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Letters to the Editor

Author(s): ~~Philippe Bordes, James Henry Rubin, Peter Welch, Linda B. Eddy, Gordon W. Sutherland, Robert Hansen and John L. Ward~~

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Letters to the Editor

Sir:

James H. Rubin's article on Topino-Lebrun in the December, 1976 issue of *The Art Bulletin*, 547–568, heralds modern research on this forgotten painter from Marseille, born in 1764 not 1769, who never fails to be cited as David's most radical pupil; and in passing the reader has the pleasure of discovering a magnificent unpublished drawing by Drouais. But confronted with the documents brought forth by Rubin and with a certain number of others, newly discovered, I have come to a quite different interpretation of Topino's *Death of Caius Gracchus* (Salon of 1798). The argument proposed by Rubin is that the painting was executed from 1795 to 1797, "that as well as a personal manifesto it was a piece of political propaganda meant to serve the cause of the Conspiracy," and that Topino expressed a willingness to play an active part in furthering the goals of Babouvism. Whereas I contend that Topino, a Jacobin but not a Babouvist, executed his painting only after Babeuf's death as an homage to the martyred democrat; it is not my intention to repeat the lengthy details of my argument, which is the subject of an article, "Topino-Lebrun, Babeuf et la peinture sous le Directoire," to be published in *La Revue de l'art* (late 1977). But I do wish to examine the piece of evidence that permits Rubin to assert that Topino was painting in 1795. This is the date he ascribes to the visit Delécluze pays to the painter who is at work in the Church of the Feuillants. Delécluze adds that on the same day Alexandre Casanova led him to a Jacobin reunion in a club of the Rue du Bac, which Rubin identifies as the Société du Panthéon (meetings authorized from November, 1795 to February, 1796). There are two reasons for rejecting this identification: the club that met in a former convent near the Pantheon is about a mile from the Rue du Bac; that Delécluze is accompanied by Alexandre, a fellow pupil of David, suggests that the visit to Topino took place after the winter of 1796–97 when Delécluze was first admitted to the studio of David. Rubin states that there is no Jacobin club activity between the period of the Panthéon reunions and the meetings in the Salle du Manège (summer, 1799), when in fact the longest section of Isser Woloch's outstanding study, *The Jacobin Legacy* (Princeton, 1970), which Rubin cites, is a discussion of the numerous Jacobin Constitutional Circles formed after Fructidor (September, 1797). The most important of the Paris clubs is precisely the Circle of the Tenth Arrondissement in the Rue du Bac (open in January, 1798). I would argue that the visit to Topino took place after the death of Babeuf (May, 1797) between January and March, 1798, when the clubs are once again closed down.

I agree with Rubin that Topino may have considered painting a subject from Plutarch's life of Caius Gracchus prior to the death of Babeuf, perhaps as early as 1792 in Rome. Drouais's unfinished project was a powerful incentive; the possible role of Chénier's play at a later date is more problematic. But it seems to me unlikely that he obtained the commission of the Church of the Feuillants and seriously began work during the agitation of Vendémiaire, followed closely by his departure for Switzerland with Bassal. In his published Letter to the Jury, dated 2 Pluviôse an IX, Topino states that at the time of the Grenelle affair (September, 1796), he had been in Switzerland for almost a year. In my opinion, this departure signifies a refusal to participate actively in the insurrectionary project of Babeuf, an attitude very common among the Jacobins. During October, 1795 Topino helps both Babeuf and Jullien publish their prison writings on Vendémiaire, and that is the extent of the painter's "active part." It should be added that Jullien and Babeuf are in open political disagreement within two weeks after being set free, that Jullien is not in the least a Babouvist.

Although his discussion rests in part on the regrettable confusion between Pyrrhus and Xerxes, the contrast that Mr. Rubin establishes

between David's *Léonidas* and Topino's *Siege of Lacedaemon* is of great interest. I prefer the other title that Topino adopted for his project, the *Spartan's Repulse Pyrrhus*, which indicates more clearly the intention to represent no individual hero. Plutarch describes this moment of exceptional danger when all of the inhabitants of Sparta—including the aged, the women, and the children—are called to defend their city until military reinforcements arrive. This extraordinary project was conceived by Topino in the wake of the war crisis of 1799—assassination of the French plenipotentiaries at Rastadt, military defeats on all fronts—and Topino adopts a political line that echoes the speeches heard at the Manège reunions during the summer.

Finally, the publication of a small-scale copy of the *Death of Caius Gracchus*, executed in the 1860's by the mediocre painter Glize, as Topino's original is a confusion that Rubin can only regret. There is in fact no photograph available of the painting, and very few persons have even seen it. It was unrolled on the floor of the Marseille museum during a weekend in 1973 and appeared to be in need of considerable restoration. The painting will soon be relined and superficially cleaned, thanks to the admirable persistence of the art critic Alain Jouffroy, and a photograph will become available. Rubin states that "the picture has for rather obvious aesthetic reasons sunk into obscurity"; the *Caius Gracchus* is no masterpiece of execution, but an important element of the painting's history is here ignored. Documents published by E. Parrocel (*Annales de la peinture. discours et fragments*, Marseille, 1867, 175–78) relate that within ten years after being sent to Marseille the exhibited canvas was damaged by the public for political reasons, to such a degree that it was rolled up and auctioned off as rubbish. Fortunately the picture survived this execution and in the 1860's Parrocel rediscovered it in the possession of Glize, who restored it heavily. In the 1880's the Marseille museum acquired it for the second time. Submitted to such a long series of aggressions, both intentional and unwitting, far from having sunk into obscurity, Topino's picture seems to possess an extraordinary capacity for survival.

PHILIPPE BORDES

Université Paul-Valéry, Montpellier

Reply

Sir:

Between the two of us, Philippe Bordes and I may well constitute a majority of the world's living "Topinistes," so naturally an exchange of our viewpoints can only benefit the general public. I am very glad that Bordes caught the two errors in my article that I do indeed regret and that I would have had to set straight myself in writing had he not done so for me. First, the confusion between Pyrrhus and Xerxes made it seem as if I conflated Topino's and David's scenes more than they were meant to be or than was necessary for my argument, which was clearly based on their generic association. I had just been talking about Pyrrhus, and I inadvertently continued to use the same name. Second, I and others should thank Mr. Bordes for his illuminating history of Topino's painting and its copy by Glize. I had long been awaiting a photograph of the original, but when I was led to believe that its restoration was indefinitely postponed, along with the possibility of obtaining a photograph, I at the last minute decided—I think rightly on these grounds—to substitute the photograph of the copy. The photograph itself was properly labelled, but I neglected to change the figure caption when it came to me in proof. I very much regret the unfair damage that may have been done to the esthetic

aspect of Topino's reputation—though as Bordes himself intimates, even restoration of his painting will show him a lesser light. I am happy to hear now that restoration is being undertaken after all.

Bordes and I shall have to remain in disagreement on the more fundamental issue of interpretation, however; that is, on the matter of where the "content" of a picture stops and where its "context" begins, assuming the two to be radically different. In Bordes's view, the significance of the *Caius Gracchus* is limited as "an homage to the martyred democrat." In mine it expands as "a personal manifesto" for Topino that was "meant to serve the cause of the Conspiracy." Whereas my argument depends indeed on the proposition that in this case the line between content and context is virtually indistinguishable, Bordes, appearing to believe the opposite, wishes to prove his point incontrovertably by removing Topino's conception of the *Caius Gracchus* to the period following Babeuf's death. Whether he is right about this date or not—a matter I shall take up in a moment—it still seems to me that given the precedent of Chénier's *Caius Gracchus* and the presence of Babouvism in the French consciousness, it is inconceivable that Topino would have chosen his subject if he had in fact wished to maintain his distance from Babouvism or, regardless of his relations with Babeuf—to which documents in fact suggest only a positive side—from the general set of ideals Babouvism represented. As Bordes quite fairly admits, the interpretation of Topino's voyage to Switzerland, as well as its exact dates, is a matter of conjecture. I believe one cannot escape Topino's active part in helping Babeuf and Jullien publish their prison writings, or that, as I also pointed out, Topino allowed his name to be used as part of a "Babouized" National Convention. In the absence of better evidence than this, I can only argue for a basic pattern in Topino's activity, and I see one without major contradictions.

Thus, it might even be said that the date of inception of Topino's painting is in itself unimportant. Still, in addressing the issue once again, I hope to resolve this small problem both for myself and for Bordes. For one thing, Bordes's letter failed to cite a significant piece of evidence that clearly supports my hypothesis, even though his own article, "Documents inédits sur Topino-Le Brun," in the forthcoming *Bulletin de la Société d'Histoire de l'Art Français* will show him to be thoroughly familiar with it. This is the remark made in the *Décade philosophique* of 30 Brumaire, Year VII [November, 1798] that the painting of *Caius Gracchus* had cost its author several years of work. Such a statement unequivocally places the picture's inception well before Babeuf's death and most likely around the period I indicated. Furthermore, Delécluze's recollection was by no means as confused as Bordes would have it, being on the contrary quite detailed in its description of historical context and in its references to dates. The visit to Topino's studio did not in fact coincide with the undertaking of the *Caius Gracchus*; rather, its importance is that it provides us with a *terminus ante quem* for it, since, according to the account, Topino had already been working for a while, presumably for several weeks or a few months. Since Delécluze commented that the picture was not exhibited until two years after his visit (at the Salon of 1798), a visit in 1796—I would say early in that year because of Topino's departure for Switzerland—would again locate the painting's beginning in late 1795. This date corresponded to Topino's active relationship with the Babouvists, thus making all the evidence totally consistent.

This restatement of my argument represents a very slight clarification of the claim in the text of my article that Delécluze's visit occurred in winter, 1795–96. The date "end 1795" used in my Appendix was intended as a *terminus post quem* for the visit, but for those who did not read the text carefully it was obviously a bit misleading. Another important point in support of my hypothesis, however, is that Delécluze's remarks themselves specifically located the Babeuf condemnation—the date of which he perfectly recalled—as sometime after the visit to Topino. Thus, even if his visit was after Topino's return from Switzerland, the point is that the artist was without a doubt working on his painting before Babeuf's condemnation and death. This makes Bordes's arguments completely untenable, for to locate Delécluze's visit as late as he wishes, Delécluze's memory would have to be mistaken twice and the *Décade philosophique* more than a little wrong.

Bordes's other points, some of which seem appealing, can be easily disposed of. First, although Delécluze could perhaps have been a student of David already, depending on the exact date of the visit, this is in no way a precondition for his encounter with Topino. Cassanove, who was older than Delécluze, knew the latter's family well, dined with them frequently, and thus could easily have taken the aspiring youngster to Topino's at almost any time. Second, although the addresses of various Jacobin clubs do suggest that the visit to a radical gathering the same day as the visit to Topino's studio could have taken place later than I indicated—and I never denied this in principle—they hardly prove, particularly since other evidence is involved, that the visit could not have taken place earlier, even well before Babeuf's death. Thus, mentioning the revival of Jacobin activity of 1797 in my text (I did mention it in my Appendix) was irrelevant to my argument and still is, although I do stand corrected for having called the Rue du Bac group by the wrong name. Finally, although when exhibited in 1798, Topino's painting inevitably invited the reference to Babeuf's death, and by this time Topino may even have thought of it principally in such terms, it should not need pointing out that the use of a death scene to glorify a hero and his ideals had been the most common of all *topoi* in history painting well before Babeuf's death. Moreover, well aware of the fate of the Gracchi, Babeuf himself had a morbid and willful anticipation of his own destiny that may help explain Topino's prophetic accuracy.

Although Bordes certainly deserves credit for forcing me to clarify a few matters of detail, I wonder if there is not another level to our disagreement. Might not his contention that Topino's picture completely postdates Babeuf's death—and his rather arbitrary relocation of the date of Delécluze's visit to help him do so—reflect a kind of vested interest, though by no means necessarily deliberate, in reducing Topino's painting to no more than an homage to a martyred democrat? Bordes seems unwilling to accept the possibility of a greater dimension to this picture or to Topino's intentions, that is, that Topino could have invested it with the power of ideas conjured up by Babeuf's mere name. This dimension would of course have to exist as the very motivation for Topino's picture if indeed, as I think I have demonstrated, he began it before Babeuf's death. Surely the long passages of my article explaining the signifying power of Babeuf's names and "emblems" and his self-consciousness about them show this to be the case. Thus, the difference of opinion that our discussion of the date of Delécluze's visit may mask could well boil down to that of our very different postures as narrow versus broad constructionists. It seems to me that here we have a rare case in which the broader view is the more informative, and also in which overcautiousness is less totally consistent with evidence and, far from being safer, actually ends up being misleading.

JAMES HENRY RUBIN
Princeton University

Sir:

May I comment upon several omissions and errors in Linda R. Eddy's article, "An Antique Model for Kauffmann's *Venus Persuading Helen to Love Paris*" (*The Art Bulletin*, LVIII, 1976, 569–573).

First, with regard to her claim that "writers have not yet considered the antique prototypes for this painting," may I call to her attention the analysis of this picture and its antique sources in my Ph.D. dissertation, "Angelica Kauffman," Princeton University, 1968, 347–51, in which I discuss and illustrate most of the probable sources she puts forward in her article (including the Farnese relief, the Jenkins Vase, the Aldobrandini Wedding, and the—apparently unknown to Ms. Eddy—drawing in the British Museum after yet another *Venus Persuading Helen to Love Paris* relief which was, as of 1788, in the collection of Colin Morrison in Rome). By the way, I remain convinced that the Jenkins Vase (despite its having left Rome in 1776) might easily have been in Kauffmann's mind when she painted this picture. Thomas Jenkins, the one-time owner of the vase, was Kauffmann's close friend; he acted as her financial agent in the sale of the *Venus Persuading Helen to Love Paris* to Prince Nicolai Youssouppoff; eighteenth-century engravings of the vase are known to exist (one, undated, is in the British Museum Department of Greek

and Roman Antiquities). All in all, a convincing case can be made for the Jenkins Vase as Kauffmann's chief prototype.

Had she troubled to read my dissertation or any other reliable modern source of information on Kauffmann, Eddy might have avoided some of the numerous errors that plague her article. Kauffmann's *Zeuxis Choosing Models for His Painting of Helen* really should not be used as evidence that she visited the Farnese Collection in 1763–65, as this painting dates from ca. 1790, well after the artist's return to Italy. This second extended residence in Rome did not, as Eddy has it, begin in 1786, but rather in 1782. Kauffmann did not make frequent trips to Naples after 1787 to paint the Neapolitan royal family; she delivered the finished portrait of *Ferdinand IV and His Family* in 1784, and never returned (thus she could not have seen the Farnese relief after it was moved to Naples in 1787). Kauffmann cannot be said to have "restored the Venus Persuading Helen theme to art." There is a Benjamin West *Helen Brought to Paris* of 1776 (ex-Graham Gallery, New York), and Gavin Hamilton's *Meeting Between Helen and Paris* of 1782–84, painted for the Stanza d'Elena in the Villa Borghese, may today be seen in the Museo di Roma.

PETER WALCH
The University of New Mexico

Reply

Sir:

I regret that I was not aware of Peter Walch's unpublished dissertation on Angelica Kauffmann. Clearly it would have been of interest to me, although, as I will point out below, it would not have altered the conclusions of my article.

Walch's main disagreement seems to be my dismissal of the Jenkins Vase as Kauffmann's model for her painting of 1790. As my article shows, I considered the Jenkins Vase in this role, but rejected it in favor of the Farnese relief and, to a lesser degree, the Vatican relief. I did so for the following reasons.

The Jenkins Vase, even as Walch admits, seems never to have been concretely accessible to Kauffmann. It is all very well to point out that Thomas Jenkins was Kauffmann's financial agent in Rome at the time that she painted her *Venus Persuading Helen*—as he was for at least a decade. But the fact is that in 1790 the vase had not been in Jenkins's possession for fourteen years. During that time it had been owned by the Smith family in Cheshire, and, as I said in my article, I know of no evidence that links Angelica in any way with these English owners. Of course a drawing or engraving of the Jenkins Vase could have made up for Kauffmann's inability to see the vase in person. But as I stated, the Jenkins Vase was unpublished when Kauffmann painted her scene of *Venus Persuading Helen*; evidently it did not appear in published form until it was included in Tischbein's *Homer nach Antiken*, 1801–04. Walch mentions eighteenth-century engravings "known to exist," but is specific about only one of these, an undated and unsigned engraving in the British Museum. I was aware of this engraving, but rejected it as a possible influence on Kauffmann for two reasons. First, assuming that the engraving was available in 1790, only weak arguments could ever be made for Kauffmann happening to have access to it. Second, and more damaging to the case, is information in the photographic files of the Courtauld Institute that indicates that the engraving is most likely an early nineteenth-century work, probably post-dating the Tischbein publication.

Thus my research gave me no reason to believe that the Jenkins Vase was either concretely or visually accessible to Kauffmann. On the other hand, for the reasons laid out in my article, there was good evidence that she knew and had actually seen the Farnese and Vatican reliefs. My conclusion was that these two antique works, modified somewhat by a knowledge of an antique Bacchus and *putto* group, were Kauffmann's inspiration. I still feel comfortable with that conclusion.

Now I want to comment briefly on the "numerous errors" that Walch feels plague my paper. I yield to him on one of these and apologize for my mistake: indeed, Kauffmann did not return to Naples after 1787. I should point out, however, that Walch, too, seems to be in error here, for Kauffmann's last trip to Naples was not in 1784; she

was there for the last of her long visits with the royal family in the summer and fall of 1785. I also understand the point he has made about my footnote reference to Farnese connections with Kauffmann's *Zeuxis Choosing Models*. The brevity of my footnote may have left the impression that this painting was the immediate result of her early visits to the Farnese Collection. I did not mean for it to be interpreted in that way. I must take issue, though, with Walch's other two complaints.

First, I think that I am right in saying that Kauffmann's second extended residence in Rome began in 1786. My aim was to use the word "extended" to distinguish this period of permanent residence in Rome from the transient period from mid-1782 through 1785 when she lived for blocks of time in both Rome and Naples. By my calculations, she was in Naples during those years for more than forty percent of her time. By contrast, from 1786 until her death in 1807 she was away from Rome for no more than a few months.

My second refutation is far more important. It concerns Walch's last point. I am of course aware of West's *Helen Brought to Paris* (1776) and Hamilton's *Meeting Between Helen and Paris* (1782–84). But these paintings do not negate my contention that Kauffmann "restored the Venus Persuading Helen theme to art," for they are not of the Venus Persuading Helen theme. One should immediately notice that the arrangement of the characters in West's and Hamilton's works, and even the figures included in Hamilton's painting, are quite different from the antique prototypes with which my article dealt. But of greater significance is the fact that the narrative implied is a very different one. The paintings by West and Hamilton are presentation scenes, not persuasion scenes; that is, West and Hamilton show us Venus presenting a modest Helen to Paris as a prize, not Venus persuading Helen to accept Paris.

But there is more to counter Walch's idea that West and Hamilton modelled their paintings on antique examples of *Venus Persuading Helen*: their paintings seem to reflect quite another source. The "girl presented as a prize to boy" kind of composition that West and Hamilton used has its own Classical prototype, and both artists were surely aware of it. In fact, Kauffmann herself seems to have borrowed from this model before Hamilton and possibly before West for her own version of the theme, *Venus Presenting Helen to Paris* (this English work was engraved for Kauffmann by Ryland; therefore a *terminus ante quem* of 1777 can be established). Moreover, this same Presentation motif sometimes served eighteenth-century artists as a convenient model for yet another Venus, Helen, and Paris scene—that from the *Iliad* 3. 424ff. A case could be made that this is the moment represented in West's painting, rather than the first meeting between the two lovers.

Thus I think it is clear that the works by West and Hamilton cited by Walch are not based on the antique Venus Persuading Helen or Venus Persuading a Bride models with which my study was concerned. I therefore saw no reason to mention them—or, indeed, any number of other eighteenth-century paintings of Paris and Helen at various stages in their history.

Again I say that I regret not having known of Walch's dissertation. I should mention, though, that in reading the pertinent section now, I see that except for his noting possible antique sources for Kauffmann's painting of 1790, it has no parallels to the theme of my paper.

LINDA R. EDDY
Stanford University

Sir:

Bruce Chambers's article, "The Pythagorean Puzzle of Patrick Lyon," *The Art Bulletin*, LVIII, 1976, is very interesting from an iconographic viewpoint; may I comment on one or two important aspects that he might like to reconsider in his analysis? They could add significantly to his interpretation.

Perhaps the most important is the place of Tubal-Cain, not Vulcan, in Masonic tradition. Tubal-Cain was the traditional worker in metals. Talmudic legend records an interesting story about him, which we could imagine epitomizes the steadfastness, dignity, and pride that Neagle portrays in *Pat Lyon at the Forge*. King Solomon is

said to have gathered together in his throne-room the craftsmen who had assisted in the building of the Temple. Here he was to choose the one among them who had contributed most, so that he could be honored by sitting on the throne as King of Israel for one whole day. He had not invited the blacksmith, however, and when the curtains around the throne were drawn back, there stood Tubal-Cain before the chair. The King's bodyguard immediately moved to seize him, but Solomon stayed them until they heard what he had to say. The blacksmith claimed exclusive right to the honor of occupying the throne because without him and the metal tools that he made, no other artisan could have done any work at all. Solomon agreed and Tubal-Cain occupied the throne for the day.

In the painting, Patrick Lyon does not have the fieriness of a Vulcan. He is more of a reincarnate Tubal-Cain standing before the throne, asserting his rights with dignity, proud of his pivotal position as an artisan without whom all the gentlemanly "great" on earth would be as nothing, and garbed with the clean shirt of the speculative craftsman.

There may be no authoritative document to support this interpretation, just as there is none to support Chambers's or Dunlap's Vulcan. But perhaps Lyon had read the Talmudic legend in some form similar to Dr. A. Posman's *Freemasonry from the Talmud*, which was published much later, in 1905, and in which the Solomon story is recorded. If the iconography is to be put in a Masonic framework, then the logic of Tubal-Cain source, rather than a Vulcan one, is much sounder.

At the same time, the whole tradition of the apron in Masonic symbolism is reflected in the "freshly laundered and unstained shirt," the shoes and polished buckles, surmounted by the heavy apron. For it is symbolic of the change from the operative to the speculative craftsman, perhaps from the uncommitted Patrick Lyon to the socially aware Patrick Lyon, from the "mechanic" to the philosopher, for by now he had undergone his trial by social fire and had written *The Narrative of Patrick Lyon* (1799). The pose should not be looked on as a "masquerade" but more correctly as an ingenious synthesis in a kind of Hegelian dialectic.

There is no lambskin apron evident. It is the usual heavy leather blacksmith's one that Lyon asked for and is wearing. Indeed, had he worn a lambskin, the effect would certainly have been a "masquerade" and even a monstrous and incongruous charade. There is therefore no really valid reason for Chambers to mention the lambskin here at all. It would tend to emphasize a very superficial, dictionary understanding of Masonic symbolism rather than a sound awareness of its deeper esoteric meaning. In fact, if there is any statement of innocence at all in the iconography, it is not to be found in the apron but in the detachment of the Tubal-Cain setting.

Finally, the whole conception seems to show that Lyon was not only a freemason but that he was skilled and knowledgeable in the Craft. In Philadelphia, a great city of great American freemasons, it is more than probable that John Neagle would also have been a freemason. Could he otherwise have executed the work?

GORDON W. SUTHERLAND
Vancouver, B.C.

Sir:

May an artist contribute to the discussion?

In "A Reexamination of Van Gogh's Pictorial Space" in the December 1976 *Art Bulletin*, John L. Ward proposes to account for curves in the *Bedroom at Arles* by positing rather unlikely behavior on the part of an artist, and I believe that he incorrectly faults Van Gogh's sense of linear perspective.

Ward appears to agree with Ernst Gombrich (*Art and Illusion*, Princeton, N.J., rev. ed., 1961, 256) that an artist "sitting in front of a long-stretched facade . . . would therefore scan the view by moving

his head . . . as he turns right the facade will appear to converge one way, and as he turns left in another; but if he wanted to paint these aspects, he would quite instinctively shift his easel so as to stand obliquely to the facade . . . While he turns . . . he is aware of a succession of aspects which swing around with him. What we call 'appearance' is always composed of such a succession of aspects, a melody, as it were, which allows us to estimate distance and size; it is obvious that this melody can be imitated by the movie camera but not by the painter with his easel."

With Gombrich, Ward seems to go to inordinate lengths to avoid the simpler conclusion that the separate saccades¹ of the eye might be experienced easily by a painter as a glissade, a melisma of a melody, a single continuum of vision like our elision of separate frames that permits us to perceive continuous motion in a film. While Van Gogh certainly experienced the separate views (in the drawing, the "collisions" are unmistakable), I submit that later, as he painted, he quite possibly kept his attention on the lower edge of the bed as he scanned from side to side. The curve as "a compromise that does not represent one aspect but many" is of course a possible explanation. More plausible to me, however, is the gradual transformation from separate staccato glances to a single scanning glissando, depicted with direct curving brushstrokes. Whether or not the painting is smaller than the visual angle of the scene (and who knows? Perhaps the artist sat or stood much closer to Ward's point B') I believe that spatial curvature might "automatically result."

As an artist, it seems to me unrealistic to assume that the canvas must be moved whenever the artist's glance moves. Indeed, because the artist customarily "puts the canvas where the scene isn't," it is nearly a rule that the canvas, paper, etc., is placed at an oblique and often perpendicular angle to the subject's picture plane. Even if the easel is moved now and then, it is difficult to imagine any artist moving it often, and certainly not nearly as often as the eye is moved. I doubt very much that it was "normal procedure" for artists at any period to block the view of half the subject with the canvas while painting the other half. Subjects such as the *Bedroom* and *Night Café* are easily drawn and painted on a surface perpendicular to the entire scene. The number of times that any painter must compare opposite ends of a composition for a variety of purposes virtually requires him to learn very early to bring to his easel, wherever it is located, the visual data from a wide-ranging scanning of the scene before him, and prevents him from acquiring the easel-shifting habit that Gombrich and Ward describe. The importance placed on this shifting of the canvas will not convince many artists, I dare say, that Van Gogh's curves were derived from an attempt to joint separated and conflicting perspective views.

I find it difficult to understand how Ward can conclude that Van Gogh had a "strong sense of shape constancy" that "made it difficult for him to see in perspective." Granted: Van Gogh's paint is "painterly" and "impastoed," and his line, color, and texture are essentially flat; he seldom uses chiaroscuro in painting to model volume. In other words, he probably did not see in *aerial* perspective. It is true that his converging diagonals do not systematically meet at a plotted point; and his small forms and short lines are often casual and imprecise. Nevertheless, his *shapes* are characteristically foreshortened, bounded by emphatically converging diagonals, as in a wide-angle view. He shares this combination of dramatically foreshortened perspective *shapes* and relatively two-dimensional color and pattern with Degas and Toulouse-Lautrec, and to some extent with Renoir, Manet, and Monet. I urge the skeptic to scan any extensive catalogue of Van Gogh's images in search of indications of shape constancy: the preservation of rectangle, circle, or any other true proportions in oblique views. Exceptions do occur in certain repetitious patterns: in *The Zouave* and *The Yellow Chair* the floor tiles are indeed conspicuously rectangular, in conflict with the perspective space. And in drawings that accompany his letters (*Complete Letters*

¹ D. Noton, and L. Stark, "Eye Movements and Visual Perception," *Perception: Mechanisms and Models (Readings from Scientific American)*, San Francisco, 1972, 219: "During normal viewing of stationary objects the eyes alternate between fixations, when they are aimed at a fixed point in the

visual field, and rapid movements called saccades. Each saccade leads to a new fixation on a different point in the visual field. Typically there are two or three saccades per second. The movements are so fast that they occupy only about 10 percent of the viewing time."

of Vincent Van Gogh, New York Graphic Society, undated), cobblestones on p. 178 and garden rows on p. 242 of Volume I maintain shape constancy, but right angles and parallel lines are rare exceptions. Even youthful paintings and early drawings exhibit academic ease and skillful foreshortening, especially where rectangular streets and garden plots are exploited to depict rapidly receding planes. Is it not fair to conclude that Van Gogh saw in linear perspective as well as any of his contemporaries, and that he rarely demonstrated shape constancy?

As for Ward's note 24, suffice it to say that my statement ("This Curving World: Hyperbolic Linear Perspective," *Journal of Aesthetics and Art Criticism*, xxxii, 1973, 151) is intended to pertain to all viewers, artist as well as spectator. *The Art Bulletin* is probably not the place to respond adequately to numerous other points raised in Ward's interesting article, concerning "veridical" photographs, "proper" viewing distance, "proof" of what an artist saw, the foreshortening of the picture itself, and the conventional nature of "correct" perspective. The debate simmers in current and past issues of *Leonardo*, stirred by contributions from Ward, Gibson, Arnheim, Pickford, Ronchi, Reggini, Maurin, James, and Turner, among others.

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Reply

Sir:

Robert Hansen (to whom I wish to apologize for misspelling his name in references in my article) believes that Van Gogh may have kept his easel in one position while painting the *Bedroom at Arles*; I had hypothesized, on the basis of my own procedures as a painter, that, given the wide angle of view, the canvas would block part of the room unless placed perpendicular to the back wall and that this position would have required Van Gogh to turn his head every time he looked from the motif to the painting, an inconvenient procedure. I felt that it would be easier to place the canvas beside the part of the room to be painted and move it when it blocked the view. In attempting to explain how the curvature could have occurred as an artifact of Vincent's procedures, I described a hypothetical situation in which the canvas was gradually moved from one side of the room to the other—although certainly not "as often as the eye is moved." Even two changes of the picture's position could create perspective shifts that curvature may have been introduced to reconcile.

It is certainly possible, however, that the painting was done with the canvas in one fixed position, far to the side and perpendicular to the back wall. Even then, I believe that the curvature is more likely to have been used to smooth over perspective shifts that occurred in making the painting than to have been perceived as such. When a canvas is positioned away from the motif, so that the slant of lines cannot directly be matched with the edges of forms, the question to be determined is, what information does the painter extract from the motif? If Van Gogh had viewed the room through his perspective frame, he might have noted that the near side of the foreground chair and of the bed slanted away from the plane of the frame in the same direction, as they do in Figure 5 of my article. Without this reference plane he was more likely to have noticed that the bed end *slanted away from his station point* in one direction and the chair side in the opposite direction. To put the matter another way, he was more likely to have noted the angles that receding edges of the motif made when projected on an imaginary plane that turned so as to remain perpendicular to his line of sight than to have attempted the much more difficult task of relating the whole scene to a single, imaginary plane that kept a fixed relationship to the scene instead of to the artist's line of sight.

The point I wish to defend is that the curvature in the painting was not merely the record of Vincent's perception of the world, but was introduced because of his painting procedures, whether they involved the physical repositioning of the canvas or only the shifting of an imaginary projection plane so as to remain perpendicular to his line of sight. I do not believe that Van Gogh thought that the room *really* looked curved; he certainly never says so. Nor is curvature present in

either drawing that Vincent did to show his brother and Gauguin what the picture looked like (one done with the painting in progress—not before it was begun, as I apparently have misled Hansen to believe by my incorrect reference to the drawing as a "study"—and one when it was completed).

Hansen takes me to task for asserting that Van Gogh had a strong sense of shape constancy. A thorough consideration of the question would consume more space than I think appropriate to this rejoinder. The issue is complicated by the fact that Van Gogh had actively, even ardently, studied perspective, to the extent of using a perspective frame at least occasionally through most of his artistic career, and therefore could deliberately anticipate and compensate for any tendency toward shape constancy by the systematic application of perspective rules or by empirical methods such as measuring the size of visual angles with one eye closed in comparison with an extended thumb or brush handle. On the other hand, certain apparently naïve qualities in his work appear, from his own remarks, to have been deliberately employed to increase his works' vividness. Hence, a perspectively correct work does not necessarily prove a low degree of shape constancy nor a work with perspective deviations prove a high one. A remark of Vincent's, written a few months before the *Bedroom* was painted, offers a reason why shape constancy might appear more frequently in works done after this time: "Even without the perspective frame, I do it now without measuring [i.e., draw], just by letting my pen go" (*Letters*, No. 500, II, 590). It seems to me that certain relationships in the painting and drawing of the *Bedroom* reproduced in my article, such as the nearly parallel receding lines of the floor (at the right in the painting and ubiquitously in the drawing) may reflect a tendency toward shape constancy that emerges here because it is no longer resisted by painstakingly checking the perspective at every point.

In a parenthetical aside Hansen suggests that Van Gogh may have sat or stood closer to point B' in my Figure 3. If I have reconstructed the ground plan of the bedroom correctly, this is not possible. As Gombrich has observed, whether perspective is accurately projected or curved, elongated, or otherwise deformed, the projection point can still be determined because the alignments and overlappings of foreground and background objects will stay the same. Conversely, from my point B' the leg farthest from the viewer of the chair at the left would be visible to the right of the nearest leg and the farthest corner of the bed would appear far to the left of the nearest corner instead of in their present positions.

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