable is on either the vertical or horizontal axis. Their results show that reasoning is significantly better when the dependent variable is plotted on the vertical axis. This is a format that honors the slope-mapping constraint: faster rates of change in the queried variable with respect to the other property correspond to steeper lines.

Furthermore, they tested graphs in which the variable plotted on the vertical axis preserved the similarity relations commonly exploited in pictures, for example by plotting altitude on the vertical axis in a graph of altitude and temperature. Subjects were shown graphs with a solid line representing a relation between altitude and temperature, and a dotted line (see figure 3.) They were queried about relative rate of change of temperature with altitude represented by the two lines. Graphing altitude on the vertical axis will produce a correlation between slope and the rate of change of temperature with altitude in which a faster rate of change corresponds to a *fllatter* line (the opposite effect of the slope-mapping constraint, which produces steeper lines for greater rates of change.) Subjects reasoned more accurately with graphs in which the slope-mapping constraint was honored (temperature was plotted on the vertical axis), than they did with graphs using the pictorial analogy (in which altitude was plotted on the vertical axis.)

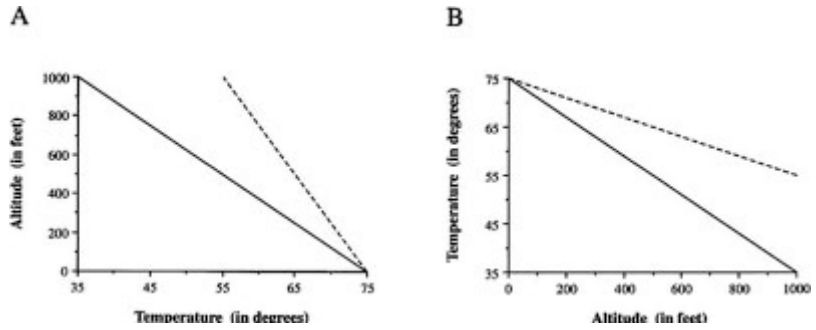

Figure 3: The slope-mapping constraint

(From Gattis, M., & Holyoak, K. J. (1996). Mapping conceptual to spatial relations in visual reasoning, Journal of Experimental Psychology: Learning, Memory, and Cognition, 22, 231-239. Copyright © 1996 by the American Psychological Association. Reproduced with permission.)

B honors the slope-mapping constraint by plotting temperature, dependent variable, on the vertical axis. As a result, as the rate of change of temperature with change in altitude increases, steepness increases. So the solid line, which is steeper, represents a faster rate of change of temperature with respect to altitude than the relatively flat dotted line. A is a graph that does not follow the slope-mapping constraint: altitude, the dependent variable, is plotted on the horizontal axis. When plotted this way, the dotted line, which represents a slower rate of change in temperature with respect to altitude, is steeper.

These results show that accuracy in reasoning from graphs requires more than just plotting the data so that properties are represented by spatial dimensions. That alone produces a relation between properties that is expressed by spatial relations. This study shows that accuracy improves when the graph is structured so that higher order relations have a certain visible form so that steepness of line reflects rate of change. This suggests that subjects were utilizing an abstract mental model of the higher-order relations, which is structured so that a faster rate of change is modeled as steeper. Formatting graphs to conform to this aspect of the mental model promotes accuracy.

5. Evidence, images, and mental models

Visual representations are frequently presented in support of hypotheses. It is striking that in many cases, the forms of the external representations offered as evidence do not have any obvious similarity to the form of the models they support. Confirmation in such cases cannot be explained by a simple matching of the visible structure of the image to that of a mental model which involves mental imagery that corresponds to the perceived image.

Abrahams et al (1994) present a model of the structure of the ATP synthase complex which supports the binding change model of the synthase mechanism. The diagrams of the models for the ATP synthase structure (for example, figure 4) and the binding change model (figure 1) look completely

different. The reasoning involved in taking images like figure 4 as support for the model expressed by figure 1 must be a more complicated matter than generating perceptual expectations, which can then be simply compared on the basis of similarity with the structure of the evidential figure. Figure 4 is a representation of the structure of the C-terminal ends of the proteins making up the subunits of the ATP synthase complex. The overall structure of the complex is sort of like a tangerine, with a central core (?, in blue) and six protein "wedges" arranged around this core. The three catalytic ? subunits, in gold, are distributed evenly around the core. The binding change model says that the three catalytic subunits are always in three different structural conformations. This implies that the complex has an asymmetric spatial structure. The authors cite the visible asymmetry of figure 4 as support for the binding change model.

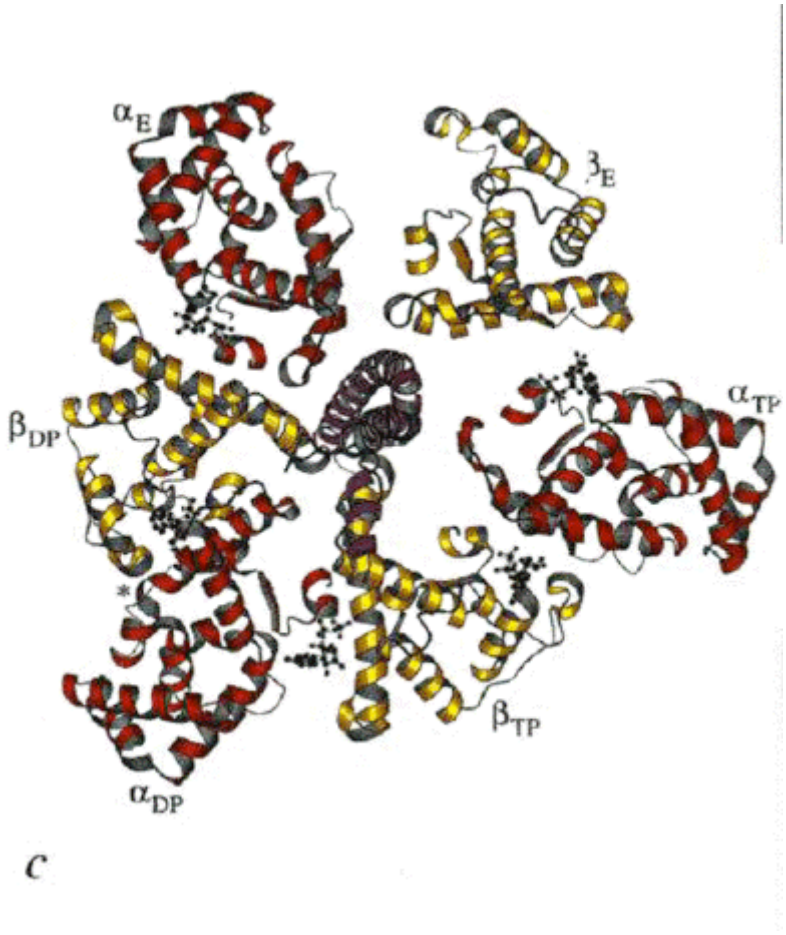

Figure 4: Ribbon diagram of C-terminal protein chains of the ATP synthase complex. Jan Pieter Abrahams, A.G. Leslie, R. Lutter, and J. Walker (1994), "Structure at 2.8 A resolution of F1-ATPase from bovine heart mitochondria". *Nature*, 370 6491:621-8. reprinted with permission from *Nature*; www.nature.com.

Figure 4 is an example of an image that serves as evidence not because of its particular form, but because it has a more general property. In this image, ribbons represent protein chains, and the spatial features of the image are used to represent spatial features among the protein chains of the complex. The diagram of the binding change model, on the other hand, uses spatial relations to represent co-location and transitions over time: these two diagrams do not relate to each other on one coherent set of dimensions. Figure 4 serves as evidence for the binding change model, because that model doesn't imply a particular structure, but rather a more general feature. The binding change model abstracts from the structure of the ATP synthase, and from many other details of its mechanism, but it does imply that the complex has a general structure feature: according to the binding change model, the complex is asymmetric. Because the visible features of the model of the complex' structure are formatted so that their spatial features map onto the spatial features of the

complex, the image can be used to evaluate binding change model, by looking for asymmetry in the visual representation. Thus the viability of the model can be evaluated perceptually, to see if the visual representation has that property.

The binding change model is a proposal about the number and functional relations between catalytic subunits of ATP synthase; it is not a complete account of the mechanism by which the complex produces ATP. In particular, researchers suspected that the ATP synthase has a rotary mechanism. This is a very unusual feature in a biological molecule, and though the structure of the ATP synthase is compatible with a rotational mechanism, scientists sought stronger evidence. This enzyme complex is far too small to visualize its movement with a microscope. This problem was solved by attaching a fluorescent filament to the central ? subunit of the complex, which had the effect of amplifying both the size and the signal from the moving part of the complex, and filming the movement of the filament with a microscope. That film shows the filament rotating (available for viewing on the Yoshida and Hisabori lab web site, at the following link: http://www.res.titech.ac.jp/~seibutu/projects/fig/f1rot_1.mov.) Noji et al (1997) publish their findings from the video—which the authors refer to as "direct evidence" for the rotational mechanism—in a paper which presents a series of stills from this film, in which the position of the filament is at different angles in subsequent images.

Both the visual and temporal format of the video are important to how it functions as evidence; it uses the dimensions of space and time to support the rotational mechanism of the complex. These are the dimensions relevant to the claim about the rotational mechanism of the ATP synthase complex, which implies a circular pattern over time. The video of the rotating filament does not, however, look anything like the complex itself would, were it visible (with its core subunit rotating with respect to the outer subunits.) This is another example of an image which serves as evidence for a model not by a simple matching procedure, or due to the specific form of the visual representation. Rather, the feature of the image that provides support is a property abstracted from the particular form of the image (in this case, rotary motion of a line.)

This case also highlights a general feature of images that serve as evidence: they do so in the context of other information than is not presented in the image itself. In this case, information about the experimental situation is critical to the evidential role of the video: in particular, the causal link between the activity of the enzyme, which isn't visible in the video, and the movement of the filament, which is visible. So the function of this moving image as evidence depends on more than what is visible in the image, or even its content. This is a general feature of visual representations presented as evidence: their warrant as evidence derives not just from their content, but from background knowledge that supports their reliability and relevance to the hypothesis in question.

6. Conclusion

These examples show several ways in which images might be involved in scientific reasoning, if that reasoning takes the form of constructing, changing, drawing inferences from or supporting mental models. The results show that it is not merely matching the structure of the mental model that is important to the use of scientific images. Structure-matching could decrease the work needed to generate a mental model, but the value of explicit presentation of higher-order relations goes beyond cognitive efficiency. In addition, other cognitive functions, like evidential reasoning with images, are often not a matter of first-order matching between image and model structure. Rather, scientists abstract features from the evidential images which provide the relevant support.

The importance of more abstract features of scientific images is also shown by the result's Gattis and Holyoak present from their study of reasoning with graphs. This shows that humans reason in terms of abstract mental models which can have structures which impose constraints on accurate reasoning with images. Specifically, they showed that drawing inferences about higher-order relations (in this case, making a comparison between different rates of change represented by a graph) depended on a specific visual formatting which conforms to the structure of the mental model.

The tactic of studying scientific reasoning with images by thinking about how they would relate to cognition involving mental models has led to a result that might be surprising, at least in light of everyday assumptions about pictures and their informational density. From the perspective of mental

models, it seems that scientific images are valuable not because—relative to text—they present rich perceptual content, or convey more detailed information, but rather due to their more abstract features.

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Discussion

What is the content of a (scientific) picture?
Anouk Barberousse
Jun 22, 2005 12:04 UT

At the end of her paper, Laura says that 'the function of [the] moving image [she refers to] as evidence depends on more than what is visible in the image, or even its content. This is a general feature of visual representations presented as evidence: their warrant as evidence derives not just from their content, but from background knowledge that supports their reliability and relevance to the hypothesis in question'. Now what is the content of the video Laura refers to? It displays the movement of the fluorescent filament attached to the central subunit of the enzyme complex which we can show on figure 4 in Laura's paper; but does this phrase express its content? For anyone acquainted with the biochemical problem in question, and with the visualization techniques employed, the video shows that the ATP synthase has a rotary mechanism. More generally, I suggest that the content of a scientific picture, and perhaps of any picture, cannot be determined outside its context of use. What would be the purpose of looking for a independant characterization of the content of a scientific picture?

Models and the structure problem
Dominic Lopes
Jun 30, 2005 0:50 UT

There something I'd like to highlight in this superb paper by Laura Perini - something I think anyone who works on scientific images can and should learn from it. Laura's working premise is that we can measure the contribution of these images to reasoning in science by looking at the workings of their cognitive analogues (mental models or images). Problem-solving with mental models is far better studied than problem solving with artifactual images. Yet philosophers (me included) whose primary concern has been to work out perceptual accounts of pictorial representation haven't fully exploited this research, for doing so seems to reverse the order of explanation: Laura's claim is that pictures help form useful mental models. Of course, what we may draw upon for an explanation of the epistemic role of images is not what's needed for an explanation of pictorial representation.

There's a down side to this, of course. Laura writes that the structural relation between an external representation and its referent facilitates the formation of a mental model that preserves the structural relation to the referent. One wants to know what the 'structural relation' is - apart from a picturing relation. The trouble is that this is a vexed question in work on mental imagery - though less so in work on pictures. So we gain and we lose. I wonder if Laura has any thoughts about this (though I realize it goes a bit beyond the scope of her paper)?

reply
Laura Perini
Jul 5, 2005 19:59 UT

Great question—unfortunately I don't have anything specific to say about the nature of the structural relation. I do think we need something more general than a picturing relation, since the concept of picturing is so closely associated with visual experience. The essential feature is some kind of dimensional structure to the mental representation: content is organized along the relevant dimensions or relations key to the model. This is a minimal conception that doesn't have implications for whether the model is conscious, spatial, etc.

pictures and text
Shimon Edelman
Jun 30, 2005 1:37 UT

Perini concludes her paper with this statement: "From the perspective of mental models, it seems that scientific images are valuable not because—relative to text—they present rich perceptual content, or convey more detailed information, but rather due to their more abstract features." I would like to draw the readers' attention to evidence that suggests that some of the key processes involved in the comprehension of text may be the very same ones that support regular, less indirect, perception. There are tons of recent papers on this topic; a good introduction can be found, for example, in this paper by Zwaan et al: "Language comprehension as guided experience" (2000; http://cogprints.org/949/). Texts may not *present* rich visual content, but they certainly can be very effective by *evoking* it. Moreover, there is no need to wax philosophical about the meaning of "evocation": in this context, it can be given a pretty concrete neurobiological interpretation (which need not be spelled out here).

Linguistic versus depictive representation
Nadine De Courtenay
Jul 8, 2005 15:56 UT

Can one really say that linguistic representation have an « arbitrary relation to the content represented » ? In a scientific language at least, the symbols obey to operations displaying the same structure as the (concrete) operations that can be performed on the entities represented. Moreover, the spatial disposition of the symbols (on the page) is essential to the correct performance of these operations. One could argue that the structure shared by a depictive representation and its referent are also grounded on operations, although not of the same kind : congruence, homothecy, symetry etc. (Could that go in the direction of Domic Lopes' question ?) The advantage of the depictive format would then have more to do with the kind of operations required to solve a specific type of problem and the form of representation that makes these operations more salient and easy for us to process (a point already made in another context by Laura Perini herself and / or Dominic Lopes, if I am not mistaken ?) Of course, the situation is quite different if we have the natural capacity to recognize at once the similarity of form (that is without involving any kind of spatial mental operations). But again, this would only make a difference on first order relations, not on the higher order relations…

Any Way You Slice It: The Viewpoint Independence of Pictorial Content

John Kulvicki (Dartmouth College)

(Date of publication: 27 June 2005).

Abstract: We understand pictures irrespective of the point from which we view them, even though pictures depict things from a point of view. Philosophers and psychologists tend to claim that we somehow compensate for odd viewing angles when viewing a picture. I show that once we understand the complexities of pictorial content, compensation becomes unnecessary.

1. Viewpoint-independence and compensation

Richard Wollheim (1980) coined the term 'seeing-in' for the special perceptual state associated with seeing pictures. Though few agree with Wollheim's account of depiction, everyone seems to agree that 'seeing-in' is a perfect name for the perceptual state evoked by pictures. When we look at pictures we can see what they are about *in* them. Seeing-in is a special perceptual state because it is twofold. We see the picture surface, and are aware of it as a colored two-dimensional plane, but we are simultaneously aware of the picture's content. (Wollheim 1980, 1987, 1998)

Many agree that pictorial experience is sometimes twofold in this manner, but Wollheim was controversial because he insisted that pictorial experience is essentially twofold. One motivation for this view is that it provides a ready explanation of an interesting phenomenon. Wollheim (1980) points out that even though what we see *in* a picture—from a postman to a piazza—is always depicted from a point of view, we are able to see it in the picture almost independently of the viewpoint we take toward the picture itself. Being aware of the picture surface itself allows us to notice that the picture is oblique to our line of sight. We then compensate for this off center viewing angle, which allows us to see the picture's content in it, regardless of the poor angle from which we have of the picture.

It is not obvious why twofold experience is needed to compensate for oblique viewpoints, however. Dominic Lopes (1996) responds that "the required information may be processed along with a wealth of other visual information at a 'sub-personal' level." (1996, 48-49) The person-level experience of seeing a picture need not be twofold if sub-personal compensation mechanisms correct for oblique viewing angles. Psychologists like Maurice Pirenne (1970) and Michael Kubovy (1986) side with Lopes. "When the shape and the position of the picture surface can be seen, an unconscious psychological process of compensation takes place, which restores the correct view when the picture is viewed from the wrong position." (Pirenne 1970, 99) Kubovy, who also cites the Pirenne passage, says "Indeed, the robustness of perspective suggests that the visual system infers the correct location of the center of projection." (1986, 86) Information about orientation need not be part of the experience of seeing the picture, since it could be used by our perceptual systems without our awareness. What these two approaches have in common is that they insist on some kind of compensation that corrects for odd viewing angles. They differ only concerning the nature of compensation required. A third view, which I defend here, is that no compensation is necessary. Pictorial content is the kind of thing we can access without correcting for oblique views of the picture. This proposal requires recognizing a complexity to pictorial content that many have failed to notice. The next section adumbrates this complexity. Section three points out a special feature of pictures that section four uses to argue against compensation, and section five considers some empirical results that bear on the issue. The weight of evidence is against compensation, though the data are far from conclusive at this point.

2. Bare bones content

It is a commonplace in discussions of pictorial representation that many different scenes can result in the same picture. Ernst Gombrich's (1961, 249) discussion of the Ames chair demonstrations is perhaps the most famous example of this. Both a chair and a disjoint collection of line segments can result in the same picture. Those pictures in one's family album certainly represent relatives on different occasions and at different locations, but there is a sense in which they could just as well represent cleverly designed mannequins on cleverly designed sets. These alternative possibilities—

chairs and jumbles, relatives and mannequins—have something in common, of course. Pictures in linear perspective, for example, pick out some information about color and illumination and spatial features that are invariant under projective transformations. This is no more and no less than what all of the different interpretations of a given linear perspective picture have in common. Thought of in this way, there is a sense in which pictures represent quite indeterminate states of affairs. If one takes this to its extreme, as John Haugeland does, "...all the photos 'strictly' represent is certain variations of incident light with respect to direction...." (1991, 189) Haugeland calls this minimal kind of content the "bare bones" content of a picture.

Bare bones content is a far cry from the "fleshed out" content that we usually ascribe to pictures: relatives, chairs, and so on. In fact, we rarely if ever ascribe bare bones content to pictures. For Haugeland, "The point is…to distinguish, within the undeniable contents of everyday representations, a substructure that is skeletal…" (1991, 189) Bare bones content and fleshed out content are not just two contents that pictures have. The former, though usually unnoticed, constrains the latter: all fleshed out contents must be consistent with a picture's bare bones content. And though one can flesh out a picture's content in a myriad of fashions, each picture has only one bare bones content. This skeletal substructure can help resolve the debate over compensation.

3. A special feature of pictures

Once we take on board the distinction between bare bones and fleshed out content, we are in a position to notice an important feature of pictorial representations. In the late 70s, Sherry Levine produced a controversial series of photographs. She made photos of some Walker Evans photographs that were basically indistinguishable from Evans' originals. This was controversial because she displayed her photos as her own work. In a sense, they are her own work: she made photos of Evans' photographs while Evans made photos of life in the rural United States. In another sense, their similarity to Evans' originals makes them seem like mere copies. Controversy aside, this sheds light on an interesting feature of pictures. Many different scenes, like a chair and a jumble of line segments, can result in the same kind of photograph or linear perspective picture from any given point of view. Levine's photos show that one potential subject for any given picture is a plane that has shapes and colors indistinguishable from the picture itself. So, in a sense, the rural US, a well-designed Hollywood set, or simply a photo like Evans' could result in a photo like Levine's. Elsewhere (2003, forthcoming) I relate this to what I call the transparency of pictures. (I thank Aaron Meskin for pointing out Levine's work to me.)

The bare bones content of a picture tells us that certain colors and rather abstract spatial properties, like being a conic section, being straight, and so on, are instantiated at certain relative locations. Bare-bones content captures what all of the diverse possible sources of a picture have in common and thus that upon which any precisification of the content to something more fleshed out must build. Pictures instantiate the very properties specified by their bare bones contents. Linear perspective, for example, is a way of making pictures based on projections that preserve some feature of what they are created from. The so-called perspective invariants are those abstract spatial properties that the picture itself shares with whatever results in the projection. The Evans photo and a scene from rural life share these invariants as well as some properties like relative brightness. By contrast, if there is some kind of bare bones content to descriptions, there is no reason to think that the descriptions themselves instantiate it. Descriptions generally do not describe themselves, so acquaintance with a description is not generally acquaintance with an instance of its content. This is part of why we seem to have such immediate and intimate access to picture's contents, as opposed to those of descriptions.

4. Viewpoint-independence without compensation

With this special feature of pictures on the table, we are in a position to address viewpoint independence. First, most instances of pictures' bare bones contents are three-dimensional, like scenes from one's family history. One thing about three-dimensional scenes, however, is that they are not instances of the bare bones content of a picture *simpliciter*. They are only instances of that content from a point of view, which is why we take fleshed out contents to be viewpoint-relative. Three-dimensional scenes generally only satisfy the bare bones of a picture from a certain point in space. Calling this a point of *view* may suggest that there is something particularly subjective about it, but that is misleading. The point of view of a picture, given a fleshed out content, is just that point in space

from which the three dimensional scene in question satisfies the picture's bare bones content. We can often put ourselves, subjects that we are, at those points, but that does not make the point itself subjective.

Now, pictures are very special instances of their own bare bones contents because they satisfy their own bare bones contents *irrespective* of viewpoint. The picture is a two-dimensional surface, so every portion of it is visible from any point in space not in the picture plane itself. Whether some object or state of affairs is an instance of a picture's bare bones content from a given point depends on whether it exhibits the perspective invariants constitutive of that content from that point. A picture exhibits its perspective invariants from any and all points that are not in the picture plane. That is, no matter what perspective one takes on the picture, it has the same perspective invariants. So pictures are instances of their bare bones content from any point of view. Margaret Hagen (1986, 104) points out that "the invariants captured in such pictures [in linear perspective] are exactly those that remain invariant across *subjective movements of the observer*..." (italics Hagen's). Again, there is nothing particularly subjective about the points in question, but the picture has its perspective invariants from any of the points from which a subject can view the picture. This is not to say merely that pictures *have* their bare bones contents irrespective of viewpoint: they do, of course, but so does any other kind of representation. Rather, unlike most instances of the bare bones content of a picture, a picture is an instance that is not relative to some other point in space, such as the points of view characteristic of the other fleshed out instances of a picture's content.

What does this interesting fact about pictures and their bare bones content have to do with the viewpoint independence of *fleshed out* content? Since the picture manifests all of its perspective invariants independently of the view we take of the picture, oblique views are just as good as direct views for getting at those invariants. So the viewpoint does not matter as far as bare bones content is concerned. In contrast to Pirenne, Kubovy, Wollheim, and Lopes, we do not need to *compensate* for oblique views if pictures make their bare bones contents available to us independently of viewpoint. Now, recall that fleshed out content is consistent with bare bones content, so the way we flesh out a picture's content depends on its bare bones content. If we have viewpoint-independent access to bare bones content, it is reasonable to make two predictions about picture's fleshed out contents. First, we flesh out pictures' contents consistently across viewpoints since they afford the same bare bones content (and do not require compensation to do so). Second, if we fail to flesh out a picture's content consistently between two viewpoints, then the distinct fleshed out contents should both be consistent with the picture's bare bones content. In this case, one needs an explanation for why the fleshed out content changes, but that is independent of the compensation hypothesis. Both of these questions are empirical and the next section focuses on the first, since it relates directly to compensation. The data suggest that my proposal about there being no need for compensation has decent but inconclusive empirical support.

5. Evidence for and against compensation

Some data seem strongly in favor of compensation. Pirenne (1970) was interested in why certain pictures seem to retain their content through massive changes in viewpoint while others, in particular ceiling frescoes, often look lopsided unless viewed from just the right point. In a letter to Pirenne, Albert Einstein sketched a proposal that Pirenne developed. The claim is familiar by now: when looking at a picture, we have perceptual access to the orientation of the picture plane. This allows us to compensate for the oblique view and thus read the picture correctly. Ceiling frescoes do not give us access to the orientation of their surfaces, compromising compensation and resulting in large distortions. Moreover, conflicting cues for orientation affect picture interpretation. Pirenne illustrates this point with a picture of Nixon standing before one of his campaign posters. The poster is at a rather oblique vertical and horizontal angle to the picture plane. While Nixon looks just fine in the photo, the poster of Nixon looks distorted, and the distortion endures arbitrary changes of viewpoint. Pirenne's view is that we compensate for the orientation of the picture of Nixon, which forces us to see the campaign poster behind im as askew, regardless of the point from which we view the picture of Nixon. We cannot compensate for the odd view of the Nixon poster by reorienting the picture, so it always looks distorted. By contrast, if we were viewing the Nixon poster itself from an odd angle, it would not look distorted because we could mobilize or compensation resources.

Three predictions follow from the compensation proposal. First in the absence of information about the orientation of the picture, our interpretations should be distorted—as with the ceiling—when the picture is viewed obliquely. Second, if information about orientation is available, then interpretations should not distort even given changes in viewpoint. And third, in the presence of conflicting information—as with the Nixon poster—interpretations should be distorted.

Busey, et al. (1990) tested something like the first proposal on photographs of faces. Subjects viewed the photos at varying orientations, though the cues that would indicate the orientation of the picture plane were removed. Even under these conditions, subjects experienced no so-called "distortions" for pictures rotated vertically by up to 22 degrees. Perceived distortions appeared for horizontal rotations smaller than 22 degrees. Given the lack of access to the picture's orientation, the undistorted interpretations cannot be due to compensation via adjusting for the angle of the perceived picture. In general, these data are inconclusive because they involve faces. For a long time psychologists have known that people are specially equipped to perceive faces accurately and rapidly under all sorts of conditions, so this may account for at least part of the lack of perceived distortion. Both of Pirenne's examples involve faces, but they also involve fairly large angles—closer to 45 degrees than to 22—with fairly large horizontal components. Cutting (1987) noticed that though moviegoers noticed no distortion at 22.5 degrees, distortions were evident at 45. (I owe the preceding reference to Sheena Rogers' (1995) excellent review of literature on picture perception.) Cutting (1988) comes out in favor of compensation for still pictures, following up on results by Goldstein (1979, 1987). In cinema, which was the focus of Cutting (1987), the picture is viewed from a relatively great distance, and information about its orientation is difficult to discern. It would be interesting to know to what extent the distortion of the Nixon poster is reduced when the photograph is viewed under Busey's conditions, removing information about the picture's orientation. To my knowledge, this has never been investigated.

Halloran (1989) tested the second hypothesis, to the effect that in the presence of information about surface orientation, there should be no distortions. He found that this is true for small angles, but false for large ones. His study follows up on one by Rosinski et al. (1980) that argued in favor of compensation. Rosinski found no distortions when surface information was available, but Halloran showed that increasing the angles at which the picture was viewed, among other things, produced distortions. Halloran's data fit well those of Cutting and Busey, who suggest that there is no compensation mechanism at all. There are no distortions for small angles even without knowing the picture plane's orientation, but distortions appear for large angles. It is likely, then, that we just do not notice any distortion for these small rotations and that large angles are sufficient to alter our interpretations. Rogers (1995, 151) suggests that the data, though in conflict, are overall against compensation. Topper (2000) suggests that compensation is "an experimentally confirmed fact" (119). He leans almost exclusively on Rosinski's work, however. Though he mentions Halloran, Rogers, Cutting, Busey in the footnotes, he does not point out that these papers call compensation into question.

Some of Jan Koenderink's, et al. (2004) results test the third prediction, that interpretations should be distorted in the presence of conflicting information. They found little evidence for compensation even when the conflicting information involves 45 degree vertical obliques. In this study, subjects observed a computer monitor on which was depicted a picture in an obvious frame on a brick wall. Subjects viewed the monitor head-on and at a 45 degree angle and were always aware of the monitor's orientation. The image on the monitor was presented either head-on, in a plane parallel to the monitor's surface, or at a 45 degree vertical oblique. In the latter case, the picture on the monitor is a picture of the wall, picture frame, and picture as seen from a 45 degree angle. (There were other conditions as well, but they are less relevant to the case at hand.) There was no significant difference in subjects' interpretations of the oblique presentation while viewing the monitor head-on and the head-on presentation viewing the monitor obliquely. In both cases, the figure was seen as somewhat slenderer than the head-on presentation viewed with the monitor head-on, and the oblique presentation viewed obliquely was, unsurprisingly, the most compressed of all. Though slightly compressed, it did not seem distorted, which suggests that conflicting orientation information does not thwart our interpretations of the picture, contrary to Pirenne's hypothesis.

The fact remains that Pirenne's (1970) Nixon example seems skewed irrespective of the view one takes of it. Koenderink's experiment does not make use of pictures viewed at horizontal obliques or angles with both vertical and horizontal components. The experiment is therefore far from conclusive. The foregoing has also ignored a wealth of data about how interpretations change as one's distance

from the picture plane changes. Those data conflict much as those for oblique angles do. Yang and Kubovy (1999) retreat from Kubovy's (1986) strongest claims favoring compensation because of data concerning interpretations at different distances from the picture plane. Also, I am unaware of any studies of how interpretations change with distance when pictures are viewed obliquely. There is clearly much more work to be done figuring out how we perceive pictures. For now, the important point is that though there is some support for the compensation hypothesis, it is far from obvious that we compensate. In fact, data strongly suggest we do not, and this fits with the current proposal.

On my account, we flesh out pictures' contents consistently across relatively small oblique angles because we have viewpoint-independent access to their bare bones contents. No compensation is necessary. This is a way of unpacking Cutting's (1987) and Busey's, et al. (1990) suggestion that at these small angles there is no need for compensation. When the angles are large enough to affect the interpretations of pictures, these alternative fleshed out contents seem to be consistent with the picture's bare bones content anyway. At these angles, depicted objects seem to be compressed, or thinned out. Rogers (1995, 138) points out an apt claim from Gombrich's *Art and Illusion*: "if trees appear taller and narrower, and even persons somewhat slimmer, well there are such trees and such persons" (1961, 144). When seen obliquely, the picture occupies a smaller horizontal visual angle. This compression results in a slimmer fleshed out content. A question that has rarely been asked is whether the distinct interpretations of a picture when viewed from varying viewpoints are consistent with its bare bones content. Those who have written on this topic in the past seem to assume that the fleshed out content we assign to a picture when we view it head-on in ideal conditions is the only fleshed out content of the picture, the others being "distortions" of it. This is the wrong way to think of pictorial content and it obscures what the viewpoint-independent access to pictorial content consists in. We have viewpoint-independent access to pictures' bare bones contents, and this results in consistent fleshing out of those contents across many viewpoints. For now, the main goals of the paper have been met: offer an alternative to the compensation hypothesis and show that is has some empirical support.

To close, here is one case that might make trouble for my account and the compensation hypothesis. We interpret anamorphic pictures quite differently depending on the point form which we view them. In the end, I think that the distinct interpretations of anamorphic pictures are consistent with their bare bones contents, but I cannot argue for that here and it is far from obvious. In any case, anamorphosis is not a friend of the compensation hypothesis. Compensation would suggest that the unintelligibility of anamorphic pictures when seen head on should carry over to when they are seen obliquely, since when seen obliquely we compensate for the odd viewing angle. Alternatively, when we seen an anamorphic picture head on, we should compensate so that we get the content from it that we get when we view it obliquely. Neither option seems to be what happens.

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Discussion

Two Challenges to Kulvicki
John Dilworth
Jun 29, 2005 4:27 UT

Kulvicki's paper is a fascinating and provocative one that deserves to be radically challenged. I'll confine my criticisms to two main points.

1. Both Wollheim and Kulvicki assume that a picture may be identified with a physical canvas. I deny this, claiming that the picture is only represented by the canvas. (Dilworth 2005). In part this leads Kulvicki to follow Haugeland in claiming that a picture has some bare bones content that constrains its fleshed out content. But I deny that there is any such thing as bare bones content; instead what he describes as such is actually certain physical configurations on the physical surface of the concrete painting that represents the picture. To be sure, these do constrain fleshed out content, but they are not themselves content items.

As a simple example showing both points, consider the Ames chair Gombrich discusses (mentioned in K sec 2:

"It is a commonplace in discussions of pictorial representation that many different scenes can result in the same picture. Ernst Gombrich's (1961, 249) discussion of the Ames chair demonstrations is perhaps the most famous example of this. Both a chair and a disjoint collection of line segments can result in the same picture."

But strictly they result only in the same physical configuration on the surface of the physical canvas. That physical configuration can represent a picture of a chair, or a picture of some line segments, but it is not itself a common part of the content of any pictures represented by that configuration.

2. The whole discussion of compensation versus non-compensation views of seeing of pictures is--both in Kulvicki and the literature generally, I would claim--currently in a state of comprehensive confusion. There is currently no clear distinction between perceptual constancy effects--such as the fact that a sheet of white paper still looks white, even in yellow light--and compensation effects, in

which the perceiver can figure out what the properties of the item are, in spite of its aspectual setting which can require some cognitive effort to overcome. (Even in distorted cases, one must be able to figure out what the relevant X is, in order to be able to see the picture as a distorted X). Contrast this case with anamorphic cases seen head on (see Kulvicki, last para of last sec), where the relevant content is indeed genuinely unrecognizable, and compensation has genuinely failed.

My main point here is that overall, the contrast between cases where no compensation is required and cases where it is are mere matters of habit and familiarity. In my view all perception must involve aspectual compensation (see my Synthese paper eg). It can be unconscious, as with the perceptual constancy of paper looking white in yellow light, or more noticable as with distorted but still recognizable shapes that require more cognitive effort, but no more fundamental distinctions are involved.

John Dilworth, The Double Content of Art (New York: Prometheus Books, 2005).

--------, "The Double Content of Perception," Synthese, in press.

Response to John Dilworth
John Kulvicki
Jul 5, 2005 21:21 UT

John Dilworth's views about pictorial representation are at odds with mine, but I will have to defer a full response until after I have read through his book carefully and I apologize for not having done so yet. He claims that the canvas is not a picture but that it represents a picture. I claim that one of the many possible fleshed out contents of a picture (which I take to be the canvas) is a picture just like it. This is what I mean when I say a picture satisfies its own bare bones content.

Since I am unfamiliar with the details of Dilworth's views, here is a request for clarification. If the canvas represents a picture, then it seems as though we can ask whether it is a pictorial representation of the picture or not. If it is, then the canvas is a picture, so Dilworth must mean that the canvas is a non-pictorial representation of a picture. Given this, however, what kind of representation of the picture is the canvas?

I am not sure that psychologists are in as much of a mess as Dilworth's second comment suggests, but even if they are I am not sure how this affects my point about compensation. I agree that discussions of compensation in pictorial representation have been misguided. This seems so because the distinction between bare bones and fleshed out content has not been appreciated. Once the distinction is taken on board, there seems no good reason for claiming that we compensate for odd viewing angles. Maybe compensation itself is a vexed issue, but my claim seems secure even given some confusion in the notion of compensation. If a picture is a viewpoint-independent instance of its bare bones content, then we do not need to compensate (whatever that may be) for anything when we view it from oblique viewing angles.

Reply to Kulvicki's
John Dilworth
Jul 23, 2005 19:24 UT

Kulvicki asks what kind of representation of the picture is provided by the the canvas, on my view. In my book, The Double Content of Art, it is described as 'aspectual' representation, while in the just published JAAC paper "A Double Content Theory of Artistic Representation", vol. 63 no. 3 (Summer 2005), pp. 249-260, it is described as a relation of 'indication', to distinguish it from the more usual sense in which a picture represents its subject matter. See those sources for more details.

Second, on the issue of perceptual compensation, Kulvicki states: "If a picture is a viewpoint-independent instance of its bare bones content, then we do not need to compensate (whatever that may be) for anything when we view it from oblique viewing angles." Since I deny pictures have any such bare bones content, I can't directly comment on whether viewing it would involve

compensation. But insofar as any content must be cognitively recognized by perceptually viewing a picture's surface, it's clear that some perceptual compensation must be involved, since different viewing angles will produce different retinal images, which require further cognitive processing to extract their invariant structural features providing bare bones content etc. But normally this is indeed a trivial and habitual operation, so I wouldn't disagree with a suitably translated analog of Kulvicki's view for contents understood my way.

If I may also venture a broader comment, I think that Kulvicki's general project of attempting to explain representation in terms of objective similarities, with no appeal to specifically perceptual concepts--as in his JAAC paper "Image Structure"--is a very valuable one. I don't think that ultimately it can succeed, because in my view both representation and content are intentional concepts that can't be pried apart from artistic intentions and contextually based, partially culturally specified perceptual recognition and reference, but it's very useful to have opposing views available that attempt to deny such integral connections.

Bare bones content and 'ordinary' perception
John Zeimbekis
Jul 8, 2005 0:33 UT

I find John Kulvicki's distinction between bare bones content (BBC) and fleshed out content convincing as part of an objective account of picture content, or more precisely picture-meaning (since it allows us to refine the concept of satisfaction as it can be used to define the truth conditions of a picture). However, I'm not sure I understand how the distinction would square with a mental account of how we understand pictures. The difficulty can be captured by thinking of a marginal case:

When we see a trompe-l'-oeil, we see a picture with BBC and fleshed out content. But there is no reason to think this perception is (either phenomenally or physiologically) any different to perception of corresponding 3D scenes. Does this mean that all perception of 3D scenes involves BBC? Presumably the conclusion cannot be avoided by equating BBC with 2D vision, because the latter doesn't count as content by any definition, whereas BBC is after all a matter of having the full-blown contents caused by looking at a 2D surface.

If the undesirable conclusion is that we see all three-dimensional scenes as flat (ie, have mental contents to that effect), a natural way to avoid the conclusion is to deny that there is bare bones content in picture perception.

Reply to Zeimbekis
John Kulvicki
Jul 10, 2005 0:21 UT

I thank John Zeimbekis for his helpful comment and for being the third John to contribute to this discussion. My guess is that his worries can be cleared up by distinguishing the contents of artifacts from the contents of mental states and epistemic from non-epistemic senses of 'see'.

First, when we see any picture we are seeing an artifact that has content and we are usually in a position to know what that content is. When we are just looking at the world at large we are not generally seeing artifacts that have content, so perception of 3D scenes in general does not involve perception of things with BBC. Perception involves having mental states that are, in my view, representations with content, but I have not said anything about the contents of mental states and my view of pictorial content does not commit me one way or another on that issue.

Being fooled by a trompe l'oeil painting is an interesting kind of misperception. One sees a painting with bare bones and fleshed out content, but one does not see the painting as a painting or the content of the painting as the content of a painting. This is interesting because one does come to know the content of the picture quite accurately, with the misperception concerning only the painting and the status of the content as content. Being taken in by trompe l'oeil may involve a perceptual state with the same content as a perceptual state associated with veridically perceiving the picture's content directly, but this does not entail that ordinary perception involves BBC.

There is still something strange going on here, however, which John's point helps bring into focus. We rarely (1) see a representation, (2) fail to see it as a representation, but (3) mistake the representation for its content. This is due to the fact that pictures not only represent, but are instances of their own bare bones contents. And it is in virtue of being an instance of those rather abstract features that one is able to flesh out a picture's content. So John's question could be: does perception tend to work by picking up on rather abstract features of the world and computing from them to the richly detailed perceptual states that we enjoy? If so, then the answer is "yes". Pictures do exploit the way in which we perceive things generally, and bare bones content seems to be an important part of how they do so. When we see a depicted scene, as opposed to the picture of it, similar processes are active in us, and in a true trompe l'oeil situation, the processes might even be indistinguishable from one another.

Here is one final point. Even when we do see a picture as a picture, and its content as its content, we rarely notice a picture's bare bones content as such anyway. On my view, BBC plays an important epistemic role in coming to know picture content but it is not something we generally notice. In this way, BBC plays a role akin to the role sense data played for philosophers in the first half of the 20th century. Sense data were a bad way to handle the problems of perceptual knowledge, but BBC is a great way to handle the problems of pictorial content. For this reason, I was pleased that John's comment had some similarities to the argument from illusion.

Film as Dynamic Event Perception

Heiko Hecht (Psychologisches Institut der Johannes Gutenberg-Universität)
(Date of publication: 19 September 2005)

Abstract: This paper assesses the differences between natural viewing and motion pictures viewing. This is done from a psychological perspective labeled dynamic event perception. A set of perceptual regularities constitute natural events. Film has the opportunity (and artistic necessity) to violate some of these regularities. But why do directors choose to violate some laws of natural viewing while they stay away from violating others? I argue that directors have violated almost every single spatio-temporal law that holds for natural events. The causality of natural events, on the other hand, is rarely touched in film. Directors fall into a realism trap that prevents them from commit ting causality violations.

1. Introduction: The reality of film

When Daguerre announced the invention of his photographic plate technique 1839, many artists considered it to be the perfect tool to achieve easily what naturalistic painting had sought to achieve all along, namely the realistic rendition of views of the world (e. g. Scharf, 1983). As a consequence, the photographic approach to realism has fundamentally changed the world of pictorial art. Painters no longer attempted to render the world naturalistically, but they started experimenting with the medium of painting. Similarly, film has freed still photography from naturalistic rendering (Krakauer, 1960). It might be the case that our views will change, once more, with the advent of virtual environments. However, it might also be the case that fundamental ecological constraints of event perception set the limits for the development of realistic rendition. The transition from the arrested or frozen optic array, which is constituted by a still photograph, to the progressive optic array (see Gibson, 1979) means nothing less than the discovery of the ecological way to render events. One of James Gibson's great insights is that photographs are a special case of an arrested or frozen optic array and that the motion picture has to be regarded as "the basic form of depiction" (1979, p. 293).

But what constitutes natural perception? If we consider natural perception of events and the underlying psychological processes, we will be able to identify where film deviates from these and what the perceptual consequences of such deviations entail. I will describe the ecological principles by which we arrive at the immediate experience of objects and their properties. Then I will analyze where film must, for structural reasons, violate certain of these perceptual processes and where film has the choice or artistic freedom to violate others. Finally, I diagnose a self-imposed constriction of this freedom.

2. Kinematic specification of objects and events

No matter how expertly a still photograph is manufactured, the mapping between real-world object and its depiction is asymmetric. While an object - when photographed from a given vantage point - can only produce one particular photo, any given photo is compatible with an indefinite number of 3-D scenes, of which it could be a photograph. Figure 1 illustrates this asymmetry or inherent underspecification of the referent by the photograph. The photo at the top could represent one bi-colored object, or it could represent two separate overlapping objects. Only additional information such as a moving camera can resolve the underspecification problem. In Gibsonian terms, motion provides the invariant structure of the object, often referred to as the principle of structure-from-motion (SFM). It is irrelevant for SFM whether motion is introduced by a displacement of the observer or by displacement or rotation of the object. Motion also allows us to pick-up of affordances of objects (Gibson, 1979), such as being reachable or providing a support surface for the observer. We are able not only to perceive the motions or kinematics of objects but also the more complex dynamic variables such as mass, friction, force, momentum, and energy. The underlying principle of kinematic specification of dynamics (KSD) states that direct perceptual qualities emerge when the dynamics of a situation are sufficiently specified by its kinematics (for a description of the principle see Runeson & Frykholm, 1983; for a discussion of the difficult theoretical status of the concept see Hecht, 1996).

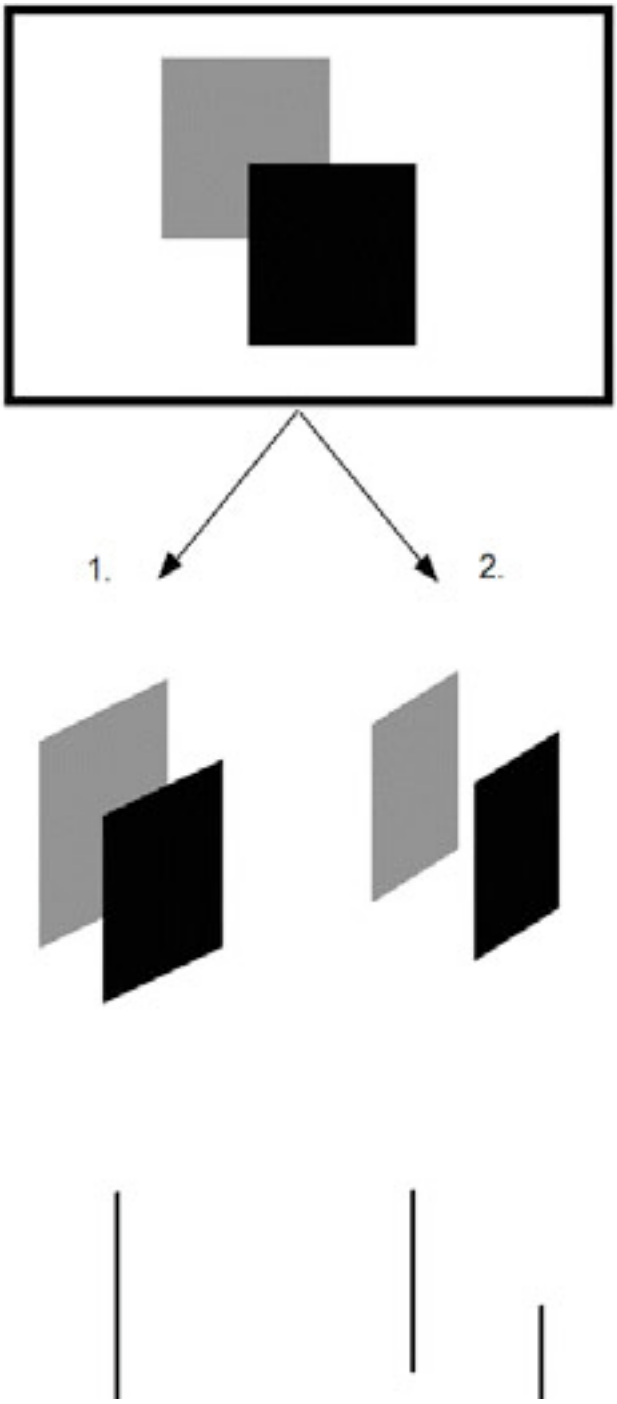

Figure 1: The image at the top is underspecified because it can represent an indefinite number of 3-D objects. Two conceivable interpretations are shown: Interpretation 1 suggests one bi-colored planar object. Interpretation 2 suggests two separate overlapping objects. Only additional vantage points, as provided in the middle and bottom panels can solve the underspecification problem.

Applied to film, the KSD principle makes the claim that as long as a particular invariant is given in the kinematics of the depicted motion, it allows the visual system of the beholder to extract these invariants in a direct manner. And in fact, studies demonstrate that even extremely simplified movies, say of an actor lifting a box of unknown weight (Runeson & Frykholm, 1981; Bingham, 1993), provide the observer with remarkably accurate judgments of dynamic facts, such as the approximate weight of the box. What makes event perception possible in the first place allows movies to be as natural as

they are. Maybe events specified in film are best thought of as a duplication of reality, although Gibson may not have liked the term.

3. Depicting events

To understand the role of event perception (Cutting, 1981; Hecht, 2000) for the analysis of motion pictures, we need to consider the basic relationship between the world and its depiction. To do so, let us take a look at the theoretical space of depictions regarding to what extent they approximate the real world. At the bottom of this theoretical space (see Figure 2) we find pre-Renaissance painting that conveys symbolic meaning but also some qualitative information about spatial relationships, as for example established by one surface occluding another. The discovery of linear perspective introduced a leap in fidelity, while photography provided a speedy automated way to conserve the optic array, which was already approximated by linear perspective. Motion pictures represent another qualitative step as argued above. I hold that even at the extreme end of this theoretical space it is impossible to imagine the perfect depiction that is no longer distinguishable from its referent. Using the conceivably best virtual environment system (VE) with close to infinite resolution and perfect response characteristics (no lag-times) we continue to receive information about being in a physical world that differs from the visual one. Such information is provided mostly by other senses, such as odors, gravity, wind. In other words, the unity of the senses is still lacking. However, all the visual affordances and invariants are present in the VE. And most of these are retained in the movie whenever the camera takes over the ability to explore, which has been lost by the observer.

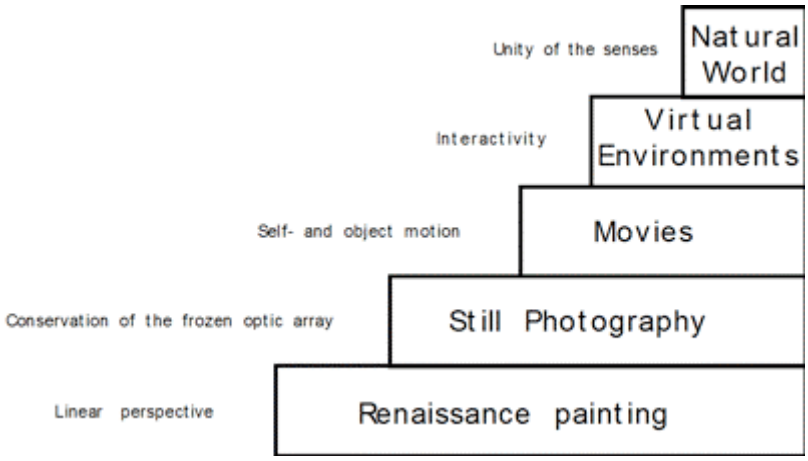

Figure 2: From bottom to top, the pyramid of visual techniques shows what each of them added to pre-perspectival painting.

Thus, the movie has obvious shortcomings compared to the (unreachable) perfection of a VE, and certainly compared to natural world. But in these shortcomings, I claim, lie the movie's unique ability to violate the laws of nature. In the space between poor drawing and perfect VE, the movie has sufficient veridicality to convey rich invariants very similar to those available in natural events, and it need not obey the laws of nature or the laws of interactivity that constrain VE. Hence, the movie is the perfect medium to create fictional worlds defying the laws of nature. Strangely, the fictional worlds that have been created by film directors are mostly very conventional and natural law abiding. To prove this point, I need to scrutinize violations to natural event perception once in terms of what is possible in principle and then in terms of what violations are commonly made.

4. Violations of the rules of natural viewing

It is possible to violate most of the laws that govern external events. I claim that this ability is unique to motion pictures and responsible for a good part of their fascination. In other words, the true power of a motion picture lies in its ability to specify events that are impossible in the natural world. These cases, which I call violations of natural viewing invariants, produce emotions like joy and magic. To support this claim I will now take a closer systematic look at different violations of internal and in particular external events. These violations are the key to understanding filmic event perception.

4.1. Violations in frozen renditions

Neglecting the rather inconsequential deviations from correct viewing size, pictorial violations focus on depth relations. They have been discovered by artists consistent with my earlier claim that still images leave no more room for artistic exploration. For instance, the "Waterfall" by Escher (Figure 3) violates global depth relations while some of Magritte's paintings violate the law of occlusion (i. e. the fact that closer objects visually cover up objects positioned behind them), as in "Le blanc seing" (Figure 4) where the lady on the horse should be occluded by the tree in front of her.

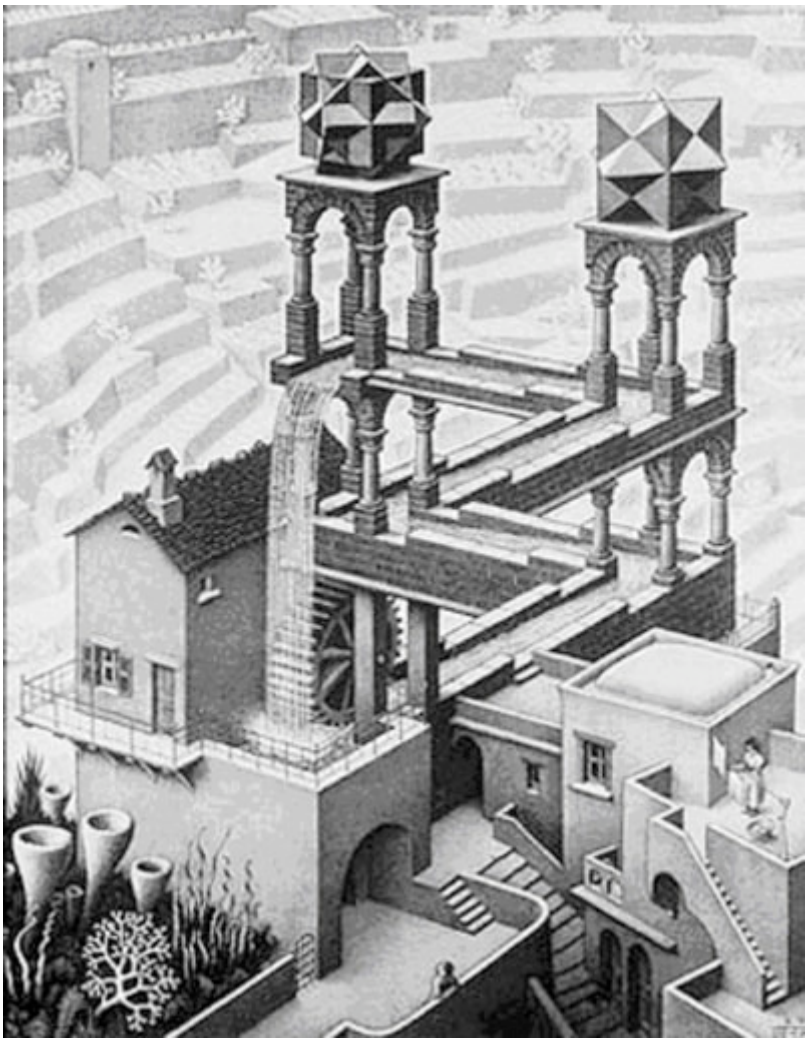

Figure 3: M. C. Escher: Waterfall (1961). Note the inconsistencies between the top and bottom of the supporting columns. Subtle violations of depth relations create the paradoxical outcome of a perpetuum mobile in the viewer's mind.

The depth violations just mentioned could in principle be transferred to motion pictures. Imagine water running down Escher's waterfall and the mill's wheel turning. What a strange paradoxical world we were to enter in such a movie. Or the animated version of Magritte's rider: imagine the lady's surface texture on every other tree behind which she rides. Let us note that these violations are of such fundamental nature that it is not surprising we cannot readily think of any motion picture that has explored them. At the same time there is nothing that prevents experimental film to do so. The film work of early cinematographers such as Hans Richter, Fernand Léger, and Maya Deren took steps in this direction of "animated painting" (Deren, 1960), but this venue soon became abandoned. This might explain why some of her short films such as "Meshes of the Afternoon" (1943) with its causality-defying events to this day appear revolutionary.

Figure 4: René Magritte: Le blanc seing (1965)

Other violations common in frozen pictures have not been carried to the movie. For instance, alternatives to linear perspective (e. g. Barre & Flocon, 1968) have only been employed when inevitable as side-effects of extreme focal lengths, or for short sequences to indicate inebriation or dream states of the protagonist, such as in Alfred Hitchcock's "Spellbound" (1945).

4.2. Violations in dynamic renditions

In addition to the radical violations that play with pictorial integrity, moving images open up a whole new realm of possible violations. Spatio-temporal violations abound in movies.

Violations of space and time

The visual system seems to ignore or compensate for many non-rigid transformations that result as a consequence of the "wrong" position (Cutting, 1987; Kerzel & Hecht, 1997; Yang & Kubovy, 1999). Many other inconsistencies, such as replacing a camera approach with a zoom usually go unnoticed as well. These internal events appear to be interpreted correctly when they suggest observer motion and they tend to be ignored when they introduce some unwanted distortions. Other spatial violations are common, for instance cases that suspend the fundamental truth that two objects cannot occupy the same space. In Ivan Galeta's "Two Times in One Space" people split, which gives them a phantom like quality (see Bordwell & Thompson, 1997).

Cuts usually introduce violations of the natural flow of time. Unlike any other possible violation of natural event perception, temporal violations have become the most widely used and discussed film technique (Bordwell & Thompson, 1997). A typical action movie contains as many as 2000 shots and more breaking the natural flow of events with every cut. Time lapse and slow motion are used subtly in almost every (action) movie to emphasize and de-emphasize parts of the action or to make scale models appear more natural. And in Godfrey Reggio's "Koyaanisqatsi" (1983) temporal compression and dilation have been used to the fullest range. Interestingly, repetitions are used much less frequently. Leni Riefenstahl employed repetition as a form of temporal violation in her documentary on the Berlin Olympics ("Olympia" 1938) containing such shots as a series of short cuts showing several athletes in the moment of soaring off the high bar without ever showing a landing in-between. At the level of the storyline where the film constructs time, duplication and fragmentation are common tools such as in parallel action sequences or in fragmented action, as in Edwin Porter's "The Life of an American Fireman" (1903).

Thus, high-level temporal violations have become standard repertoire while their low-level counterparts remain the exception. Hochberg and Brooks (1996) point out that what makes cuts visually comprehensible is not a conventionalized film grammar, but rather the avoidance of unnatural apparent motion effects. This is supported by findings that the visual system is very forgiving when scene changes are introduced during eye-movements, or during other visual disturbances. This so-called phenomenon of change-blindness (O'Regan, Rensink & Clark, 1999) suggests that observers fail to notice even large objects that are added or removed from the visual field during eye-movements and presumably also during cuts. The internal representation of events is so sparse that disruptions are easily tolerated. This can explain the tolerance for high-level violations but not the system's sensitivity to low-level violations. A scene played backwards is just as smooth as its forward counterpart but it looks wrong and - for some of us - funny. I claim that reversals only look wrong if event causality is violated (see below).

Violation of internal events

A number of optical changes specify what Gibson called internal events. Normally internal events go hand in hand with changes of viewing direction, head position and locomotion. In motion pictures the camera replaces the head and records the optical changes in the eye's stead. Changes in camera position, angle, focal length etc. all contribute to filmic internal events. Contrary to Hochberg and Brooks' (1996) claim that some movement-produced information is ignored or contradicted by film makers, as for instance in a trucking shot, it might be better to think of the camera as an omniscient observer. In the movie theater the spectator can locomote in ways she normally does not or does not have the means to (levitate, walk backwards without looking in that direction, fly, shrink to fit into a keyhole, etc.). Under the premise that the observer attends to the window into the film world provided by the canvas, is the director is confined to playing with these optical changes never exceeding what an infinitely fast, movable and scalable observer could do?

The first attribute of an internal event discussed by Gibson is dynamic occlusion consisting in the progressive deletion and accretion. It occurs at occlusion edges but not at edges inside an object, such as color boundaries. For instance, when a car moves on a road some texture gets deleted at the front of car, and at the same rate texture gets uncovered behind the car. Precisely because of this smooth deletion on one side and accretion on the other does the visual system signal a moving object with its direction of motion pointing in the direction of the texture deletion. It is inconceivable to have systematic deletion and accretion but no motion or vice versa to have motion but not texture change. Thus, we are dealing with a universal law of perception. The invariant of accretion and deletion cannot be violated in film.

Perspective transformation and the apparent foreshortening of objects when different viewpoints are assumed by the camera, on the other hand, can be manipulated. It would make for a very strange world indeed, if every time the camera moves to the left one particular object were to behave as if the camera had moved to the right. However, note that such violations are not impossible, the object could have turned at exactly the same time the camera did. Likewise changes of perspective in parts of the scene are consistent with parts of the visual world warping. Such violations have been used for dream scenes and the like.

The zoom is the only standard technique specifying an internal event that is not reproducible by the above omniscient observer. All other internal events do not violate what this observer could experience. An observer approach (dolly shot) should cause dynamic occlusion but a zoom does not. The fact that nonetheless zooms do not look strange or unnatural is remarkable. It might be explained by the familiarity with binoculars or - more likely - by the failure of observers to discriminate the subtle differences between zoom and actual approach.

In summary, those invariants specifying internal events that can be violated are typically not violated by film makers with the exception to achieve special unrealistic effects. Presumably the transformational invariants that specify observer motion need to be left untouched in order to prevent the observer from attending to the fact that she is not actually where the camera is.

Violation of event causality

At the higher level of meaningful perceptual events the director can use layout specification to create countless external events that defy the very causal laws that govern our world. For layouts can be specified that are inconsistent with almost any law of physics that we can think of, such as the law of gravity or the law of energy conservation. If Wile E. Coyote - after running off a cliff - remains suspended in mid-air for an instance before "remembering" the law of gravity and then inevitably falling to the ground, the visual system has the choice of a) reinterpreting the timing of the scene and conclude an immediate fall, b) question the pervasiveness of gravity, or c) decide that the situation is unecological. Presumably, the scene is funny just because c) is concluded. Otherwise we would probably not take any particular notice. And as a matter of fact, the temporal suspension of gravity has to be timed just right for the effect to be noticed. Hecht & Kerzel (2003) have presented observers with a computer-animated scene of a basket ball propelled toward the floor and rebounding at an angle. Upon varying the ball's deformation such that it happened several frames too early or too late, observers rated the early deformation as natural as the canonical event, whereas the delayed deformation looked goofy. This is evidence that the visual system anticipates the mechanics of animated events even if the animation is rather crude. It must have knowledge of classical mechanics at a very basic level. Thus, Wile E.'s fall is anticipated. The brief discrepancy between anticipation and visual evidence produced the humorous effect.

Basically, film can specify an indefinite number of layout changes and combine them such as to violate all causal relations that govern complex natural events (Kerzel & Hecht, 2001). One can make the case that the underspecification problem that Gibson's approach so nicely solved for natural scenes is not only unresolved in film at the level of causal interactions, it is even exacerbated because a new class of possibilities arise, the unecological. Take, for instance, the bullet that slows down in front of Keanu Reeves in "The Matrix" (Wachowski Brothers, 1999) and then can easily be plucked out of the air by him. Is a real bullet specified at the moment the trigger is pulled and a fake one as it gets close to Reeves? Is the thin medium air specified at first suddenly replaced by an invisible thicker medium? Does the bullet have a propulsion of its own, or does the hero have strange powers? The plot makes us believe the latter, but without knowledge thereof we are at a loss. The situation is no longer uniquely specified. Here we touch on a major assumption that the viewer has to make. The assumption that we live in a terrestrial environment. As reasonable - and unnecessary - as this assumption may be in the real world, it is no longer mandatory in the realm of film. The underspecification problem is wide open again as soon as we have to drop the terrestrial environment assumption.

We have an exceedingly hard time parting with this assumption. We have yet to encounter a movie that carries through the consistent violation a basic law, such as gravity. Imagine a movie where everything works normally as long as objects are in contact with the ground plane, but as soon as they loose contact they fall upward until they touch another surface or else disappear forever into the sky. We would walk around but never lift both feet off the ground in the outdoors. We would need no garbage collectors and lifting someone off the ground would be murder. Such an alternate world would be strange and powerful once the viewer buys into it. Is this possible? Or would our visual system be at a complete loss? Could this be the reason why many of these effects have not yet been explored by film makers? Current cutting-edge movies are not pursuing this venue, as if the viewer could only tolerate minor modifications of terrestrial physics, and those only if limited to heroes and magic situations.

5. Realism

If we apply a realist interpretation to the principle of direct specification (KSD) in ecological theory, we have to conclude that the visual system would be at a loss once "impossible" events are specified. However, I do not believe that our visual system is constrained to perceiving ecologically possible events. To the contrary, it is extremely flexible and plastic. We have no trouble understanding footage taken of astronauts floating in weightlessness, and we can get used to objects that fail to fall down. Fears that the visual system might not be able to handle speeds of locomotion exceeding that of a horse turned out insubstantial when fast railroads came along. And fears that the visual system might not support spatio-temporal violations when cutting from one scene to another were likewise misguided. Causality violations have just not (yet) become fashionable. This would entail that Gibson's realist position (1978, 1979) can no longer be applied to avant-garde motion pictures. Gibson may not have not have realized the dissolving power that movies could have on his realist position.

Let us go back to the question posed at the beginning: What is the best pictorial rendition of reality? I claim that most directors strive for realism and that they are aware of small violations of event invariants that are required to maximize realism. This is why we see mostly films that timidly violate a select few event invariant. Just as painters knew that in certain instances linear perspective had to be violated, directors learn to create a film world that looks most realistic. In painting and photography, for instance, spheres far from the central camera axis should be depicted as ellipses, but they look more natural when they are painted as circular areas, which is exactly what Renaissance painters did (Pirenne, 1970).

Experimental film aside, movies attempt to make things look natural, even animated cartoons do (DeMarchi & Amiot, 1977), although they sometimes plays with the medium. Kracauer (1960) calls this the realistic tendency of film. Cartoon directors have only scratched the surface of what is possible. And when they did scratch it was for funny effects rather than to create unreal worlds. A notable exception is Disney's "Fantasia", which attempted to create a visual analogy of sound. For instance the section on Bach's "Toccata and Fugue in D Minor" was used to inspire a series of entirely abstract images: shapes dance around completely defying gravity, there is no story, the silhouette of Stokowski dissolves into blotches of color, place and time lose their narrative meaning, terrestrial causality is inexistent, objects are reduced to their traces, etc. In this respect Fantasia was (and still is) a highly revolutionary film. Its flop at the box office when it was released in 1940 seems to prove the point that it was basically an experimental film (see Culhane, 1983). Its recent sequel (Fantasia 2000) is more of a success I dare say because its flying whales are closer to terrestrial reality.

Thus, realist film only deviates from true rendering (of event causality) in order to make things normally unseen visible, to emphasize the small by making it big, the transient by rendering it visible (see the revealing function of film, Kracauer, 1960). But we do not have to be realists, neither in film nor in reality, to benefit from an analysis of natural event perception and its potential violations. Interestingly, if we do not follow Gibson in his realism but rather assume that the visual systems needs to interpret and infer its precepts in all cases, be it natural vision or filmic events, hardly anything will change in our analysis. The visual system is then confronted with a discrepancy between well-ingrained inferences in natural viewing and less ingrained or inconsistent inferences in the case of watching a movie.

Since the advent of virtual environments (VE), we have a new generation of visual renditions that may well be the final step to visual realism. In VEs the internal events are qualitatively different than in the movies. The visual scene changes with head and eye movements almost the way they do in natural viewing. In other words, in film the efference-afference coupling is broken, head- and eye movements are inconsequential as long as I keep the screen in sight. In VE's the illusory visual world is a function of our real movements. Obviously, most extraretinal cues that normally accompany vision are still absent (vestibular stimulation, tactile feedback, kinesthetic cues, fluid shift in the body etc.). Notwithstanding the remaining differences between natural and VE viewing, I suggest that this qualitative step in the visual media should make realist film theoreticians reconsider the role they reserve for traditional movies. We may well be at a turning point where movies be freed from the burden of being the best technique of pictorial rendering that we have. Once freed from this task maybe they can lead us into new (experimental) domains of events that are physically impossible.

6. Conclusion

I have shown that facts and theories of ecological event perception can explain why film has yet to undergo the stage that painting and still photography have undergone already, namely the unconditional experimentation with the limits of the medium. As Gibson (1979) has noted, the main difference between the perceptual awareness provided by film compared to that provided by real events lies in the lack of intentionality and interaction that occur natural when we look at an object, walk up to it, touch it etc. Granted this difference, however, optical events are usually created to be as similar to real events as possible. The reason for this lack of adventurous spirit, I claim, is constituted in the need to always have one medium of depiction that fulfills the necessity to render naturalistically. To this date film represents this medium. Note that this need corresponds to Bazin's (1967) notion of the psychological need for realism. But in contrast, I suggest qualitative jumps in realism were constituted by photography, film, and ultimately by virtual environments. While these media share many aesthetic features, they are vastly different from the point-of-view of perceptual psychology. The importance of motion was only appreciated comparatively recently (see Cutting, 2000) and the importance of action for perception is often relegated to ecological psychology. At least from an ecological perspective, the three visual media are vastly different in terms of the provisions for realism that they make.

With the new medium of virtual environments around the corner of mainstream entertainment, will non-interactive film be succeeded by interactive VEs in its role? And will traditional film hence become a medium for experimentation? Inferring from the past we can make this prediction once the main function of the motion picture, to tell a story, can be accomplished in VE. A story in this new medium has to be interactive, that is the spectator has to be able to manipulate the outcome of the story, or in the case of historical VEs should at least be able to move around on the Waterloo battlefield as combat rages. The latter might be easier to accomplish than a spectator-contingent story development, which would require programs for the robot-like agents reacting to the spectator's moves. Certainly once we are able to create interactive simulations of the quality envisioned in "Star Trek" with help of the holodeck will the movie screen look completely outdated. In this case we could look forward to decades of testing the limits and exploring new violations of ecological event structure in movies. I hope that my reflection on what constitutes such violations in the face of realism can be used to analyze this potential development. Maybe the innovations in some music video clips are the beginning of film freeing itself from obeying event causality. We might be in the midst of film loosing its role of being the prime medium to render reality. On the other hand, motion pictures may have reached a degree of realism that approximates a ceiling that cannot be surpassed in terms of what is needed for a "perfect" depiction of reality. But as mentioned above, we tend to be conservative until the next innovation teaches us otherwise.

While experienced realism holds the key to evaluating event depiction, the present chapter is not meant to come down on one side of the debate between realists and formalists (Singer, 1998). Rather, I have investigated the realism of film from a mostly ecological standpoint of event perception. My mission was to discover whether Münsterberg (1916, p. 185) was right in stating that "While the moving pictures are lifted above the world of space and time and causality and are freed from its bounds, they are certainly not without law." It is hard to understand why the psychological study of film is so limited. It is fruitful to ask about event perception in film in terms of violations of natural event regularities. It offers a unique criterion to place a director's efforts into a space of what can in theory be done with the medium of film and what the director has chosen to do. If the director attempted to recreate natural event perception as closely as possible, as Evces (1994) suggests Orson Wells did in "Touch of Evil" (1958), we can gauge if he really minimized as many of the violations as he could. On the other hand, we can now start to understand why temporal violations have been explored to the fullest, and why many causal violations have thus far not been tampered with.

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Discussion

Blindness to change?
Jose Luis Guijarro
Sep 23, 2005 8:40 UT

The possible innovations that filmmakers may attempt can be seriously impaired, I think, by our “change blindness”, i.e., the difficulty of perceiving changes not expected, as two pictures in this German paper show:

http://www.spiegel.de/wissenschaft/mensch/0,1518,372161,00.html

Since it is not obvious that every one will be able to read the German text, I will try to explain how to proceed. You click on the first picture of the town by the lake and it will expand. It will also appear in flashes. In every flash the picture makes a big change, but, as it is unexpected, it is very difficult to perceive it. You may, after a while, though. I confess I didn't. Once you are able to see the change, you will subsequently see it immediately with no problem (even wondering why it took you so long to see it!). In the second picture, however, the change is immediately perceived, for it is somehow focused. In the third picture, again, it takes a while to find the change, although it is easier than it was in the first picture.

My question is: if unexpected changes are produced in a film, will they be all perceived as such by viewers? Does not that feature of our perception system preclude the wild innovations we could have in that field? If they are focused or perhaps described BEFORE we look at them, won't that be a hindrance in achieving innovations?

That's why critics exist
José Luis Quiroga
Sep 25, 2005 23:05 UT

I'm not sure what's being analysed here. If the subject is film, then I'm afraid Heiko's paper is too short of giving us any important information of why films are how they are and how they are perceived. And that is basically due to the fact that films are what they are because of the social context in which they are produced. The difference between the possible effects of innovation on viewers depends, I think, on social context, especifically in the way we conventionalize our practices. If we open the link shown by Guijarro already knowing what we are to expect and how to react to it, the differences are very easy to perceive. Regarding movies, our system of perception is conventionalized by years of cinematic productions from the film industry and television. As a public, we go to see a film knowing what a movie is for, just as much as we know what a novel or a painting is for, once literature and painting are conventionalized practices. The filmmakers (like the painter or the novelist) on the other hand, work upon those conventions to decide wheter they introduce innovations. Only an especialized part of the public (the critics), which have trained themselves to look for those innovations, will be able to find them. Those "discoveries", made public, can enter and affect the circuit of production and consumption, creating another conventionalized form of creating and viewing films, until another innovation comes. However, if the subject of Heiko's paper is "the act of perception of a film", we should feel more discouraged to accept his/her explanations for the simple fact that perception itself, understood as the direct mechanical or phsycal relation between man and object, is a philosophical asumption which, although useful for other natural phenomena, is not scientifically useful for understanding culture. And if it is, it goes to the most basic and primary of human relations with the world, which are very important to recognize, but become of a secondary role once cultural artifacts shape our historical consciousness. Since Kant, the world is not perceived in its real nature, it is not a source of data automatically processed by the human mind. It's our mind itself that shapes the world we see. It is not important here to discuss wheter the conditions of our experience exist in our minds a priori or a posteriori, since all that matter is experience itself, but it is important to emphasize that culture has its own autonomy. Escher's "Waterfall" is a perfect example of historical relativism in perception. The only way a viewer can discover violations of depth relations in a painting is once that viewer has been taught to decode that system of representation in which a three dimensional object is simulated in a flat surface. The violations of the rule might not be so obvious for a primitive man not familiarized with such rules.

Nature -> nurture (or vice versa?)
Jose Luis Guijarro
Sep 27, 2005 16:08 UT

It seems that Quiroga is of the opinion that cultural ways of dealing with the world overtake and change our specific natural ways to do so.

On the contray, I strongly believe that our natural cognitive endowement strongly determines what cultural factors may or may not influence us in our dealings with the environment.

But after seeing how we have each of us interpreted the question of innovations proposed by Hecht, it may well be that those two apparently contradictory notions, Quiroga's and mine, are just another case of looking at the elephant from two different vantage points.

In the end, we are saying the same thing! That only those changes that are somehow expected are able to hit our consciousness; therefore, a wild unexpected innovation may pass us by without we ever noticing it.

change blindness is a productive necessity
Heiko Hecht
Oct 7, 2005 13:00 UT

José Luis Guijarro refers to the so-called change blindness phenomenon that Reagan and Simons have recently made popular. Change blindness is old wine in new jugs, a fundamental truth about the severe limits on human information processing. William James, among others has commented on the stark difference between the phenomenal completeness of our perceived world and the narrow focus of our explicit attention. Basically, change blindness is the demonstration that we ignore changes in the vast unattended world at the expense of highly accurate information processing in the focus of our attention. We detect change in the unattended world only when we see a transition, the ongoing change. However, we fail to detect even enormous changes when the whole visual field changes, such as is the case with every non-matching film cut. Film has the option to either direct our attention to the ongoing change (e. g. by avoiding whole scene cuts or by using a fade) or to hide it. The innovative violations of normal viewing that I have in mind for future experimental films cut across the dimension of change blindness. That is, violations of a fundamental law of perception (e.g. motion of the whole visual field is indicative of self motion) can be implemented either in our focus of attention or in the unattended world. The spectator's reaction will of course be substantially different.

natural vs. unnatural
José Luis Quiroga
Oct 9, 2005 18:22 UT

I understand that disagreement between Guijarro's view and mine lies on the fundamental epistemological principles of a given science, in this case, cognitive science. Our discussion would end up being philosophical and relative to the paradigms that result from different conceptions of the world and human understanding of it. Using Guijarro's example, it's true that we may be looking at the same elephant, but giving different interpretations from different point of views. My point of view about what is called "natural vision" or "natural events perceptions" as opposed to artistic or unnatural events is that there's no such thing as a pure, mechanical way of perceiving the world in which there's no influence, for example, of past experiences or the experiences of others. Our necessary inclusion in a community of people determines our individual possibilities, being culture (historical social experience) our true "nature", or if preferred, our second nature, one from which we cannot escape. A baby is born and has (naturally) all the potential of becoming a grown adult with his/her way of dealing with the world, but let alone since birth, will not survive. Social context overtakes the role of nature, even though we are given by nature all our particular abilites. It is a dialectical movement, a process in which history (human experience) not only reproduces, but produces a new reality, which becomes a given stage of mankind. That's why it seems to me that the introduction of the word "regularity" for making a distinction between "natural vision" and "film vision" is deceiving. Regularity itself is not an objective, measurable state, defined by positive sciences. It is just an assumption, a judgement, one that can be easily changed or made irrelevant. In that sense, many innovations on film (as for example, those found in Eisenstein) may have become so typical of massive culture, that they seem to be the normal way of representation, while other method's like Peter Greenaway's still camera shooting a long, uncut dialog scene, which might be considered closer to "natural vision" (although it really imitates vision in a theatrical representation or a framed painting), seems weird or strange to a particular public. In this case, it is the so called regularity (which was first an innovation) that goes unnoticed.

narrative and events, + methodology
John Zeimbekis
Sep 27, 2005 20:47 UT

By 'experimental', Heiko Hecht mainly understands films representing fictional events which violate the laws of the physics, in the same way that (say) cubism violates the laws of phenomenal vision.

But film, just as it is fitted to representing events because it is a temporal medium, is also fitted to representing actions, agents, expectations, disappointments, and all the things people-narrative usually represents. Motionless visual depictions (paintings, sculptures and photographs) could not explore such things. So, why does Heiko think that film will evolve in the same way as these still depictions? Perhaps it will evolve the way other *narrative* forms have, not the way other *virtual* forms have.

I also have a doubt about the wider methodology, which should concern us on an interdisciplinary forum. Heiko Hecht attempts to use findings from psychology to argue a quasi-historical point: ie, to support the claim that film should become a medium for experimentation once richer cognitive environments than film itself become more widespread. But what if the causes for artistic innovation are more chaotic (eg, more dependent on social and economic factors) than the kinds of psychological causes Heiko Hecht describes? There is something reminiscent of Greenberg's claims about the development of painting to its 2D essence in Heiko's claim that film will begin to explore its Lessingian essence [sic] once it is supplanted by richer virtual environments.

Cross-Modal Effects in Motion Picture Perception: Toward an Interactive Theory of Film (Washington University in St. Louis)

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Abstract: Pictures can be distinguished from other symbol types by virtue of the fact that their interpretation is grounded on perception in certain ways. I argue that the relevant perceptual abilities involve heuristic processes that require internal representations. However, those are limited in detail and in the scope of knowledge on which they draw. To a large extent, this is due to the interactive nature of the internal mechanisms that implement them. Cross-modal effects exemplify the relevant type of interactions. I suggest that an analysis of such effects shows that movies as multimedia representations do not constitute a distinct representational genre, characterized by a special mode of interpretation that sets them apart from static pictures.

Introduction

It has been argued by a number of philosophers and scientists that there is a type of interpretation that is characteristic of our response to pictures. It is a type that is dependent on perception in ways that the interpretation of other symbol-types is not (Schier 1986; Cutting 1986; Lopes 1996). On this view, any correct interpretation of pictures must derive from the perception of objects and events, which are recognized in the pictures using ordinary perceptual abilities alone. Moreover, it is said, any interpretation that is so derived will, to that extent, be correct. Pictures are distinguished from words in that respect. The referent of a word is not recognizable in it; thus the possibility of basing a correct interpretation of words on ordinary object and event recognition processes does not arise.

These claims depend on showing that ordinary perceptual processes are not themselves entirely continuous with cognition; that is, that they are not highly dependent on background knowledge and beliefs or *theory-laden*, to use the classic phrase. The danger here is twofold. First, a perception-based theory of pictorial interpretation purports to be a naturalistic account, one that focuses on perceptual psychology rather than on a theory of conventional symbols or signs. But the deep cognitive penetrability of perception can underwrite substantial cultural differences in perception, and those might be viewed as driven by local representational conventions, rather than simply being embedded in them. In that case, the conventions rather than psychological processes may become the central focus in explanations of pictorial interpretation, leaving naturalism in the dust. Second, the door is opened by pervasive cognitive penetrability to the invidious effects of individual differences. Those could take the form of idiosyncratic beliefs and desires, which cannot be accommodated by explanations of a general sort.

An obvious response to this problem would be to appeal to the strong modularity of vision, which blocks cognitive penetrability by imposing limits on the types of knowledge upon which low-level perceptual processes may depend. Unfortunately, the evidence suggests that the strong modularity thesis is false. In any case, even if the thesis were true, the modularity of low-level visual processes would not suffice to ground pictorial interpretation in the way that the perception-based approach requires. The reason is simply that the outputs of such modules will always underdetermine conscious, central interpretive processes, which can take highly variable forms. One might get around this problem by positing centralized modules as well. These would take the form of potentially conscious reasoning processes that draw upon a knowledge base sufficiently broad to constitute a theory, but of a domain-specific sort; that is, a theory that does not depend on other theories and involve access to knowledge and belief of any and every kind (cf. Melzoff and Gopnik 1997). However, as we will see, while higher-order structures and processes may sometimes be involved in picture recognition, understanding what a picture represents based on them alone does not constitute thinking and reasoning about what a picture means. Moreover, it is not necessary to treat such knowledge representation structures as drawing on encapsulated, domain-specific theories in order to give a perception-based account of pictorial interpretation. Knowledge effects can be constrained in another way. In any case, I will cite evidence that indicates that interpretation does not always depend on such higher-order structures of the relevant type.

The evidence that I will consider concerns *cross-modal effects* in perception, specifically interactions of sights and sounds. Such effects can take a variety of forms. My concern here will be with cases of

cross-talk in adult perceptual systems, in which one sense modality, operating normally, influences a different modality. For example, the 'ventriloquist effect' is experienced when the location of a sound source is influenced by visual stimulation; in other cases, sounds affect either the location or the quality of the visual stimulus. I suggest that the knowledge that cross-modal interactions require is limited in scope and content. What is important about them in that regard is that they comprise heuristic processes, in several respects: They make possible a limited reliance on internal representations, thus economizing on available mental resources. And they provide a means of categorizing events that is reliable, but also fallible and not fully rational. However, as will emerge, heuristic processes play central roles in a variety of theories, in which perception is said to be either more or less knowledge-dependent than on the view that I espouse. Thus it is crucial to be clear about the differences among these accounts.

If a distinctive mode of pictorial interpretation can be grounded on perception in this way, the question then arises of whether it is possible to make even more fine-grained distinctions, on the basis of which movies (construed as multimedia representations) might be the object of a special type of interpretation, not applicable to paintings or photographs. [1] Certainly there are important differences between the perceptual experience of paintings and film. Except for ambient noise, sounds do not usually figure in the perception of pictures in the museum or gallery. Moreover, unlike paintings, motion pictures represent events dynamically, i.e. as unfolding in real time, and they are typically viewed transiently under time constraints. Thus there is no opportunity for viewers to scrutinize scenes. Nonetheless, it has been said that "cross-modal interactions are the rule and not the exception in perception …," and if that is true, then they should figure in a larger account that includes still photographs and paintings as well (Shimojo and Shams 2001: 505).

Cross-modal Effects: Theory and Evidence

There are three models in terms of which cross-modal effects have been explained: (1) a strictly feedforward account in which the visual and auditory systems operate independently and in parallel to provide information that is used to construct a higher order representation in multi-modal areas of the brain; (2) a model that locates the effects of one sensory system on another within the primary sensory areas themselves, but in a way that is mediated by feedback from multi-modal cells; and (3) an explanation in terms of direct links from one sensory system to another.

The first of these models treats sensory systems as modules and is undercut by evidence in that regard. It is purportedly bottom-up, in the sense that early sensory processes are unaffected by higher-order perception and cognition, and because the latter are supposed to constrain the former by providing particular kinds of input to them. However, the implication is also that there is a final, conscious stage in which all sensory inputs are combined into a coherent whole (whether processing is actually completed in multimodal areas themselves or not), and that is a view that is now widely rejected.

While there is evidence that there are, in fact, direct connections between sensory systems, lending support to the third approach, there is also substantial empirical weight on the side of the idea that interactions across systems are mediated by higher order, polysensory neurons. For instance, animal and human studies show that activity in such neurons is enhanced with cross-modal interactions; the amount is greater than the sum of activities in the unimodal areas that are involved (Bushara et al, 2002, p. 191). In light of that, I will concentrate on the second model, in which the potential for top-down effects and learned variations across individuals and cultures is greatest. There are a number of candidate regions for multimodal mediation, some cortical and some not (e.g. superior colliculus and prefrontal areas). The question then is how much *integration* is required of the auditory and visual features and what effect, if any, does the integration have on the encoding of the elements themselves? To the extent that integration is required, in a way that modifies the use of basic movement features, by what principles might the modifications be constrained?

Consider, then, two recent studies of cross-modal effects. The first involves the ventriloquist effect, which obviously plays an important role in the experience of film. In a series of experiments, Vrooman and de Gelder (2004) used a basic paradigm in which a centrally located sound from a computer screen was heard as alternating between left and right when synchronized with lights flashing in the relevant locations. It might be thought that this phenomenon depends on a higher-order judgment

about the source of the sounds, based on assumptions about how visual stimuli and sounds are usually paired. Or perhaps the result was due to a post-perceptual Stroop-like response competition: Visual object recognition is a largely automatic response, we might say, that competes with a process of locating objects by attending to sounds and makes it difficult to separate them from the visual stimulus. Finally, an argument could be made that the subjects are responding strategically in the face of what they believe to be task demands. Noting the odd way the stimuli are presented and speculating about the experimenter's aims and goals, they respond appropriately.

However, Vrooman and de Gelder found that subjects could not be trained to ignore cross-modal effects, and the effects occurred even when psychophysical measures of responses were used about which subjects could not strategize. The response occurred more-or-less automatically, thus ruling out explanations in terms of higher order judgments and strategies. Further, it appears that neither attention nor consciousness were required to produce the interaction. Specifically, focusing attention covertly on the shifting visual stimuli (as opposed to monitoring the center of the screen) did not produce a stronger ventriloquist effect. Moreover, introducing exogenous attention-attractors on one side of the display did not shift the location of the sound in that direction. When a 'singleton' feature (one small square in the context of other large squares) appeared on one side of the screen (with only large squares on the other side), such a feature would be expected to pop-out and draw the subjects' attention. Nonetheless, the location of the sound actually shifted away from the presumed focus of attention and in the direction of the larger stimuli. Finally, in this study, cross-modal effects were found in patients with unilateral visual neglect. This syndrome is sometimes said to be an attention deficit; in any case, it results in the patient being unaware of the presence of an object on the relevant side, although he can still process some information about it. In this case, subjects failed to detect the visual stimulus in their left visual field; nonetheless, their pointing to a sound was shifted in the direction of the stimulus. Thus neither attention nor awareness was required for the effect. This conclusion is supported by other evidence showing that one sense modality can influence the response to another, even when they are not presented simultaneously; i.e. in situations in which attentional binding is not involved. For instance, Braille reading by early blind subjects has been shown to produce activity in visual cortex (Sadato, et al 1996 pp. 526-528; cf. Shimojo and Shams 2001, p. 505). Further, brain imaging studies by Bushara, et al (2002, p. 190) show that, while activity in multimodal areas increases with sound-induced changes in visual motion perception, the activity is actually lower in unimodal sensory areas. This is not what would be expected if attention were in play.

The second study is one in which sounds affect the perception of a visual stimulus, using a 'collide-or-pass' paradigm. In this type of experiment, subjects see two spots move toward each other, intersect on the screen, and then move off in a different direction. However, the movements can be seen in two distinct ways. On one interpretation, both spots move in straight line trajectories, merely passing by each other, producing an X pattern. On the other interpretation, they collide, and both spots change direction, being reflected by their contact. With no sound, collisions are sometimes seen, but the 'collision' and 'pass' interpretations are equally likely. However, if a sharp click is heard at the moment when the two spots intersect, 'collision' becomes the favored response. Note that in this case, not only the perceived direction of movements, but also the description of the event is affected by the cross-modal interaction. This determines the mathematical and physical principles that would be appropriately applied: collision dynamics for one response, but not for the other.

It is important that the effect of sound on the perception of movement does not appear to be due to a prior categorization of the event as a collision. There is no significant difference in brain activity when subjects describe the objects as colliding, as against subjects classifying them as passing, when there is no added sound (Bushara et al 2002, p. 194). If the brain activity corresponding to the perception of a collision were due to an inference based on general knowledge, then we would expect to find it whether sound provided input to the inference or not. Further, the effect can be produced regardless of which sense modality is synchronized with the intersection of the visual stimuli. Tactile stimulation of a finger will work as well (Shimojo and Shams 2001, p.508). The implication is that the result is not due to a learned association of typical sights and sounds that are produced when objects collide.

These results suggest that whatever integration and modulation of features are required, it does not depend on extensive post-perceptual processing. Nonetheless, Vrooman and de Gelder argue that this evidence is consistent with feedback from multi-modal levels in the brain (2004; cf. Shimojo and Shams 2001, and Bushara et al 2002 for similar arguments). The implication is that the mediating role performed in multimodal levels does not derive from judgments about the type of event in which the

features co-occur, or from beliefs, desires, or goals of the perceiver, upon which the direction of attention might depend.

From what, then, does it derive? One possibility is that there is a reciprocal interaction between the unimodal and multimodal areas through which the effects are produced; an interaction that constitutes a "perceptual interpretation" of the signals (Bushara et al 2002, p. 190). In that case, a model is required of the form that interactions take. In order to sketch such a model, I turn now to a comparison of two possible accounts of cross-modal interactions. These emerge from contrasting ways that events in film might be categorized, especially in cases where human actions are portrayed. On one approach, very little internal representation, if any, is required. 'Interactions' among features will consist simply in using body, head, and eye movements to identify and employ cues in the environment according to selection principles in which the cues can come from different sense modalities. Any changes in the relevant features will come largely from changes in the perceiver's point of view or task. On the face of it, the evidence for multimodal neurons does not seem to sit well with the approach. However, it can be argued that the discovery of subpersonal brain systems responsible for mediating sensori-motor and cross-modal interactions need not be taken to imply internal representations, in terms of which the interactions are explained (cf. Pessoa, Thompson, and Noe, in press.) Yet on the other approach, it is precisely an elaboration of an internal representation that is required, although in a limited and nonlinear sense. Arguments for construing cross-modal mediation in this more integrative form derive from the inadequacy of the first approach to account for perceptual behavior and from an analysis of the mechanisms on which mediation depends.

The Categorization of Events in Film

A central issue in regard to event perception has to do with the extent to which the process by which events are segmented or parsed is bottom-up or top-down. A bottom-up account will explain the parsing of events in terms of elemental patterns of movement, taken singly or in small, relatively isolated sets, where the movements are individuated by points of maximal change in space and time. A top-down approach makes the identification of such movements depend on their place in a larger representational structure, which can be more-or-less elaborate, conscious, and centralized: a story, perhaps, which is informed by substantial background knowledge and theory; or, alternatively, a more skeletal narrative schema or script. However, some attempts have recently been made to show that the intentions in terms of which the characters' actions are described, can themselves be recognized on the basis of a few cues, using models that are explicitly heuristic in form. As we have seen, interactions of sights and sounds can affect the properties that basic movements are perceived to have. Thus, in so far as events are segmented with reference to such movements, we can ask, "is scene analysis itself a cross-modal phenomenon?" (Vrooman and de Gelder, 2004, p. 147). If so, the question of whether event perception in film has a bottom-up grounding or is top-down becomes a question of how cross-modal interactions should be understood.

The bottom up approach comes in two varieties, which differ in how they describe the status of cues. On the one hand, cues are said to *specify* the identity of a movement, construed in a way that is derived from James Gibson; i.e. they can be used selectively to get a uniquely correct result. In that sense, the cues are taken to be informationally sufficient for categorization tasks. However, it is possible that there may be more than one such cue, and in that case the door is open to the perceiver to *satisfice*, i.e. rely on the first source of information that will do the job (Cutting 1986). That is where heuristics come into play.

On the other hand, cues may be taken to be only *diagnostic* of action types. They can be elements in motion patterns that are characteristic of more than one action; thus they can never be correlated with properties in a law-like way as to give a determinate result. However, the cues can be ranked in terms of the frequencies with which they are correlated with various actions types (either through experience or natural selection). If the cues are considered individually in an order based on their ranking, and the process stops at some point before all the relevant features are considered, then the cues can be said to be *psychologically* and *behaviorally sufficient* for the task, even if they are informationally inexact. Cues might be used selectively in this way because of time pressure or because the viewer is simply not predisposed or capable of taking all of the relevant information into account. Although it can lead to mistakes in particular cases, this kind of selection heuristic might be typically employed because

perceivers get the identification of the event right often enough to make further cue elaboration and processing a waste of time.

A central example of an application of this approach to the recognition of intentions in film can be found in Blythe, Todd, and Miller (1999), who appeal to a heuristic called *Categorization-by-Elimination* (henceforth, CBE). Blythe et al argue that CBE can provide for the recognition of intentions on the basis of a few fundamental movement types. On this model, the order of cue consideration depends on the fact that some cues are more useful than others, in the sense that they serve to eliminate more action categories, thus narrowing the field of choice. For instance, frequent contact between two moving shapes might rule out courtship, pursuit, evasion, and play, whereas high speed may only rule out play. Although Blythe et al do not specifically consider the effects of sound on perceived motion, it is clear that the account could be extended to simple combinations of sounds and visual features, which could be ranked by their value in event recognition and used by perceivers in accordance with CBE.

What is crucial about this model is that cues are registered in isolation from one another and consulted in a fixed order. Further, a cue cannot be reconsidered once it has been employed and the system has moved on to other cues. Thus no global integration into an action story is possible on this account. From the perspective of representational parsimony, this is a large part of its appeal; however, in that respect, it can be shown to be inadequate on empirical grounds. The evidence suggests that cues are not employed in the rigid and inflexible manner that CBE implies. Rather, the order in which cues are selected can be modified, not only by the perceiver's beliefs, but also by modifications in movement stimuli themselves (Zacks 2004). In the first instance, it appears that the identifications of movements as actions often depends on relating the movements to goals and subgoals that are hierarchically ordered. When movements are seen in that way, different areas of the brain are involved than when identical movements are not seen as intentional actions in this sense, even if the movements are perceived to be biological (Saxe et al 2004). This suggests that cues are treated as more interrelated and in more variable ways that CBE allows. Because the selection of cues for event segmentation can also be modulated by changing the properties of the stimulus, the implication is that, while we may parse events on the basis of a few movement cues, they, too, must be seen as interrelated in some way; a way that can be affected by variations in the representational properties of the video or film.[2] This suggests the need for a more top-down approach, although as I have argued, one in which knowledge effects are somehow constrained.

In seeking a model of event segmentation that involves the appropriate level of integration, a natural place to turn is to theories of apparent motion. A number of experiments on apparent motion have been designed to eliminate the effects of high-level cognition, by requiring a response to a stimulus that allows no time for such effects to occur. Nonetheless, the central feature of the results is that the properties of basic movements of objects, such as their directions and identities, appear to depend on global effects that might be construed as higher order or top down. Although these experiments involve short-range apparent motion, which is different from the more long-range movements that are represented in video and film, there are reasons to think that the two phenomena may involve similar mechanisms (Liu, et al 2003, p. 1772.). [3] Specifically, the perception of apparent motion is often said to involve a process of perceptual completion that depends on the capacity of some neurons to expand their receptive fields (and a corollary inhibition of cells with overlapping fields; cf. Liu, et al, 2003). In various types of perceptual completion, this is described as an interactive process involving the detection of features of different kinds, through which global effects are produced (Ramachandran 1987; Livingstone 2002). Although there is evidence that, in so far as apparent motion depends on filling in, it occurs only in the human motion processing complex (MT+), the evidence also suggests that the perceived continuity of objects through apparent movement derives from an interaction between cells in MT+ and lateral occipital complex (LOC) neurons (Liu et al, 2003 p. 1772). This occurs by virtue of their areas of overlap. Moreover, MT+ has been identified as a possible area for multisensory integration; and various multisensory areas of the brain have been said to integrate inputs from different sense modalities by exploiting overlapping receptive fields (Stein et al 2004 p. 253; Schroeder and Foxe, 2004, p. 297 and 301). Thus, it may be that perceptual completion in MT+ depends on an interactive mechanism involving changes in receptive field properties of neurons and that cross modal effects on apparent motion derive from that sort of mechanism as well. Although in that case, the recognition of basic movement types (and that actions to which they correspond) will not simply derive from early responses to isolated stimuli, it would also not seem to require an account in terms of substantial cognitive processing. It depends instead on mutual constraints imposed by the

related features and on the anatomical and physiological properties through which the constraints are applied. [4]

Two points are important in that regard. First, the mechanism makes sensory integration depend on internal representations, but only of a limited sort. This is due to the coarse coding involved in the 'capture' of one type of sensory feature by another. When cells respond to regions in their extended receptive fields that contain features that they would not typically detect, the responses need not delineate all of the features located there. (And other cells whose response to different features in the region is inhibited will continue to be active, but only weakly so). Thus multimodal integration is heuristic in a general sense. Second, the neuronal enhancement that derives from this integration can be viewed as serving to reinforce the diagnostic status of certain cues. Those cues can be used selectively, not simply by way of a predetermined diagnostic ranking, but because of their dynamic relation to other cues; a relation that need not itself be explicitly represented, being vested an interactive process of the sort I have described. [5]

Representational Genres

These points bear on the nature of pictorial interpretation and the perceptual abilities on which it may depend. In some of the experiments discussed above, the recognition of intentions and the segmenting of events in film have been shown to depend on the selective enhancement of features (Zacks 2004; Vrooman and de Gelder 2004). Cross-modal interactions can be understood in those terms, especially in film. However, the analysis of those interactions points to an account of picture perception that does not emphasize the role of attentional binding of external features, and this distinguishes it from other theories in which the need for internal representation is minimized (cf. e.g. Ballard 1999).

At the same time, the analysis suggests that the mode of interpretation of movies is not fundamentally different from that employed with paintings and photographs (cf. e.g. Livingstone 2002; Grossman et al 2000, 2001). The continuity of cross-modal interactions with interactions among subsystems within a sense modality may have phenomenological consequences; that is, it may be at work in the fact that cross-modal interactions always produce effects in one sense modality or another: Ventriloquism concerns the location of sound, tones synchronized with flashing lights produces visual apparent motion. The principles that govern the experience of cross-modal effects in that regard remain unclear. [6] However, it can be argued that, as long as the effects of cross-modal interactions include responses within primary sensory areas and do not simply terminate with activity in multimodal neurons per se, the response to multimedia presentations should not be construed as a distinctive mode of interpretation. Rather, the response should be classified with reference to the pattern of activity in the unimodal areas that are involved. So construed, movies are motion pictures that include sounds.

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Notes

[1] For reasons I have expressed elsewhere (Rollins 2002), I do not think that pictorial representation can actually be defined in terms of a type of interpretation that applies only to pictures. Being the object of a certain kind of perception-based interpretation is only a necessary condition for being a picture at most. In any case, it will be sufficient for my purposes to show that there is a distinctive perception-based mode of interpretation that is characteristic of pictures (i.e. typically employed in understanding them). The question then is whether there is also a characteristic mode of interpretation of movies, which is sufficiently different from our interpretive response to static pictures as to require a distinct type of account. I will argue that the answer is no.
[2] The area that Zacks et al and Saxe et al find to be implicated in the detection of biological motion, the post superior temporal sulcus (PSTS), has been described by Saxe et al, as well as Grossman et al (199x) as a special purpose mechanism for recognizing biological motion, which may even contain a subregion dedicated to intentional biological motion in particular. Grossman et al argue that PSTS does not provide feedback to earlier levels, in which case it would seem to be a higher level structure that simply receives input from earlier visual processes. However, two points are important in that regard. First, even if PSTS is a dedicated intention detector, Saxe et al argue that the activity in that area specifically does not involve reasoning about the mental states of the agents (because other brain regions known to be active in such reasoning are not involved in detection intentional action). Second, the Saxe et al and Grossman et al studies involve only visual stimuli. It is not clear what other areas might be involved when the perception of biological or intentional motion which depends on cross-modal effects.
[3] Of course the representation of long-range apparent motion in movies depends on the perception of I changes in the positions of objects, the perceived continuity across depends on an apparent motion effect. It should be noted that cross-modal versions of short-range apparent motion can be produced: A single flashing light on the left side of a screen will appear to move behind a piece of tape on the right, if it is coordinated with tones alternating in the relevant locations. The effect lends itself naturally to description in intentional terms: “In ‘hiding’ the missing dot behind the piece of tape stuck to the screen, the visual system is doing more than merely filling in the missing dots between the two points. It ‘assumes’ that the dot ‘hid’ from view[;] …” it attributes “‘hiding’ to the behavior of a dot…” (Anderson and Anderson, 200x, pp. 358-369).
[4] Although it appears that processes in MT+ may be modulated by connections to areas involved in the direction of attention, I take it that the extent to which the function of MT+ should be explained in more bottom up or top down terms is empirically an open question. Cf. Speer, Swallow, and Zacks 2000.
[5] More must be said about the nature of heuristics than space allows here. In earlier debates, a reliance on heuristic principles was taken to be opposed to the use of invariant properties. Cutting mitigates that opposition, to some extent, in allowing for perceptual strategies in which invariants can be selected and combined. The distinction between the use of heuristics and invariants can be blurred in other ways as well. For instance, Hecht (1996) argues that accounts that appeal to invariant properties sometimes explain failures in perceptual judgment in terms of the perceiver’s use of incomplete invariants. That makes the response sound heuristic in form. Nonetheless, I suggest that progress can be made by focusing on particular phenomena and the mechanisms

that underwrite them. Studies with such a focus support the conclusion that film viewers commonly rely on more feature integration than some models allow, but not so much as to make event perception heavily dependent on inference and background knowledge.
[6] Vrooman and de Gelder 2004 suggest a principle of cross-modal control that consistent with this point.

Discussion

Crossmodal interactions and understanding movies
Nicolas Bullot
18 nov. 2005 23:47 UT

In the beginning of the article, you present a set of problems for any naturalist approach to picture understanding -- capable of explaining the relation of perceptual processing with conceptual thinking. According to you, the appeal to visual modules does not work since "(…) the outputs of such modules will always underdetermine conscious, central interpretive processes, which can take highly variable forms." In contrast with the purely an approach based on a visual module, you examine the function of crossmodal interactions for movie perception (crossmodal interactions between vision and audition seem to play an important role in movie perception or/and understanding). You give reasons for maintaining that film understanding (e.g. the recognition of intentions and the segmenting of events in film) may depend sometimes on the crossmodal selective enhancement of perceived features. If true, this suggests that (the epistemic/heuristic strategies of) motion-picture understanding may be routinely based on crossmodal selective enhancement. In this context, I am not sure to that I understand the meaning your conclusion:

"it can be argued that, as long as the effects of cross-modal interactions include responses within primary sensory areas and do not simply terminate with activity in multimodal neurons per se, the response to multimedia presentations should not be construed as a distinctive mode of interpretation. Rather, the response should be classified with reference to the pattern of activity in the unimodal areas that are involved. So construed, movies are motion pictures that include sounds."

Are you excluding the possibility the crossmodal interaction would be associated with a distinctive mode of interpretation (which may make sense: e.g., think about the distinction between silent and speaking movies)? Is this because you conceive of the interpretative process as being amodal or non-perceptual? Is this related to a claim according to which pictures can only be visual? Can one consider movies as evolving sound environments that include pictures or multimodal (i.e. audiovisual) pictures?

> **Pictorial Interpretation and Vision**
> Mark Rollins
> 26 nov. 2005 16:49 UT
>
> My claim was twofold: (1) There is a distinctive form of interpretation, pictorial interpretation, which is perception-based and not heavily knowledge-dependent. (2) The interpretation of film, at a basic level, is pictorial interpretation. Evidence for cross-modal effects in film do not require us to treat the interpretation of it as either (a) always a function of higher-order cognition and background knowledge or (b) perception-based interpretation of a special, multi-media sort; filmic interpretation, as it might be termed.
>
> Regarding (a), the argument is that activity in multimodal areas of the brain can be modeled in a way that does not entail much reliance on background knowledge. Ironically, this requires feedback from those areas to unimodal sensory processes, i.e. interactions of certain kinds. Otherwise, multimodal activity may be viewed as only a stage leading to further higher-order processing in terms of which cross-modal effects are ultimately explained.
>
> However, regarding (b), my claim is not that the interpretation of film at a basic level is pictorial because the relevant processes terminate in visual cortex. For various reasons, I do not think that pictorial interpretation has to be grounded on vision, even if it usually is. I suspect that a further point I make has clouded the fact that this is my view.

That point is that (c) cross-modal effects have a direction: Sights influence the perception of sounds or sounds impact the experience of visual stimuli. One modality controls, to a certain extent, the experience of stimuli in another modality; one sense is the phenomenological locus, as it were, of the influence of the other. It is true, I think, that visual experience is often the phenomenological locus of cross-modal effects in film perception (we see a collision partly because of what we hear); but clearly auditory experience is also affected by what is seen (the ventriloquist effect). Thus the direction of cross-modal effects in film is complex, and it is not the specific direction of the effects that makes film perception pictorial, on my view. Rather, my suggestion was that principles governing the direction of cross-modal effects can be articulated in perceptual terms; it is for that reason that interpretation is pictorial.

To add a bit to the original text: It has been suggested (e.g. by Shimojo and Shams, 2001: 507-508) that the direction of cross-modal effects is at bottom determined by properties of the stimuli. Specifically, the more discontinuous and structured stimulus alters the experience of the more continuous and unstructured one. In the collide-and-pass paradigm, sound affects the visual perception of the objects' movements because the sound is heard at a distinct moment (in that sense is discontinuous and temporally structured), whereas visual motion is continuous. Other evidence suggests that only discontinuous visual stimuli affect the perception of sound. Whether this particular account is correct or not, I take it to illustrate the point that the direction of cross-modal effects can be explained in perceptual terms and thus that pictorial interpretation is the kind of response we make to film.

On the nature and perception of depictions

Thomas Stoffregen (University of Minnesota)

(Date of publication: 28 November 2005)

Abstract: Following a venerable debate, I discuss the nature of depictions. My discussion focuses on dynamic depictions (film), but the arguments are intended to be general. I also discuss the perception of depictions. Here I argue that the perception of depictions is veridical. That is, I claim that people correctly perceive the depiction, as such, and rarely (if ever) believe that they are experiencing "the real thing" (that is, that which is depicted). Finally, I point out that while many depictions are uni-modal (e.g., paintings, silent movies), the act of perceiving depictions always involves stimulation of multiple perceptual systems. The perception of depictions is always multimodal.

On the nature and perception of depictions

This online forum is devoted to the perception of depictions. In this essay, I discuss several issues that I believe are relevant. My discussion is focused on film and on film theory. However, I believe that my arguments apply to all forms of depiction, that is, to visual depictions (e.g., drawings, paintings, sculpture, silent movies, visual virtual environments), to auditory depictions (e.g., radio), and to depictions that are available to multiple perceptual modalities (e.g., cinema, television, and multimodal virtual environments). As these examples indicate, my concept of depiction is broad. I am interested in the nature of depictions (that is, what distinguishes depictions from things that are not depictions). I am also interested in how depictions influence the structure of potential sensory stimulation (that is, the structure that depictions impart to ambient light, sound, and so on, and how these structures differ from structures imparted by non-depictions). Finally, I am interested in the perception of depictions: What do we perceive in the presence of depictions? Is perception of depictions accurate? Can we distinguish depictions of things and events from corresponding real things and events, and if so, how? In my discussion, I take the perspective of the Ecological Approach to Perception and Action (e.g., James Gibson, 1979/1986). In this essay, I combine elements from my own earlier writings on film theory (Stoffregen, 1997), on the nature of simulation (Stoffregen, Bardy, Smart, & Pagulayan, 2003), and on general theories of perception (Stoffregen & Bardy, 2001).

Depictions are of interest to many cognitive and perceptual psychologists. This interest is theoretical (e.g., this online forum) but it is also practical. The practical issue arises because there is a venerable and robust tradition of using depictions as laboratory stimuli in experimental research on perception and cognition. In most cases, the researchers conducting these studies seek to understand perception and/or cognition, in general. That is, there is a long-standing assumption that perception and/or cognition in the presence of depictions is equivalent to (or even identical with) perception and/or cognition in the absence of depictions, that is, when we are confronted only with “real” things. I believe that this assumption is not valid.

The perception of depictions

Are we fooled by film?

Films are widely believed to give rise to “the illusion of reality”. For example, in presenting a novel theory of film Anderson (1996, p. 161) claimed that the perception of films is illusory, or non veridical. He attempted to explain the putative illusion of reality by an hypothetical capacity of the visual system “to process a synthetic array of light as reality,” (p. 113). James Gibson made similar statements, suggesting that the viewer has “an awareness of being in the place and situation depicted”, (1979/1986, p. 295). In these statements both Anderson and Gibson imply that the film is perceived as real (i. e., that viewers believe they are in the presence of physical events, rather than depictions of physical events). Gibson went so far as to assert that “the seated viewer never actually turns his head, of course, but he gets the essential optical information for doing so”, (p. 298), implying that when the camera pans the viewer might perceive his or her head to turn (cf. Hochberg, 1986). At the same time, however, these authors realize that viewers are not fooled. Gibson was aware that “no one is ever wholly deceived”, (J. J. Gibson, 1979/1986, p. 301), while Anderson pointed out the simple fact that “we do not get up and run out of the theater at the appearance of the monster”, (Anderson, 1996, p. 47). Gibson concluded, “the illusion of reality is a myth” (1979/1986, p. 281). These statements appear

to be contradictory. If the synthetic array is processed as reality, and if it can contain information specifying a head turn, then how can it be that viewers differentiate filmed events from physical ones?

The nature of depictions

Films are a subset of depictions: In perceiving a film we perceive a depiction. One of the few psychologists to grapple with the nature of depictions, as such, was James Gibson, who returned to this issue repeatedly over several decades (for a summary statement, see Gibson, 1986/1979). What is a depiction? After struggling with this seemingly simple question for many years, proposing and rejecting a series of candidate definitions, James Gibson (1979/1986, p. 271) defined a depiction as a surface treated to provide "an array of persisting invariants of structure that are nameless and formless". Gibson proposed that in structuring light depictions create a "duality of ... information", which gives rise to "a dual experience", in which the things that are depicted "are not perceived, and yet they are perceived", (1979/1986, p. 283; cf. Gibson, 1971). This derives from his claim that "a picture is both a surface in its own right and a display of information about something else" (1979/1986, p. 283). However, this does not eliminate the contradiction, leading Gibson to argue that the perception of depictions is "a paradox" (1979/1986, p. 282). Similarly, Anderson describes film as creating "two incompatible sets of information" (p. 48).

Gibson attempted to embrace this contradiction by concluding that "a picture always requires two kinds of apprehension that go on at the same time, one direct and one indirect", (1979/1986, p. 283), with the perception of the surface being direct and the perception of the subject of the depiction being indirect. I think that this strategy deals with only part of the problem. An implication of "incompatible", "dual", and "paradox" is that the perception of the surface and of the content of the depiction are mutually exclusive, and/or that the depiction structures light in two ways that are mutually incompatible. In this both Anderson and Gibson may be mistaken. If the optic array is a product of physical law, then its structure cannot bear a paradoxical relation to physical reality, any more than physical law can produce a paradox. Similarly, if films are created in accordance with physical law, then they cannot structure optic arrays in ways that are incompatible or paradoxical. The subject of the depiction may be paradoxical (e.g., the lithographs of M. C. Escher), but the specification cannot be.

I think that the problem arises from Gibson's assertion that "a picture is a surface that always specifies something other than what it is", (1979/1986, p. 273; see also Gibson, 1971, p. 32). The paradox would vanish if depictions were specific to only a single, unitary thing. I believe this to be the case. Rather than simultaneously specifying something that is there (the surface) and something that is not there (the subject of the depiction), the array structure engendered by a depiction specifies only one thing: It specifies a depiction. To perceive a depiction, then, is to have a unitary perception of an image on a surface, or, more properly, a state or event depicted on a surface. Rather than directly perceiving the surface and indirectly perceiving the subject of the depiction, I am suggesting that we have a single, direct percept of the depiction, as such. There is no need to postulate paradoxical specification, or paradoxical experience. There is no need to propose distinct percepts of "that which is depicted" and "that which supports the depiction". To perceive a depiction is not to perceive "an image, and a surface", but "an image on a surface".

My definition of depiction may help to resolve the muddle over whether viewers perceive films (or other depictions) to be real. No matter how compelling the depiction, we never run from the monster. This is easily explained when we understand that in movie theaters people do not have separate percepts of that which is depicted and the surface on which it is depicted. To perceive the movie monster is, fundamentally, to perceive that the monster is in the movie. The depiction is perceived, as such. My position is implicit in Gibson's assertion that "the notion of an image that is literally and actually indistinguishable from the reality is a myth", (1971, p. 33). However, Gibson did not discuss this point in the argument that led him to conclude that the perception of depictions is paradoxical (for example, compare the section entitled "The naive attitude and the perspective attitude"; 1971, p. 32, with the section entitled "But what about the illusion of reality?"; 1971, p. 32-33).

The specification of depiction, as such

I regard it as a fact of fundamental importance that viewers differentiate depictions from "real" things, and from "live" events. In addition to not running from the movie monster, we behave very differently

when we observe a film of a murder than we would if we were present while a murder was being committed. Similarly, we behave differently in the presence of a volcanic eruption than we do when the eruption is depicted in a movie. Surely this is so because we perceive the depictions, as such; that is, we differentiate the meaning of depictions and "real" events. What is the basis for this differentiation?

In an ecological theory if depictions are perceived, as such, then they must be specified, as such. Specification occurs when there is a unique, 1:1 relation between some aspect of reality and a pattern of ambient energy (J. J. Gibson, 1979/1986; Stoffregen & Bardy, 2001). When this unique relation exists between reality and ambient energy, then the patterns in ambient energy are sufficient for accurate knowledge, that is, a perceiver who is sensitive to the patterns in ambient energy can, on that basis alone, perceive the world accurately. Perception of this kind is said to be direct, as opposed to indirect perception, which is required if patterns in ambient energy bear a non-unique or ambiguous relation to reality. When stimulation is ambiguous, accurate knowledge of the world requires intervening mental processes that remove ambiguity from the stimulus. The Ecological Approach to Perception and Action asserts that specification exists, and that perception generally is direct (Gibson; Stoffregen & Bardy).

How is depiction specified? The answer is that depictions (e.g. a film of a baseball game) and "real" things (e.g., a baseball game) give rise to different structures in ambient arrays (for optics, a long and diverse list of differences is given by Hochberg, 1986). A depiction, no matter how technologically sophisticated, cannot have perfect fidelity to that which is depicted (J. J. Gibson, 1971, p. 33; 1979/1986, p. 279-280). One reason for this is that the depiction is finite (in space, time, and/or resolution), while the structure in the undepicted environment is unlimited. Depictions and depicted differ also with respect to changes in the point of observation. Changes in the position or motion of the point of observation give rise to variants and invariants of array structure that differ for depictions and depicted. For example, movements of an observer's head when viewing a baseball game will give rise to different optical changes than when viewing a film of a baseball game. Another difference is that viewing of films (and pictures) is partially open-loop (not responsive to perceptual information), whereas viewing of the depicted events (if we are really in front of them) is wholly closed-loop, or controlled with reference to perceptual information (cf. Smets, 1995, p. 192). These differences probably underlay much of the ease with which depictions are distinguished from depicted. The difference between open-loop and closed-loop viewing is so salient that even suckling infants show acute sensitivity to it. For example, Kalnins & Bruner (1973) presented infants aged 5-12 weeks with visual displays whose focus was contingent on the infants' sucking rate. When sucking improved focus, it was rapid and prolonged. When sucking reduced focus, it was suppressed. Ironically, the stimuli used by Kalnins & Bruner were films; their findings should, therefore, be of direct relevance to Anderson's (1996) thesis.

If depictions are specified, as such, and perceived, as such, then the issue is not how, or even whether, film is perceived "as reality". Film is not perceived as reality, just as a radio drama, a stage play, a painting, or the performance of a mime are not perceived as reality. All of these depictions are perceived for what they are. There is no illusion of reality because there is no illusion, at all. This does not mean that viewers (or listeners) necessarily perceive the depicted events to be false in the sense of being fundamentally fictional. It is possible, and common, to perceive the depicted events to be real. This can occur even when they are fictional, as with Orson Wells' notorious radio broadcast of War of the worlds. But while many listeners believed the events in Wells' production to be real, no one believed that the events were taking place in their radio sets, or in their living rooms. In this sense the depictions were perceived, as such.

The analysis in this section and the previous one requires us to develop a new explanation for the compellingness of film. Films are perceived as depictions, rather than as "the real thing". A film may "seem real", but this does not mean that we believe that it is real (that there is no depiction). Given this, how is it that we find films to be so absorbing? It may be that, rather than being drawn into the *film* (the depiction, as such), viewers are drawn into the *story*, that is, into the events that took place in front of the camera (this is what happened with Wells' *War of the worlds*). The real question, then, would be how it is that film (or radio, or a stage play), seems to be able to draw us into the story (or the *diegesis*, to use the formal term). How is it that we can become so involved in things that we clearly perceive to be depictions? This issue of involvement (as opposed to perception) leads us back to relations between film and other media, such as literature. Readers know that involvement with a good book can be just as intense as with a film. I am not suggesting that the experience of books and films

is identical; obviously, it is not. What I am suggesting is that an understanding of film, *per se*, will require us to understand relations between film and other story-telling media (some early experiments on this issue were conducted by J. J. Gibson and R. M. Gagne; Reed & Jones, 1982, p. 17).

Multimodal perception of uni-modal depictions

In discussing how filmed and "live" events structure ambient arrays Anderson (1996), Gibson (1979/1986), and Hochberg (1986) all limit their analysis to optics. Yet observation of film (and of depictions, in general) structures multiple forms of stimulus energy (Stoffregen & Bardy, 2001; Stoffregen, Bardy, Smart, & Pagulayan, 2003). Exploratory movements (e.g., of the eyes and head) cause changes in structure of gravitoinertial force, strain in muscles and joints, and pressures on the skin, as well as changes in the optic array. Consider a film that depicts rotation of the point of observation (e.g., a pan, which bears some resemblance to a head turn). Physical turning of the head influences the optical structure that is sampled, but it also influences the stimulation of the vestibular and somatosensory systems. Thus, a presentation that is limited to the optical consequences of a head turn (i.e., a film) does not specify that the head has turned. To put it more precisely, a real head turn leads to changes in the orientation or position of the head relative to the visible environment, relative to the audible environment, relative to the environment of forces (e.g., gravitoinertial force), and relative to the rest of the body. The head turn produces changes in patterns of optical, acoustic, gravitoinertial, and mechanical stimulation (that is, changes that can be seen, heard, and felt). By contrast, a pan (the filmic approximation of a head turn) leads to changes in the orientation and position of the head relative to portions of the visible environment, and relative to portions of the audible environment, but not relative to the environment of forces, or relative to the rest of the body. Thus, considered jointly, multisensory stimulation provides abundant perceptual information that makes it possible, in principle, for observers to differentiate head turns from camera pans. Countless laboratory experiments have demonstrated that people are acutely sensitive to variations in their orientation relative to the gravitoinertial force environment (e.g., leaning, tilting, or rotating), and that we are acutely sensitive to changes in the orientation of parts of the body relative to one another (Clark & Horch, 1986). Thus, we have good reason to believe that people routinely distinguish camera pans from head turns. This is likely to be part of the reason that viewers know that they are looking at a film, and not at the corresponding reality.

Film can be a uni-sensory art form (think of silent movies), but the perception of film is never confined to a single perceptual system. Viewing of film (or of paintings, or listening to radio, etc.) always involves stimulation of multiple perceptual systems. This is because perception is an act (it is something that we do, rather than something that happens to us). The act of perception depends upon movement of the perceiver. In viewing visual imagery (paintings, or film) perception entails movements of the eyes in the head, movement of the head on the neck, movement of the trunk relative to the legs and (for standing viewers) movement of the entire body.

Conclusion

The issue of how film is perceived has implications that extend beyond the entertainment industry. Issues of film perception are relevant to recent attempts to understand teleoperation (Smets, 1995), and to the dynamic depiction of industrial processes (Vicente & Rasmussen, 1992). Film perception may also have considerable relevance for scientific psychology (Hochberg, 1986). This is because much of contemporary psychological research depends on a methodology in which humans are asked to perceive depictions. The favored screen is that of a computer monitor (E. J. Gibson, 1991, p. 607; Reed, 1996), but the issues of depiction, and the perception of depictions, are much the same for computer displays as for film. Such studies have an uncertain relation to the perception of physical events. Contemporary experimental psychology may tell us more about how we perceive depictions than about how we perceive the world in which we evolved. It is important that we understand what the perception of depictions *is*, but it may be important in a much broader sense that we understand what the perception of depictions is *not* (cf. Ittelson, 1996; Reed & Jones, 1982, p. 16-17). This can be done only if we conduct research that does not depend on depictions. It would also be extremely useful to conduct direct comparisons between perception and action relative to depictions and relative to physical surfaces, objects, and events. Consistent with one aspect of James Gibson's analysis (1971, p. 33), I have concluded that films can be distinguished from physical events, and that this is so because the difference is specified. This may have important implications for research that utilizes

depictions. It may also suggest logical (as opposed to technological) limitations on the fidelity of simulation (Stoffregen et al., 2003; cf. Edgar & Bex, 1995).

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Art and Neuroscience

John Hyman (Oxford University)
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Abstract: Some prominent neuroscientists have recently made ambitious claims about their work on the visual arts, which they regard as initiating a new scientific enterprise, called 'neuro-aesthetics'. In particular, V.S. Ramachandran says that he has discovered 'the key to understanding what art really is'; and Semir Zeki claims to have laid the foundations for understanding 'the biological basis of aesthetic experience'. In this article I discuss the prospects for this new scientific field, focusing on these two authors.

Art and Neuroscience

I want to discuss a new area of scientific research called neuro-aesthetics, which is the study of art by neuroscientists. The most prominent champions of neuro-aesthetics are V.S. Ramachandran and Semir Zeki. They have both made ambitious claims about their work. Ramachandran says boldly that he has discovered 'the key to understanding what art really is', and that his theory of art can be tested by brain imaging experiments, although he is vague about the experimental design.[1] And Zeki, who originally coined the term 'neuro-aesthetics', claims to have laid the foundations for understanding 'the biological basis of aesthetic experience'. [2]

If these claims are true, we are at the dawn of a new age in the study of art. Up to now, most of the people studying art have been historians, some of whom can read Latin, but hardly any of whom have mastered even the rudiments of brain science. And aesthetics has been in the hands of philosophers, who still disagree among themselves about ideas that were stated in the fourth century BC. Neuro-aesthetics is different. As Ramachandran says:

"These ideas have the advantage that, unlike the vague notions of philosophers and art historians, they can be tested experimentally."

So, is neuro-aesthetics the next big thing? I want to assess its prospects, starting with Ramachandran.

As I have said, Ramachandran claims to have discovered 'the key to understanding what art really is'. He also calls this key '[a] universal rule or "deep structure", underlying all artistic experience' and 'a common denominator underlying all types of art'. [4] He writes as follows:

"The purpose of art, surely, is not merely to depict or represent reality – for that can be accomplished very easily with a camera – but to enhance, transcend, or indeed even to *distort* reality. … What the artist tries to do (either consciously or unconsciously) is to not only capture the essence of something but also to amplify it in order to more powerfully activate the same neural mechanisms that would be activated by the original object."[5]

By 'the original object' Ramachandran means the object represented by an artist: for example, a man or a woman, the interior of a room, a landscape, and so on. His hypothesis is that the works of art we enjoy activate the neural mechanisms that are normally activated when we see the kinds of objects which they represent, but they activate these mechanisms more powerfully.

But why should a distortion of reality have this effect? Ramachandran's answer, which he describes as 'the key to understanding what art really is', is that this is an example of a psychological effect called 'peak shift'. He writes as follows:

"If a rat is taught to discriminate a square from a rectangle (of say, 3:2 aspect ratio) and rewarded for the rectangle, it will soon learn to respond more frequently to the rectangle. Paradoxically, however, the rat's response to a rectangle that is even longer and skinnier (say, of aspect ratio 4:1) is even greater than it was to the original prototype on which it was trained … this principle holds the key for understanding the evocativeness of much of visual art."[6]

Ramachandran's favourite example of peak shift in art is the way in which the female figure was represented by classical Indian sculptors. Figure 2 shows an example from the eighth century, a sculpture of the goddess Kaumari. This kind of sculpture, Ramachandran says, is essentially 'a caricature of the female form'. And he adds this:

Figure 2: *The Goddess Kaumari*, Madhya Pradesh, 8th century A.D. Formerly in the collection of Nasli M. Heeramaneck.

"There may be neurons in the brain that represent sensuous round feminine form as opposed to angular masculine form and the artist has chosen to amplify the 'very essence' of being feminine by moving the image even further along the male/female spectrum. The result of these amplifications is a 'super stimulus' in the domain of male/female differences. "[7]

So Ramachandran proposes a generalization about art and then postulates a mechanism to explain the generalization. The generalization is that 'the purpose of art … [is] to enhance, transcend, or indeed even to *distort* reality. … not only capture the essence of something but also to amplify it'. More pithily: 'all art is caricature'.[8] And the mechanism which explains the biological function of art is peak

shift. In this way, Ramachandran explains a profound and pervasive part of human life in terms of a simple physiological mechanism, which can be demonstrated in the laboratory with a rat, square, a rectangle and some cheese.

This is quite enough to damn the theory, in some people's eyes. It is brazenly reductionist, and that, some people think, is a bad thing. This is of course a well-established view of modern science. For example, it is expressed in following lines by William Blake:

"The atoms of Democritus /And Newton's particles of light /Are sands upon the Red Sea Shore /Where Israel's tents do shine so bright."

For my own part, I love Blake's poetry but I do not accept his anti-scientific world-view. I do not believe that modern science drains enchantment from the world. In my view, explaining complex phenomena in terms of simple mechanisms, or explaining a variety of phenomena in terms of a single mechanism, is a good thing. Furthermore, Blake's verse reminds us that we cannot accept reductionism in science that is more than a century old, and reject it in more recent scientific work. This is not an intellectually defensible position. Anyone who uses the word 'reductionist' as a term of abuse should ask themselves whether Darwin's theory of natural selection and Newton's theory of universal gravitation are reductionist theories, and whether we should reject them for this reason, if they are.

So I do not believe that Ramachandran's theory of art should be dismissed on the grounds that it is reductionist. If the enjoyment of art really can be explained by peak shift, discovering this is a stunning intellectual achievement, a formidably impressive piece of science. Unfortunately, however, Ramachandran's theory has three fatal weaknesses. First, Ramachandran seems to have misunderstood the peak shift effect. Second, the theory is not really about art at all. It is really about why men are attracted to women with big breasts. And third, the theory is based on an extremely limited knowledge of art. I shall comment on these points in turn.

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I begin with the peak shift effect. One kind of psychology experiment which was popular about fifty years ago involved training a bird to peck when it saw a light with a certain colour or when it heard a sound with a certain pitch. The bird was rewarded each time it responded to this particular stimulus by pecking at a target, and then once it had learned to do this, it was tested with a range of different stimuli including the training stimulus. The solid line in figure 3 represents an experiment where pigeons were trained with a 550 nanometre light, and then tested with different lights, some with higher wavelengths and some with lower wavelenghts. The training stimulus is called S+, and as the figure shows, the bird's response decreased more or less uniformly as the stimulus became less similar to S+.

Now in the experiments represented in figure 3 by broken lines, pigeons were rewarded for responding to S+ but they were also shown another stimulus in the training period, which is called S–, which they were not rewarded for responding to. When the pigeons were tested after this kind of training, they responded more vigorously to a stimulus that is different from S+, in the direction away from the S–, than they did to S+ itself. So the peak of the distribution was shifted to about 540 nanometres. This new peak is sometimes called S++. Hence the term 'peak shift'.

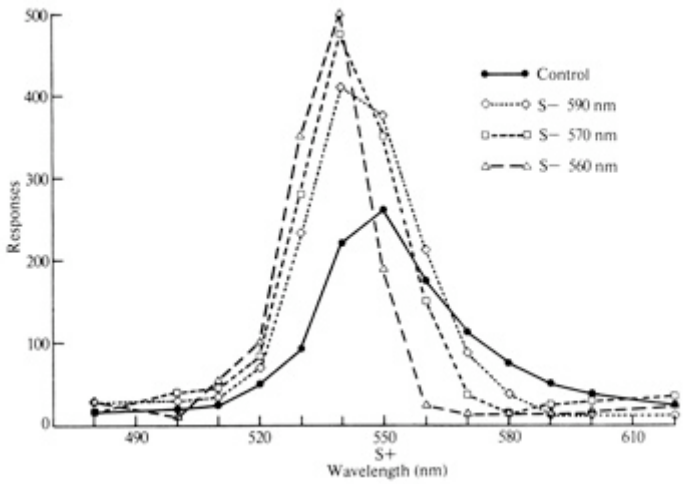

Figure 3: The peak-shift effect.

Now we can see that in this experiment S+ and S– are very similar to each other. S+ is a 550 nm light, which is bright yellow. And S– varies from 560 nm, which is yellowy-orange, to 590 nm, which is orangey-yellow. In fact peak shift only occurs when S+ and S– are very similar, and the commonest theory of peak shift explains why. The theory, which is represented by figure 4, is that peak shift is the result of an interaction between an excitatory gradient around S+ and an inhibitory gradient around S–. What is thought to happen is that when the animal is trained to respond to S+, it also acquires a tendency to respond to stimuli that resemble S+, both to the left and to the right. But if it is also trained not to respond to S–, and S– is similar to S+, this part of its training also inhibits its tendency to respond to S+. So the net effect is that the animal responds more vigorously to a stimulus that resembles S– a bit less than S+ does.

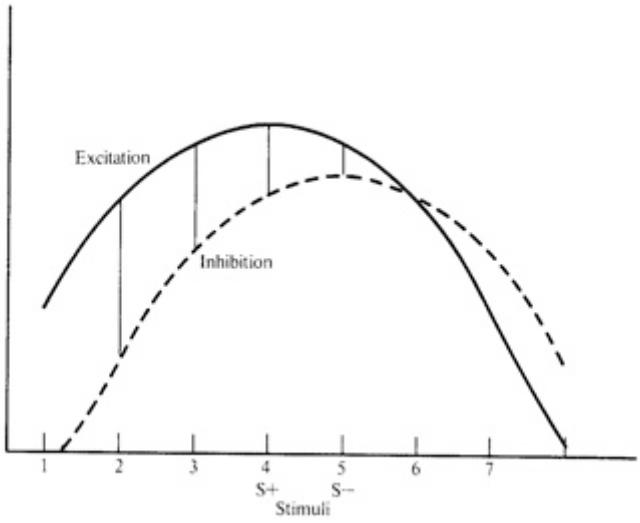

Figure 4: An interaction between an excitatory gradient around S+ and an inhibitory gradient around S–.

The lessons of figure 4 are, first, that S– has to be very close to S+ in the subject's quality space to produce the peak shift effect; and, second, that if the effect does occur, the new peak stimulus, S++, will be even closer to S+ than S+ is to S–. This means that if male subjects were predisposed to respond positively to a stereotypical female body at reproductive age – in other words, if that was the S+ body shape – the peak shift towards wider hips and larger breasts would only occur if an inhibitory gradient was created around a female body with slightly narrower hips and smaller breasts than average, and the predicted effect would be that the subject's response would peak at a female body with very slightly wider hips and very slightly larger breasts than average.

It follows that Ramachandran's explanation of the beauty of the Indian sculpture does not work. In the first place, there is no evidence that the spectators who find it beautiful have been trained in this way. And second, the body shape of the goddess deviates much too far from the norm. Peak shift is the wrong mechanism to explain how a '"super stimulus" in the domain of male/female differences' affects the male brain.

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That is the first reason for rejecting Ramachandran's theory of art. The second is that the theory is not really about art at all. It is really a theory about why men are attracted to women with big breasts.

Remember: the theory is meant to be giving us 'the key to understanding what art really is'. But the fact that the Indian sculpture is a work of art is completely irrelevant to this theory. It could just as well be a theory about Pamela Anderson. The theory would be that Pamela Anderson has amplified the 'very essence' of being feminine – in other words, she has had her breasts enlarged – and the result is a 'super stimulus' in the domain of male/female differences. And of course this is more or less true, although it cannot be described as a cutting-edge piece of science.

The point I want to underline is that Ramachandran's theory of art – we can call it the Baywatch Theory of Art – doesn't distinguish between a work of art and the kind of object that it represents. For example, if it doesn't distinguish between a sculpture that represents a woman with big breasts and a woman with big breasts. And it follows that the theory cannot be telling us what 'the key to understanding what art really is'.

This is something every undergraduate who studies aesthetics learns in the first couple of weeks. In Plato's Republic, Socrates says that everyone can be an artist:

"Don't you see that you yourself could make all these things in a way? … Take a mirror and carry it about everywhere. You will quickly make the sun and all the things in the sky, and quickly the earth and yourself and the other animals and artefacts and plants and all the objects of which we just now spoke." (Republic, 596d)

We can't be sure how seriously Plato meant us to take the comparison between painting and mirroring. But every student learns how to criticize it. Every student learns that understanding 'what art really is' means understanding first and foremost that it is art. Ramachandran seems to have grasped half of this lesson. He seems to have grasped that a work of art isn't a true mirror image of the world. Remember, he says that the purpose of art is to enhance and to distort reality. But if a work of art isn't a true mirror image of the world, it isn't a silicone-enhanced mirror image of the world either. This is the part of the lesson he seems to have missed.

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That is the second reason for rejecting Ramachandran's theory of art. The third is that it is based on a very limited knowledge of art. There are really two points here. First, as we saw earlier, Ramachandran begins with following observation:

"The purpose of art, surely, is not merely to depict or represent reality – for that can be accomplished very easily with a camera – but to enhance, transcend, or indeed even to *distort* reality."[9]

When E.H. Gombrich was asked to comment on Ramachandran's theory of art, he made the following remark:

"To the historian of art, it is evident that the authors' notion of 'art' is of very recent date, and not shared by everybody … They do not explain how one could photograph Paradise or Hell, the Creation of the World, the Passion of Christ, or the escapades of the ancient gods – all subjects that can be found represented in our museums." [10]

I cannot improve on this remark. I would only add that photography itself is one of the visual arts, and has become increasingly important during the last hundred years. So even the kind of representation of reality that *is* accomplished with a camera cannot be excluded from the domain of art.

The second point is that even if we limit ourselves to erotic images made by male artists, and presumably in conformity with male taste, it is obvious that Ramachandran's idea about the distortion of reality, the idea that all art is caricature, is quite unconvincing. Here are a few examples, which were made in very different societies and with different techniques.

The first is a small red-figure jug made in Athens in about 430 BC, which is an unusually touching image of a boy and a girl making love by the standards of Greek art (fig. 6). The boy is leaning back in his chair, his arms at his sides and his hands gripping the seat, his mantle pushed down around his knees. A young girl, naked except for a wide band around her hair, is about to straddle his uncovered lap. Their foreheads touch and they gaze into one another's eyes with a tenderness which is rare in this period. (I don't mean that tenderness between lovers was rare. I mean that it was rarely represented in art.)

Figure 6: Attic red-figure oinochoe, ca. 430 BC. Attributed to the Shuvalov painter. Berlin, Antikenmuseen.

My second example is a woodblock print by Utamaro made in the 1780s (fig. 7). Several things contribute to the subtle eroticism of this image: the intense concentration of the couple, which is expressed in their hands and the man's eye, just visible below his lover's hair; the confusing tangle of limbs which is partly hidden by the man's delicate silk kimono; and the powerful contrast between the fabrics, with their intense colours and lively designs, and the graceful forms of the woman's bottom and neck, the clear white of her skin divided by two similar curves.

Figure 7: Utamaro, Lovers, from The Poem of the Pillow, 1788. Woodblock print.

My final example is one of Rembrandt's last etchings, *Jupiter and Antiope*, which he made in 1659 (fig. 8). The composition is based on an etching by Annibale Carracci which is dated to 1592 (fig. 9). But the figure of Amor, the curtain in the foreground and the landscape have all been omitted, to concentrate on the two main figures, and Antiope has been given a more natural pose. Her arms are thrown back behind her head, and she is lost in a deep sleep.

Figure 8: Rembrandt van Rijn, *Jupiter and Antiope*, 1659. Etching, drypoint and burin.

Figure 9: Annibale Carracci, *Jupiter and Antiope*, 1592. Etching.

None of these images confirms Ramachandran's generalization about art, and of course they stand here for many hundreds of others.

I said earlier that Ramachandran has missed the basic point that understanding 'what art really is' means understanding that it is art. In other words, it means understanding that works of art are objects made with specific tools, materials and techniques. A comparison between these two etchings will help to bring out the significance of this point.

When we look at Annibale's etching, we can see that although he used the tools of an etcher he worked in the style of an engraver. So when he wanted to depict a shadow, he employed the regular cross-hatching of the engraver. The result is a competent print. But there is nothing personal about the technique: it is merely a useful means to an end.

By contrast, Rembrandt's use of drypoint and burin over an initial layer of etching gives his print an extraordinary depth and subtlety of light and shadow. The burin is used to provide an intermediate tone of shading on Antiope's stomach and thighs. And then various details on her arms and head are touched in with drypoint, as is the thick blanket of tone behind her, which sets off the bright upper part of her body. The mixture of these techniques yields a richness and variety of tone, and thereby a subtle atmosphere and register of feeling, far beyond anything Annibale's straightforward method could provide.

The lesson of this example is twofold. First, the comparison brings home how deeply involved Rembrandt was in printmaking. A skilful etcher could follow Annibale's design, but only Rembrandt himself could execute his plate, because the technique was so important to the final result. Second, it illustrates the fundamental point that works of art are made with specific tools, materials and techniques. Understanding 'what art really is' has to involve understanding how the ability that works of art have to express meaning, and to communicate thoughts and feelings and perceptions, depends on these tools, materials and techniques.

Ramachandran's theory of art therefore fails three times over. It fails because he has missed this fundamental point about what art is; it fails because his generalization about what works of art represent is not borne out by the facts; and it fails because even if the generalization were true, the peak shift mechanism would not explain why.

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I shall turn now to Semir Zeki, and in particular to the two key ideas in his book *Inner Visions*, the book in which he attempts to lay the foundations for 'an understanding of the biological basis of aesthetic experience'. One of these ideas is about the visual arts in particular and the other is about the arts in general.

The first idea is expounded in a large part of Zeki's book. But it is expressed in the most striking way in his remark that 'artists are in some sense neurologists, studying the brain with techniques that are unique to them'. [12] Zeki happily concedes that this is a surprising thing to say. But although the formulation is surprising, the idea has been well established since the last quarter of the nineteenth century. I shall explain what is original about Zeki's version of it shortly. But first I shall quote a passage from its original source, which is a lecture given by Helmholtz in 1871:

"We must look upon artists as persons whose observation of sensuous impressions is particularly vivid and accurate, and whose memory for these images is particularly true. That which long tradition has handed down to the men most gifted in this respect, and that which they have found by innumerable experiments in the most varied directions, as regards means and methods of representation, forms a series of important and significant facts, which the physiologist, who has here to learn from the artist, cannot afford to neglect. The study of works of art will throw great light on the question as to which elements and relations of our visual impressions are most predominant in determining our conception of what is seen, and what others are of less importance. As far as lies within his power, the artist will seek to foster the former at the cost of the latter." [13]

In this passage, Helmholtz combines the idea that artists test and explore the visual system with a theory of vision whose broad outlines he inherited from Locke and Kant. The theory is that visual perceptions occur when the unconscious mind interprets 'sensuous impressions'. Sensuous impressions are raw patterns of colour, without any intrinsic meaning. Artists, he claims, are particularly good at observing their sensuous impressions, and at figuring out which patterns trigger which interpretations.

Most visual scientists have abandoned Helmholtz's theory of vision. They no longer talk about sensuous impressions, or about the unconscious mind interpreting sensuous impressions. Instead, it is generally held that different parts of the brain are simultaneously performing various highly specialized tasks, reacting to form, or to motion, or to colour; and that somehow or other the results of these processes are combined to form a unified visual perception, although nobody is sure yet how this synthesis occurs.

But abandoning Helmholtz's theory of vision does not entail abandoning the idea that artists test and explore the visual system. On the contrary, it allows for a more detailed and discriminating version of the same idea, since different kinds of art can now be shown to correspond to different parts of the visual system. For example, kinetic art specializes in V5, the part of the visual cortex that reacts to motion. Fauve art specializes in V4, which reacts to colours. A painting by Mondrian will excite V1, which reacts to horizontal and vertical lines. And so on. The message is that in some cases different kinds of art excite different groups of cells in the brain. This is the principal idea that Zeki defends in his book.

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I want to make two comments about this idea. First, it is undeniable that we could not appreciate a painting by Mondrian if the cells in our brains which are excited by vertical and horizontal lines were not functioning properly. But this does not explain why the painting is pleasing or interesting to look at, or what it means. In fact, it reveals nothing whatever specifically about art. Because it is equally true that I could not see the text on a page or the railing in a fence if the cells in my brain which are excited by vertical and horizontal lines were not functioning properly.

The second comment I want to make is this. It may be an amusing paradox to describe painters as neurologists, studying the brain in their own special way. But the real substance of this claim is, first, that paintings are designed to have specific kinds of psychological effect on viewers; and second, that specific kinds of psychological effect are produced by specific kinds of activity in the nervous system. I do not want to dispute either of these ideas. They have been commonplace for more than a hundred

years, and they are both surely true. But if we can think of paintings in this way, the same is true of many other things. For example, hamburgers and ice-cream are designed to produce a specific kinds of psychological effect on consumers: the experience of tasting hamburgers in one case and the experience of tasting ice cream in the other. And these specific psychological effects are produced by specific kinds of activity in the nervous system.

So there are two reasons for doubting whether the claim that artists are in some sense neurologists is a useful one to make. First, it does not say anything distinctive about artists. It tells us nothing about Picasso and Cezanne that doesn't apply equally to Haagen Dazs and MacDonalds. And second, it skates over many interesting differences between artists, for example, the difference between painters who are interested in geometrical optics, such as Piero della Francesca, and painters who are interested in the psychology of perception, such as Seurat and Bridget Riley. Or the difference between painters who are interested in the character of visual experience, as Monet and Bonnard seem to have been, at least in theory; and painters who regard themselves as being more like naturalists, and are therefore uninterested in visual experience, but very interested in the visible word—for example, Constable and Turner.

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The second idea I want to comment on is the boldest and most speculative in Zeki's book. 'Aesthetic theories', Zeki maintains, 'will only become intelligible and profound once based on the workings of the brain.' [14] Encouraged by this thought, he proposes what he calls a 'neurobiological definition of art'. [15] He writes as follows:

"Great art can thus be defined, in neurological terms, as that which comes closest to showing as many facets of the reality, rather than the appearance, as possible … The inestimable quality [of great art] is the opportunity that the brain is offered to give several interpretations, all of them valid." [16]

Zeki sometimes calls this inestimable quality of great art 'ambiguity'. This may not be the right word for it. But I shall not quibble about terminology. The important question is whether the opportunity it affords us to give several valid interpretations is what we value in great art. There is also the interesting and contentious question of whether it is brains that do the interpreting, or whole animals. But I shall not address this question here.

So, is Zeki's claim about the value of great art plausible or not? I am not sure that the category of great art is a useful one, in the history of art or in philosophy or science. But I think we all know roughly what properties make art repay serious and sustained attention. We think about these properties when we use the concepts to which criticism constantly returns—among them the concepts of imagination, truth, beauty, form and emotion. But we face two difficulties when we theorize about art. First, none of these concepts is pellucid. They all need careful study. And second, their significance lies in the very particular uses that we put them to, in criticism, so merely identifying them by name does not get us very far.

Take the concept of imagination.[17] Every serious work of art, every work of art that deserves close critical attention, is imaginative, at least in some respects. But the idea of imaginativeness works by contrast. To describe a work as imaginative is to say what it is not. It to say both that it is not banal, conventional or academic, and that it is not gimmicky or fanciful or kitsch. Of course it is sometimes hard to decide whether a work of art, or part of one, falls on one or the other side of imaginativeness. For example, consider the famous opening lines of T.S. Eliot's poem, *The Love Song of J. Alfred Prufrock*:

"Let us go then you and I, /When the evening is spread out against the sky, /Like a patient etherized upon a table."

Is the simile 'like a patient etherized upon a table' imaginative, or is it meretricious? In many cases, opinions vary and it is hard to know. But part of the business of criticism is make these hard decisions, and to back them with convincing reasons.

So we cannot hope to assess the idea that imaginativeness is a central concept in the theory of art without considering examples. And the same is true of the idea that the value of great art – or art that repays serious attention – lies in 'the opportunity that the brain is offered to give several interpretations, all of them valid.' Zeki acknowledges this, and in fact he offers many examples of the kind of art he thinks of as having several valid interpretations. He mentions, for instance, Vermeer's painting *Woman in Blue Reading a Letter* and comments that there is no way of telling what the letter she is reading is about (fig. 10). True. As it happens, there is an earlier painting by Vermeer entitled *A Girl Reading a Letter by an Open Window* (fig. 11), and in this case an x-ray revealed a painting of Cupid hung on the wall behind the woman, the very same one we see in *A Lady Standing at a Virginal* (fig. 12)). This was a clue that the girl is reading a letter from a suitor or a lover. But Vermeer decided in the end to hide the clue, and I think we can see why.

Figure 10: Jan Vermeer, *Woman in Blue Reading a Letter*, ca. 1662-65. Oil on Canvas. Rijksmuseum, Amsterdam.

Figure 11: Jan Vermeer, *A Girl Reading a Letter by an Open Window*, ca. 1657-59. Oil on Canvas. Staatliche Kunstsammlungen, Gemäldegalerie, Dresden.

Figure 12: Jan Vermeer, *A Lady Standing at a Virginal*, ca. 1670-73. Oil on Canvas. National Gallery, London.

But the difficult question is what kinds of indefiniteness or multiplicity can contribute to the value of a work of art. Multiplicity is not always a good thing, and more multiplicity is not always better than less multiplicity. For example, consider one of Chardin's still lifes of a hare, a partridge or a duck (fig. 13). I doubt whether it would have been a greater painting if Chardin had managed to paint a duck-rabbit hanging on the wall instead, although this would have introduced an ambiguity which is not there now (fig. 14).

Figure 13: Jean-Siméon Chardin, *Duck Hanging by One Leg, Pâté, Bowl and Jar of Olives*, 1764. Museum of Fine Arts, Springfield Massachusetts.

Figure 14: ***Duck-Rabbit Hanging by One Leg, Pâté, Bowl and Jar of Olives.*** **After Chardin.**

Perhaps what we want is *imaginative* multiplicity—multiplicity that it is not banal, conventional or academic, and that is not gimmicky or kitsch. But if that is right, the idea that great art is art that has many valid interpretations boils down the to the idea that great art is art that is imaginative, in this specific way.

My last comment on this idea is that this kind of imaginativeness seems to me to be one property among many which sometimes contributes to the value or interest of a work of art. It certainly is not a definition of great art in neurological terms. And there are many other reasons for admiring art. Here, for variety, is a literary example. It is a well-known fact that the most erotic line in English poetry – which is in Donne's elegy *To His Mistress Going to Bed* – consists entirely of prepositions:

License my roving hands and let them goBefore, behind, above, between, below.

It is true that this line leaves room for different interpretations, or at least for different ideas about what exactly the author has in mind. For example, there are several things that Donne could want his roving hands to get between, although one doesn't imagine that he has her teeth in mind. But I doubt

whether this explains the line's extraordinary effect. Surely it is the economy of means and the combination of imagination and technical control that enables Donne to pack such a erotic charge into a string of prepositions. In other words, it is sex and syntax, not ambiguity. This example reminds us, again, that works of art are produced with specific materials, and it is often the relationship – in some cases the suprising relationship – between these materials and the thoughts and feelings they communicate that matters most. But please do not think this is my universal theory of art. It is just one kind of example. And it is meant to show that we should be pluralists about artistic value. It is a mistake to think that ambiguity or caricature or anything else of this kind defines all art, or all 'great' art. Philosophers and historians have for a long time considered it a truism that there is no single source of value or single overarching motive in art. Why should there be such a thing in art, any more than in human life generally?

Zeki maintains that 'aesthetic theories will only become intelligible and profound once based on the workings of the brain.' So, does neuroscience at last hold out the promise of an intelligible and profound aesthetic theory, or one that will provide 'the key to understanding what art really is'? And will we soon be able to discard the unintelligible and shallow aesthetic theories proposed by Plato, Aristotle, Hume and Kant?

Extrapolating from the present, the answer must be no. Neuroscience can explain some features of some paintings. For example, some of the colour effects of impressionist paintings are explained by lateral inhibition. But the idea that there is a neurological theory of art in prospect is utterly implausible, in my view. The eye-catching paradoxes Ramachandran and Zeki propose – that all art is caricature, that artists are neurologists – are in fact very weak ideas. And in Ramachandran's case, this weak idea is dressed up as a piece of science by misleadingly associating it with the peak shift effect. This, in particular, gets a black mark in my book.

I shall add one final comment. The main defect in the work I have discussed is that both authors propose extravagant generalizations about art – all art is caricature; all great art is ambiguous – and then discuss a small number of examples, which are chosen to *illustrate* the generalization they favour and not to *test* it. Would Zeki or Ramachandran tolerate this procedure in their own subject? I expect they'd laugh at it. How easily we shrug off our academic training when we take the brave step outside the furrows we were taught to plough!

Discussion

Explaining everything
Seamus Sweeney
19 nov. 2000 23:30 UT

Does anyone else find that neuroscience is beginning to take on one of the qualities of Freudianism and Marxism that made those -isms so unscientific - when they both aspire to be rigorously scientific and rational? That is, the tendency to aspire to an explanation of everything, to merrily ramble from the domains of psychopathology or economic theory to "explain" art or religious belief or what have you. This way pseudoscience lies.

(not that I am disputing John Hyman's point that neuroscience, and indeed science in general, has much of great interest to teach artists and those for whom aesthetics are important. Nevertheless, there's a difference between this and some of the bathetically overblown comments of neuroscientists in the article)

neuroscience and pseudoscience
John Hyman

20 nov. 2000 14:02 UT

I think ‘pseudoscience’ is a fair description of the work I discuss in this article. But much of neuroscience is more rigorous and more soundly based in experimental work.

two questions
John Zeimbekis
11 janv. 2006 12:11 UT

I share John Hyman's negative appraisal of the neuro-aesthetic theories discussed, and partly for the reasons he gives. But I'm not too clear on his methodological position, or on the principles underlying his condemnation. Is there a methodological argument against *any* neuro-aesthetic project? Or is John Hyman claiming that in the occurrence, these theories are false?

Secondly, think there's a lot in common between neuro-aesthetics and some 18th century aesthetic theories (the first aesthetic theories which focused on psychological or mental descriptions). Both kinds of theories seem to perceive a need to explain the 'why' of aesthetic response, and this is surely in itself not a bad thing. So, does John's appraisal also apply implicitly to theories such as,eg, Hutcheson's or Kant's?

one answer
John Hyman
12 janv. 2006 18:44 UT

In response to the first question, I would say this.

The scientific study of the visual system can contribute to our understanding of the visual arts in various ways.

For example, some impressionist and post-impressionist painters used bluish shadows to convey the intensity of sunlight. The bluish shadow registers the colour of the sky. But on a bright day the this colour is intensified by the effect of simultaneous contrast, induced by the yellow colour of sunlight.

So in this case, the artist uses chromatic colour as a surrogate for brightness. And unless we understand simultaneous contrast, we won’t understand how this works.

Again, a lot of drawings depend on the fact, which hasn’t yet been adequately explained, that we can see a line as the boundary of a solid form. Neuroscientists have had a stab at explaining this – unsuccessfully, in my view. But perhaps they will do better one day.

So I don’t doubt that neuroscience can contribute to our understanding of the visual arts. But these kinds of examples shouldn’t lead us to imagine that there will ever be a general neuroscientific explanation of art, or any other kind of general explanation of art. And I’m not aware of any evidence that ‘a common denominator underlying all types of art’ (as Ramachandran calls it) exists.

As I say in this article, I think we should be pluralists about artistic value, as we should be about values generally.

neuro-aesthetic project vs
Seamus Sweeney

12 janv. 2006 23:23 UT

As John Hyman outlines, neuroscience along with other sciences can contribute greatly to our understanding of art.

What seems to be the problem is certain neuroscientists desire to "explain Art." Perhaps I should write "explain" "Art" as, firstly, it is not clear to me that an understanding of "the biological basis of aesthetic experience" actually does bring us much closer to "understanding what art really is."

Secondly, both Ramachandran and Zeki's theorising depends on defining "Art" in ways, as John Hyman wittily shows, that are hopelessly limited and limiting.

We all generalize
Jacky Tibbett
12 janv. 2006 7:21 UT

I doubt that peak shifting plays no role in the aesthetic response to art, rather, it describes a small part of a response to certain pieces of art. Just as a single pathway in a cell cannot account for an organism's behavior, neither can Ramachandran's proposed mechanism satisfy our understanding of the aesthetic response – but we must not rule it out completely. Scientists only manipulate one variable at a time, and thus their theories are necessarily reductionist. Ramachandran and Zeki should never claim they've found the key to understanding art, for surely no single key exists. Still, to criticize headway these authors have made in an emerging science squashes hope for the field to develop into the sound science that I hope it can become.

... more often than we should
John Hyman
12 janv. 2006 18:52 UT

I'm not aware of any evidence that the peak shift effect plays any role in our response to art at all. If Jacky is confident that 'it describes a small part of a response to certain pieces of art', I'd like to know why. Having said that, I'm certainly not trying to 'squash hope'. I'd like to encourage scientific work on the visual arts which is not hot air.

What is the measure of the appropriate distance between S+ and S-?
Gloria Origgi
12 janv. 2006 12:52 UT

John Hyman puts too much emphasis on "bold claims" that are so common these days in the so-called "scientific best-seller" literature, like: "Great art is ambiguity" or "All art is caricature"- and which are often part of the editing strategies to make a scholarly book appealing for a larger audience - instead of addressing some more substantial claims. Nobody wants to find the definitive neural correlate or art, the "art neuron". Yet, we are not entirely satisfied by socio-historical explanations that explain a form of artistic expression in terms of a vector of social, cultural and historical forces of power and domination, and that seem to be much more mainstream today in art studies than neuro-aesthetics. It seems to me a reasonable project to take into account some cognitive constraints in the stabilization of a cultural form of expression, as art is. Zeki and Ramachandran's attempts are efforts in this direction, that is, trying to isolate some perceptual/cognitive constraint at play in the stabilization of an art form. But let's go through some of the details of Hyman's objections: In his discussion on the inappropriateness of the Peak Shift effect in order to explain our art preferences, Hyman "knock-down" argument against

Ramachandran boils down to the idea that the female figures in Indian art are a too far departure from reality to be a super-stimulus: The distance between the stimulus we are predisposed (by training or by evolution) to respond positively and the super-stimulus cannot be too large for the Peak-Shift to take place: *"Ramachandran's explanation of the beauty of the Indian sculpture does not work. In the first place, there is no evidence that the spectators who find it beautiful have been trained in this way. And second, the body shape of the goddess deviates much too far from the norm".* I don't see how this could be such a devastating objection: One can reply that the spectator doesn't need to be trained, but he may have been predisposed by evolution to respond positively to a class of stimuli. And what counts as an appropriate "distance" from two stimuli? A rat that has been trained to positively react to a square (S+) instead of a circle (S-), will react to a bigger square even it is not clear in which sense the bigger square is more distant from the circle than the original square. A better line of objection would be to argue that, at least in the case of female goddesses, Ramachandran's theory seems to presuppose a male audience as target of these works, and I wouldn't bet on this hypothesis. I will return on the objections to Zeki in another message. Just would like to mention in conclusion that a lively discussion around Ramachandran's work has already took place on our website at: Art and Cognition The original paper by Ramachandran and the archives of the discussion are online and available to you all.

two issues
John Hyman
17 janv. 2006 14:32 UT

I shall comment on two issues - briefly on the first issue, and at greater length on the second.

First, Gloria says that I put too much emphasis on the bold claims about art Ramachandran and Zeki make. But these claims are centre-stage in my article because they are centre-stage in Ramachandran's and Zeki's work.

Second, I shall comment on Gloria's account of, and response to, my "knock-down" argument against Ramachandran's theory of art.

Her account of my argument is incomplete. In fact, I claim that the theory fails for three reasons. It fails because it isn't really a theory of art at all, since it doesn't distinguish between a work of art and the kind of object that it represents. It fails because it isn't true that all art is caricature. And it fails because the peak shift mechanism doesn't explain why we often find caricatures interesting, engaging or amusing.

Gloria's remarks are only about the last of these three points. But I don't find them convincing.

(1) Gloria writes: 'Hyman "knock-down" argument against Ramachandran boils down to the idea that the female figures in Indian art are a too far departure from reality to be a super-stimulus.' It doesn't. It boils down to the claim that the beauty or artistic quality or visual impact of the female figures in Indian art isn't explained by the peak shift effect.

(2) Gloria asks how similar S++ and S+ have to be to qualify as an example of the peak shift effect. Actually, it's more helpful to begin by asking how similar S+ and S- minus have to be; and if the generally accepted account of peak shift is correct, they have to be close enough to interact in the way illustrated in figure 4. And S++ has to be close enough to S+ to be the product of this interaction.

(3) Gloria remarks that "the spectator … may have been predisposed by evolution to respond positively to a class of stimuli." This is true, of course. For example, we are all predisposed by evolution to respond positively to sweet tastes. But this doesn't support the idea that the beauty or artistic quality or visual impact of female figures in Indian art is explained by the peak-shift effect.

Readers interested in pursuing this further should read Colin Martindale's response to Ramachandran, 'Peak shift, prototypicality and aesthetic experience', Journal of Consciousness Studies 6 (1999) and H.M. Hanson, 'Effects of discrimination training on stimulus generalizations', Journal of Experimental Psychology 58 (1959).

Similarity and Evolution

Gloria Origgi

31 janv. 2006 12:45 UT

Thank you John for your exhaustive answer to my question. Still, similarity is a quite tricky notion and if S+ and S- should be close enough for the peak shift to produce, I still can't understand why a square and a circle are "close enough" stimuli to produce the interaction in figure 4 and S++ (a bigger square) is close enough to S+ (a square) to be the product of this interaction.

About part (3) in his reply: my point is just that is not completely unreasonable to think that men are not only predisposed by natural selection to have a preference for (S+) big breast, but also to avoid (S-) narrow breasts, a conclusion that seems to be denied by John: *"if that was the S+ body shape – the peak shift towards wider hips and larger breasts would only occur if an inhibitory gradient was created around a female body with slightly narrower hips and smaller breasts than average, and the predicted effect would be that the subject's response would peak at a female body with very slightly wider hips and very slightly larger breasts than average (…)In the first place, there is no evidence that the spectators who find it beautiful have been trained in this way."* I agree that we don't like this kind of speculations because it is a particularly unpleasant way of conceiving the evolution of attractiveness, but we can find many more neutral examples that are compatible with the hypothesis of an evolution of a predisposition to be attracted by S+ and an inhibition towards S-. We are predisposed to favour sweet tastes, and at the same time we are inhibited to like sour tastes. May be a peak shift can produce in front of an even sweeter taste (S++). But I recognize all this is very speculative.

the whole hog

Douglas Galbi

23 janv. 2006 4:07 UT

"There is also the interesting and contentious question of whether it is brains that do the interpreting, or whole animals."

Using a few examples and limited but wide-ranging evidence, I've argued that the whole human body makes sense, including sense of art. See, e.g., my analysis of the Morgan Bible of Louis IX at http://www.galbithink.org/sense-s4.htm

Of course I chose my examples to illustrate my generalization rather than to test it. What's wrong with illustrating? Isn't that a good way to help persons see something new?

"How easily we shrug off our academic training when we take the brave step outside the furrows we were taught to plough!"

What about inspiration? Imagination? Creativity?

It's a sad day in the life of the mind when an insighful, amusing, and learned article about art concludes by encouraging academics to keep to the furrows that they were taught to plough.

sad days

John Hyman
23 janv. 2006 12:36 UT

> Nothing is wrong with illustrating, as long as we remember that it's not the same as testing. And nothing is wrong with stepping onto unfamiliar ground, as long as we maintain our intellectual standards when we do so. It's a sad day when we fail on both counts at once!

3 levels ov adequacy 1 and 2

Jose Luis Guijarro
23 janv. 2006 9:45 UT

In every art discussion I have witnessed so far, we seem to take for granted that we all share the very same concept covered by this one word: ART. This might be so in a sense, especially if we share a lot of cultural presuppositions. It seems to me that this possible sharing, however, is rather problematic, not only because there are people that prefer Falcon Crest to Finnegan's Wake, but also because when we think about the abstract concept of art we immediately make it somehow concrete, relating it (more or less consciously) to objects that are considered art in our cultural world. Ramachandran's paper shows, to my mind, a painfully typical case of this tendency. But so are most of the ideas expressed by Hyman and the other participants.

I am not a neurobiologist. I come from the field of chomskyan linguistics. And I have found that Chomsky's three levels of adequacy are a valuable method to start talking about abstract concepts that we try to understand.

LEVEL OF OBSERVATIONAL ADEQUACY: What are we pointing at when we use the word "art"? Instead of making a list of what all the philosophers and researchers of the world have said about what art is, let me point to a simple cognitive feature without which human beings would be incapable of talking, enjoying and doing what we now call art. It is our strong metarepresentational power. It has been assumed that humans have de cognitive talent to store and manipulate representations in two main ways: representations which are directly stored, and representations which are stored in other representations in incredible chains. If we call some of these metarepresentations "attitudes", we may start pointing to one of them as the (likely) first cause of human idea of art.

LEVEL OF DESCRIPTIONAL ADEQUACY How do we describe this attitude? At first blush, it seems an evaluative attitude. The computational form of it might be as follows. It's just a suggestion, mind you.

ART [V (x)]

In which an object (any object!) might be evaluated as art.

Now, if we want to refine the description of this evaluative attitude further, we may turn to a representational type of description in which we can analyse how art is represented in any given culture and study the relations of these representations with the computational formula I have suggested, or some other.

Finally, we may also look at the implementational kind of description. How are the representations of art actualized in the world: in a cave, around a tree, during an exhibition, or in a museum, etc.

The third level will come in another message.

level 3

Jose Luis Guijarro
23 janv. 2006 9:50 UT

I post as a reply to my previous message the final part of my message.

LEVEL OF EXPLANATORY ADEQUACY To attain this level of explanation in things human, one must understand how all our biological devices evolved by natural selection. Unfortunately, there are still many problems in that field. However, it has been well explained that the mutations which have a positive effect on reproduction and life preservation are the ONLY ones that might stay in a species. It seems doubtful that art, as we know it today in our cultural environments, has any such value, despite some claims in this direction. But, we may yet again point to a deep selective value of our ability to embed representations into other representations. With this sort of functioning we are a species with strong predictive power. Once this is established, we may use a distinction made by Ruth Millikan between direct and indirect functioning of the naturally selected items. We may then be able to posit a material chain of small cognitive arrangements that end up in creating an autonomous device specialised in what we now call art (and which, in some intermediate state, could have been an autonomous device to create supernatural beings who helped us in coping with mysteries).

I know this is all too abstract and, in this short account, probably difficult to accept. But it is the only way I see to tackle the question of this human phenomenon in a biological (cognitive) frame with some kind of hope to achieve more understanding of the complexity of this concept in a series of methological steps which I think are worth taking into account.

on reductionism

Mauricio Suárez
24 janv. 2006 10:49 UT

I agree with this article, and just have one brief comment to add. The author criticises Ramachandran's and Zeki's theories, but he seems to think that in principle the idea of a neuroscientific theory of art is ok. He thinks it is not the reductionism that is criticisable but the details of these particular attempts at reduction. And his critics on this list agree. My view is that we are all being far too deferential to science here, and to a reductionist picture of science in particular.

Philosophers of science in the last 20 years have come to consider highly a very large bunch of strong antireductionist arguments (I am not saying that we all agree on these theses, but we certainly agree that they look so much more plausible now than 30 years ago), namely: that economics can not be reduced to psychology, psychology can not be reduced to biology, biology can not be reduced to chemistry, chemistry can not be reduced to physics, and macrophysics can not be reduced to microphysics.

In light of all these strong antireductionist conclusions (which in no way diminish the value of any of the sciences involved) the idea that ART could be reduced to neuroscience seems to me completely implausible from the start! With all due respect to Ramachandran and Zeki, we (I guess I mean we, philosophers) should not be wasting too much time on this!

But

Rudolf Scheutz
1 févr. 2006 15:57 UT

1. All science can be reduced to mathematics/logics - what is the alternative? 2. What about interdisciplinary collaboration - should we stop it?

All science can be reduced to mathematics/logics
Jose Luis Guijarro
10 févr. 2006 16:47 UT

In my worldview, the problem of mathematics/logic that Rudi suggests is a pseudo-problem. It's like saying that "if all Literature can be reduced to writing" ... etc.

For me, both, mathematics and logic are two symbolic codes with which one might try to reach the second scientific requirement (DESCRIPTIVE ADEQUACY) that I pointed out in an explicit way.

Why should that stop interdisciplinary collaboration?

As I could say in Spanish: "¡Héme quedado de estuco, Doña Ramírez!" (impossible translation)

photomontage
muralee navaratnam
25 janv. 2006 12:45 UT

that photomontage of ramachandran is very amusing! :)

your point about the pitfalls of going beyond your expertise is well made.

very good article, any web links to the martindale article?

Journal of Consciousness Studies
John Hyman
31 janv. 2006 13:11 UT

Look for links to Journal of Consciousness Studies. The journal may be available online, although you may need to subscribe, or to access it from subscribing institution.

Commonality in art already exists
Robert Stonjek
17 févr. 2006 5:21 UT

Art does have a commonality sufficient for the use of a single label to describe it all, that is, “Art”. If something in the perceptual domain can be nominated as “art”, and if there is a reasonable probability that the nomination of that percept can be generally understood, then the commonality already exists – it is up to science to discover and describe it.

If neuroscience can achieve this then all well and good, but this neither means that art has been reduced to neuroscience nor that a neuroscientific description is the only valid scientific account of art.

The first commonality that **I** observe is the subjective perspective utilised by the artist. Among the forms of subjectivity I list the following:

***)** from the artist’s perspective – what strikes the artist is the prominent feature or attribute of a percept;
***)** an insight that the artist may have that may not be readily apparent to others;
***)** from a particular aspect of the artist’s consciousness not readily accessible by or expressed by

conventional (non-artistic) means;
***)** the artist's insight into the impact of a percept on others, that is then reflected back on the percept as depicted (eg The Scream, war scenes; sexually evocative scenes);
***)** any of the artist's experiences, interpretations, or anticipated subjective experiences of others that can be reflected in a particular work (eg patriotic imagery also depicts the feelings the patriotic viewer may experience);

And there are bound to be many others. Even when an object that is quite mundane, like a toilet, is displayed as art we are seeing that the artist sees this is art and so we try to see what the artist sees. The reference to the subjective experience is still present.

Photographs can also be art if the use the artist can capture a seen in a way that has special and unique significance to the artist, a perspective that even another person present at the same scene would not have perceived at the time but would see upon meditation of the resulting photograph.

The world seen through my eyes may be very different to yours. Upon walking down the street, a mundane and featureless act, I may note the old man whose life is draining away, and yet he has stopped to smell a rose and contemplate the rose given to a beautiful young woman upon first meeting, to a wife after a disagreement that left them both weeping; and cast upon the grave that was dug far too soon.

That is art.

Kind regards,
Robert Karl Stonjek

some casual remarks on neuro-aesthetics
Jacques Morizot
21 févr. 2006 13:26 UT

I perfectly agree that any general characterization of art is questionable; it is undoubtedly true of "caricature" or "ambiguity" but I think it is equally true of lots of venerable formulae such as "unity in diversity" or the like. The difference is not all so great and focusing on these aspects is largely beside the point. Neurobiology raises indeed a special problem because it is a conquering field today. Is something refered to as neuro-aesthetics bound to become the "next big thing"? Of course I don't know and I doubt somebody knows for certain but I don't find in principle any good reason to see this possibility as a frightening menace. It is a fact that the great steps forward in experimental techniques (like cerebral localization or response of selective groups of neurons) give the impression that some old (and hitherto resistant) problems are about to be solved. It is at the same time a normal reaction and a commonplace illusion. But does it cast suspicion on the whole process? It would seem to me regrettable and I think better to lean towards Patrick Colm Hogan's stance when he says that humanists have a personal responsability vis-à-vis the cognitive studies because they are in the best position to tackle art's and literature's most specific contents. Here is the difficult challenge. One prominent book on art is Martin Kemp's Science of art that shows its dependency of physical facts and theories. Why would it be more pertinent to broach painting through optics or geometry rather than neurobiology? Of course the problems are not at the same level and the study of the structure of works is different from that of the ways of creating. Nonetheless, we are ultimately neuronal organisms if not machines: it must mean that our activities and productions are in part released or regulated by animal reactions, and conversely that any explorations of the works must find some trace of it. A passage from Flint Schier may make think twice about it: he said that if he was asked how we can recognize so many things at once [in a given area of a picture], he would "reply that we just do. Any further explanation must be of a psychobiological kind" (p. 82) and nobody would take this remark as reductionist. Many formulations, notably by Ramachandran, may indeed seem and are probably excessive and even extreme. Is it not the cost due to the advancement of research in an uncertain

area and a necessary condition to get improved theories? In fact have we not often today a similar impression regarding the cartesian theory of passions or maybe Burke's notion of delight? So let us try to make an optimal use of falsifiability. In sum I'm not so pessimistic and defensive as you seem to be. Your remarks are convincing but taken too literally, they would also miss to take the opportunity to take up a challenge.

Is Neuro-aesthetics derived from 'Theory-Of-Mind'
Robert Stonjek
22 févr. 2006 12:29 UT

Any new branch of science must have predictive power – what is Neuro-aesthetics going to offer?

The extent of ***Theory-of-Mind*** in humans is well known to be unique among animals, other primates having only a rudimentary form. Mirror neurons are well developed and extensive in humans, and this ability to effectively mirror other's mental states is the foundation upon which *Theory-of-Mind* is built.

It would not surprise me at all if a correlation between the possession of mirror neurons and *Theory-of-Mind* is discovered in various species generally.

But do we stop with what we are born with or does the evolutionary push that must have led to greater ability to emulate others perspective extend to our culture as well? I think this is exactly what has happened and is amply demonstrated in modern art (in particular) where ever more obscure, rare, and infrequent states of mind are depicted and thus can be mirrored.

The artist can project back onto the art object the feelings inspired within the artist when exposed to the model (which could be an object, person, situation, vista or even an internal state having no obvious expression outside of art). The exaggeration of this projection may well be described as caricature for some forms of art, but can this description extend even to written work?

The cutting edge of human evolution is no longer genetic, but cultural, where 'memes' may be said to proliferate or vanish according to the whim of the humans who select them.

Our *Theory-of-Mind* ability has been evolving through culture, and it is the artist that offers the raw material for this further evolution. What else were the first cave paintings but events outside the cave as seen and experienced by the hunter – can we see their fear, elation, skill and so on, almost as if we were there hunting with them? Wasn't Dali venturing beyond the cave of our experience and returning with portraits of experiences we may never before or since have experienced for ourselves, except via the vicarious form available to humans with their advanced *Theory-of-Mind* capacity?

Using *Theory-of-Mind* as guide, future forms of art can be anticipated. Those emotions commonly experienced in the past but no longer by modern people will become the subject of art as people wish to be exposed, vicariously, to the full range of possible human experience.

Kind Regards,

Robert Karl Stonjek

some casual replies

John Hyman

22 févr. 2006 15:00 UT

Here are a few comments in reply to Jacques Morizot's challenging intervention.

First, Jacques is right to point out that philosophers have also tried and failed to define art, or to identify its essence. However, the philosophy of art has abandoned this ambition in the last fifty years or so, rightly, in my view. Sadly, Zeki and Ramachandran have so little patience for philosophy, or interest in it, that they are evidently unaware of this. So they repeat the mistakes of earlier generations of philosophers, even though they imagine they are entirely free from the influence of philosophy.

Second, I don't find neuroscience menacing either. But it would be sad, in my view, if attempts to understand art, or indeed any other significant aspect of human life, simply ignored the intellectual tradition in which this attempt has been pursued for two and a half thousand years. I don't say this for nostalgic reasons, but simply because discarding Plato, Aristotle, Aquinas, Hume and Kant, on spurious grounds that 'that 'aesthetic theories will only become intelligible and profound once based on the workings of the brain', is bound to lead to the kind of banal and jejune thought I criticized in this article.

Third, I see no particular reason to doubt whether neuroscience can contribute to our understanding of the visual arts. But it will not do so by proposing a general theory of art. If neuroscientists cannot explain how we can recognize a tooth brush, how can they expect to explain 'the key to understanding what art really is' (Ramachandran) or 'the biological basis of aesthetic experience' (Zeki)? This doesn't deserve to be called hubris. It's just silly. It is like trying to understand the mathematics of general relativity before one has learned to add.

Finally, Jacques says that he is not as pessimistic and defensive as I seem to be. I was trying to be offensive, not defensive. And as for being pessimistic, I have always been impressed by Gramsci's remark: 'Pessimism of the intellect; optimism of the will.'