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# Did Early Renaissance Painters Trace Optically Projected Images? The Conclusion of Independent Scientists, Art Historians and Artists

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## 8.1 Introduction

In 2000, the contemporary painter, photographer and set designer David Hockney claimed that some western artists, as early as 1420, secretly built optical projectors, projected portions of a sunlit scene or subject onto their supports (canvas, panel, ...), traced these images and later applied paint [11]. In the words of Hockney and his collaborator, thin-film physicist Charles Falco,

> "Our thesis is that certain elements in certain paintings made as early as c. 1430 were produced as a result of the artist using either concave mirrors or refractive lenses to project the images of objects illuminated by sunlight onto his board/canvas. The artist then traced some portions of the projected images, made sufficient marks to capture only the optical perspective of other portions, and altered or completely ignored yet other portions where the projections did not suit his artistic vision. As a result, these paintings are composites containing elements that are 'eyeballed' along with ones that are 'optics-based.' Further, starting at the same time, the unique look of the projected image began to exert a strong influence on the appearance of other works even where optical projections had not been directly used as an aid."

We refer to central projection claim as the *direct tracing* claim and the claimed that artists *saw* such projected images and they strove to duplicate elements of this new "optical" ideal, even without directly tracing projected images as the *indirect influence* claim. This direct tracing claim, if proven, would have great import for the history of optics and would show that projected images were recorded roughly two centuries earlier than scholars previously thought. Confirmation of the theory would also radically alter our understanding of artists' praxis in the early Renaissance.

The optical projection theory (or tracing theory) has been widely promoted in the popular media, in a BBC documentary, a website, and a number of non-expert-peer-reviewed papers by its two promoters. It has also been examined thoroughly by a wide range of international expert in the relevant domains of Computer Vision, Pattern Recognition, conservation, history of optics and art (including a four-day symposium in Ghent in 2003), as well as by a number of realist painters. Our goal here is to bring together the evidence, the arguments and the counter-arguments by independent experts, all in order to render a final judgement about the optical tracing claim, at least for the early Renaissance. Clearly, this chapter can provide but a summary of the key evidence and arguments; readers should consult the primary cited literature for full details.[1]

In Section 8.2 we describe the optical tracing theory in a bit more detail and explore its scholarly and even philosophical foundations in order to determine what aspects of the claims can—and cannot—be tested, even in principle. That is, we clarify the scope of any scholarly analysis. Then in Section 8.3 we consider the image evidence from key paintings and the arguments concerning the projection claim, both pro and con; in particular we describe alternative non-optical explanations for the "optical" evidence. We turn in Section 8.4 to the important matter of contemporary documentary evidence—or lack of evidence—in support of the projection claim and the proponents' speculation that this lack of evidence is due to artists protecting "trade secrets" or fearing the Inquisition. In Section 8.5 we examine the physical evidence and material culture of the time to see if adequate materials and knowledge about appropriate projections existed. In Section 8.6 we describe other general developments circa 1430—such as the rise in oil paints and worn spectacles—that may better explain the rise in realism in art of that time. In Section 8.7 we question whether even if artists *had* traced images in the early Renaissance whether it would have led to an increase in realism such as is found.

[1] Many of these papers are available from `www.diatrope.com/stork/TechnicalPublications.html`.

In Section 8.8 we summarize the independent scholarly consensus about the tracing theory and we conclude in Section 8.9 with some speculations on the further use of rigorous computer analysis in the study of art.

## 8.2 The Projection Theory

As mentioned above, artist David Hockney, after surveying the grand sweep of the development of western painting, claimed to have identified a newfound realism or "optical" style circa 1430 that he and his colleague, Charles Falco, attribute to some artists secretly building optical projectors, tracing projected images of sunlit tableaus or subjects on their supports (canvas, oak panel, ...), and then applying paint [11].

It is unclear which of these two claims—the *direct tracing* claim or the *indirect influence* claim—is the more important or central to Hockney, but there is no doubt that the direct tracing claim is central to the *team* of Hockney and Falco; it is also the claim that has captured the public's imagination, that appears prominently in Hockney's book, in his BBC documentary, in television stories, in public lectures and in other publications.

### 8.2.1 Philosophical and Logical Foundations of the Projection Theory

Hockney is an artist and, as such, we do not expect traditional academic rigor in his speculations or claims, but most of the subsequent work and promotion of the projection theory was done with scientist Falco, so we are justified in exploring, with traditional rigor, the intellectual foundations of their claims and evidence.

It pays to examine the philosophical and logical foundations of the claims in order to determine what kind of evidence can and cannot be brought to bear, and if the claims even rise to the status amenable to objective test. We consider first the indirect influence claim. Recall that Hockney states that artists circa 1430 *saw* optically projected images and that some artists took these as a new ideal to duplicate—even if partially, with modification—in their works. What kind of objective scholarly tests could ever prove or disprove such a claim? Consider putative contemporary documentary evidence, for instance early Renaissance writings by artists, patrons or critics. Surely if an artist wrote that he had seen projected images, and possibly delighted in them, and that he deliberately sought to duplicate some of the visual properties in his paintings, that would be strong and convincing evidence, especially if objective image analysis (somehow) corroborated this claim. As we shall see in Section 8.4, though, there is little or no such evidence for the period in question. Absent such documentary evidence, what visual or material evidence might we then evaluate, such as visual evidence in the paintings themselves? Surely the existence of optical devices (e.g., concave mirrors) of sufficiently high optical quality is a necessary—but not sufficient—pre-requisite. We shall see in Section 8.5, however, that there is little persuasive supporting evidence for such claims.

It is hard to imagine persuasive visual evidence, at least for the period in question, other than if the painting included purely "optical" features arising only in projectors, such as blur spots—such as appear in some paintings much later by Jan Vermeer (1632–1675), including *The milkmaid*. Rigorous Computer Vision and image analysis—for instance perspective, lighting, brush stroke and color analysis—would be of little or no value in testing the influence claim, at least in the early Renaissance, because there are numerous additional confounding influences, such as the introduction of new media (oil paints), social and cultural changes (secular, scientific and humanistic subjects), and so on. There are non-projection optical influences as well, such as the well-documented rise in the use of spectacles [15], which would enable artists—especially those over 30 or 40 years of age—to see both distant subjects and a nearby canvas in sharp focus. In short, there would seem to be no way to disentangle the many complex, indirect and interacting influences to prove projected images exerted some indirect influence upon artists of the time.

Moreover, whose scholarly or subjective judgements on which works or passages possess the "optical look" should be favored? How would we objectively decide between the differing and competing impressions of several scholars or artists? Indeed, the award-winning professional realist artists among the current authors (J. Collins and N. Williams) often disagree with Hockney on numerous such matters. How do we objectively test which of them is right? Such debates arise in humanistic studies seeking new interpretations, but these bear little weight in objective tests of scientific or technical matters, such as this debate over the projection theory. In short, there seems to be no satisfactory objective and scholarly answer to such questions.

For these reasons we shall not consider further the *indirect influence* claim.

The philosophical and methodological drawbacks attending the *direct tracing* claim are subtle, but no less severe than the ones just described. Note that Hockney and Falco's claim, above, was that an artist such as van Eyck and Campin would secretly build a projector and then trace some portions and freely draw others where projections "did not suit his artistic vision" and, as such, any painting would be "composits containing elements that are 'eyeballed' along with ones that are 'optics-based' ." But how can we know, *a priori*, which are which? Surely we cannot rely on a single modern artist's impressions, including Hockney's, on this matter of "artistic vision." After all, suppose other artists (such as the artists among the present authors)—or indeed art historians or scientists—have different views on that matter. Who is right? Nor can we pick and choose which features are "optical" after they have been "fit" with an optical model—as to do so would risk confirming a pre-conceived "conclusion." Such a danger of creating "just so" stories is evident elsewhere in science, particularly evolutionary theory. Arguing in such a way would make the projection "theory" technically *non-falsifiable* [29], and thus devoid of explanatory power—thus not even an acceptable theory. Ernst Gombrich, too, developed this point in the context of scientific explanations of perception in visual arts [9]. Proponents would be free to alter their claims as evidence or analyses disconfirm theory, always in order to confirm their pre-determined "conclusion." In short, such revisions would expose the fact that the projection claims do not constitute a true scholarly theory. In fact, though, such major revisions have arisen several times in the debate, except to note that, as developed in Section 8.7, the 'optical look' of such a projection has the specific character of an extremely narrow depth of field projecting most of the scene as blurred, a look that is not found in a single painting from the 14-th and 15-th centuries, which seems to constitute strong evidence against the *indirect influence* hypothesis in its explicit form.

It is important, too, to be clear about the logic of the optical hypothesis. One might imagine that the use of optical projection would imply that the perspective of the painting would be perfectly accurate, and that the occurrence of obvious deviations from accurate perspective would constitute evidence against the projection hypothesis. In fact, however, the opposite is the case, and Hockney and Falco consistently argue that the presence of perspective flaws is "proof" of its validity. Why is this the case? It is because they acknowledge that the optics of this period would use spherical lenses, and optical projection with spherical lenses is subject to spherical aberration, so that there would only be a small zone of the optical projection clear enough to be usable to guide the painter. They estimate the size of the usable zone to be 30 cm in diameter, beyond which the lens (or the canvas) would have to be moved to bring the next portion of the painting into focus. Such shifts would disrupt the continuity of the perspective between the two zones, and hence predict the occurrence of breaks in perspective in works painted by the projection method.

However, this analysis does not imply the reverse implication, that any errors in perspective are a convincing argument for the use of optical projection. Precisely such errors in perspective were almost universal in paintings before the proposed date of the introduction of the optical projection technique (1430), simply because the painters were painting by eye and did not understand the logic or the rules of perspective. Thus, finding errors in perspective after this date is most easily explained by the same logic, and carries no implication of any change in technique. The only form of perspective analysis that would constitute plausible evidence for the use of optics is if the perspective was perfect within 30 cm zones of paintings but showed errors between these zones of accuracy [62]. Not only do the theory's proponents not report any example that fits this description, they do not even perform this dual comparison in any of their analysis. They either report errors in perspective without establishing zones of full accuracy, or argue for zones of perfect accuracy without comparing them to adjacent zones. In fact, as we shall see, even in these cases the analysis was flawed, and the perspective is in fact inaccurate in all the cases that they use the argument from perfection.

In the final analysis the main evidence adduced by Hockney and Falco is the composition of the paintings themselves. As an example, consider Jan van Eyck's *Arnolfini portrait* (Figure 8.3.3, below). In their web posting for the 2001 *Art and optics* symposium at New York University, Hockney and Falco wrote: "van Eyck placed a convex mirror at the center of this [Arnolfini] masterpiece, the very mirror which, turned around, he may well have used to construct this image." Note especially that this quote refers to "this image"; indeed the painting is displayed, prominently, *in full*, on the home page of that website. (The full image also appears in Hockney's book, the cover of the journal bearing their first technical article, and indeed elsewhere.) This quote, in full context, gives no hint that the claim might refer to a teeny portion of the image, or what portion that might be. Shortly after that conference, Stork showed that the full Arnolfini image was in such poor perspective—even within putative "exposures"—that it was extremely unlikely that projections were used throughout the body of the painting. (Stork also showed that the focal length of the convex mirror differed significantly from that of the putative projection mirror, and hence the theory's proponents claim that "the very mirror may well have been used..." was false on another ground, as we review in Section 8.3.3.) Later, Hockney and Falco focused technical attention on the splendid chandelier or *lichtkroon* (Dutch, "light crown"), Hockney asserting in a high-profile television broadcast: "That chandelier is *in perfect perspective*," as it would be were it traced under optical projections. In that broadcast Hockney then demonstrated his claim by tracing the projected image of a similar chandelier—the arms *as well as the decorative structures most distant from the arms*. That is, he demonstrated that the believed van Eyck would have traced the *full* chandelier.

Hockney is not even accurate in his description of the chandelier in this painting. He argues that it was optically projected because it is "seen from head on (not from below as you would expect)." By this assertion, he means as you would expect from the composition of the painting as a whole. As pointed out by Tyler [62] in a thorough analysis of the errors throughout the original Hockney book, this claim is obviously false, since the front arms of the chandelier are clearly much higher in the painting than the rear ones, in every feature from the candles to the lowest ornaments or *crockets*. This arrangement must imply that the chandelier is seen from below. Thus, this argument for the optical projection hypothesis completely collapses on even casual examination.

Moreover, shortly thereafter, Stork and Criminisi showed rigorously that the full chandelier was not in perspective—not even close [3, 5, 31, 32]. More specifically, they showed that if the physical Arnolfini chandelier was fairly symmetric, then its image in the painting was not in perspective. In this way, they refuted Hockney's projection claim. In response, Hockney and Falco then claimed that the decorative structures or crockets were "soldered on" by hand to the base arms and hence would have been haphazardly arrayed—in short, that the *full* chandelier image was in proper perspective, just that the chandelier itself was highly irregular and asymmetric [13].

Then Stork and Criminisi used rigorous photogrammetry of a modern casting of a 15-th century chandelier as well as of large, appropriate Renaissance chandeliers and a prayer book holder, and in one case direct physical measurement with a tape measure *in situ*, all to show that all of these chandeliers were far more symmetric than is consistent with the projection claim [5]. Furthermore, experts in *dinanderie*—the decorative metalwork of the early Renaissance—pointed out that the arms on such a chandelier were fashioned from a *single* mold and then arrayed around the central staff, thus making the chandeliers highly symmetric—too symmetric to be consistent with the theory's proponents' claims. In that era, decorative structures were *never* soldered onto arms of dinanderie.

Stork and Criminisi also showed that a talented realist painter could paint highly complex chandeliers in excellent perspective—far better than did van Eyck—without any aids whatsoever, optical or mechanical. In this way they undermined experimentally Hockney's key motivation.

In summary, then, the proponents' claims about the *Arnolfini portrait* went through the following revisions, in order:

1. the *full image*—"this image"—was executed using direct tracing from the convex mirror depicted in the painting
2. just the full (assumed nearly symmetric) *chandelier image* was executed using direct tracing from some alternative and unknown (concave) mirror
3. the *full chandelier image* was executed using direct tracing from a concave mirror, but the physical chandelier was *asymmetric* (because the crockets were "soldered on" to the arms)
4. just the image of the nearly symmetric chandelier *base arms* were executed using direct tracing from a concave mirror, the (asymmetric) crockets added "by eye."

Clearly, this successive retraction of claims—a jettisoning of the claim about the depicted convex mirror, and then an overall reduction in painting area to teeny portion of their original claim—is not a principled refinement due to improved measurements or the inclusion of more data, as is typical in scientific research, but instead a wholesale revision of the key aspects of the claims as independent scholars rebut each claim in turn.

Our goal in rehearsing this history of the debate over the Arnolfini portrait is not to merely rebut the tracing claim, but instead to expose these *ad hoc*, *ex post facto* revisions to the proponents' claims as evidence and independent analysis shows that their previous claims were wrong. In short, if the projection "theory" allows such *ad hoc*, *ex post facto* alterations and major revisions to the claims under the subjective and debatable impressions concerning "artist's vision," well then the "theory" is devoid of explanatory power and not a true scholarly theory at all.

A closely related issue to the construction of "just so" stories centers on how a theory accommodates and explains truly new evidence—evidence that was not available when a claim was made, but which is surely relevant. Consider for a moment a case touted by Falco: Walter and Luis Alvarez's bold claim that a comet strike caused the extinction of dinosaurs roughly 65 million years ago. This claim received very strong support *later* when the "smoking gun" impact site beneath the shore of the Yucatan Peninsula and associated higher-than-expected concentrations of the element iridium were discovered. There remains some debate over this theory, but the relevance of this corroborating evidence is beyond question. This newly found evidence fit very well into their theory, without requiring *ad hoc* modifications.

In this regard, and by contrast, the optical projection theory fails. Consider the theory's proponents' claim that van Eyck secretly built an epidiascope—a simple projector to form the image of a flat object such as an artwork or document—to copy and enlarge his silverpoint study of Cardinal Niccolò Albergati (1431), which we shall revisit in Section 8.3.4. Stork explored a number of non-optical copying methods too, including the use of a reducing compass or proportional compass, *compasso da reduzione* or *Reductionszirkel*, known from as early as Roman times, and their relation to the fidelity and "relative shifts" in the van Eyck works [5, 30]. Later, Thomas Ketelsen and his team (which included two physicists) published their dramatic discovery of nine tiny pinprick holes along the contours of the silverpoint study. These holes are completely consistent with the use of a reducing compass, whose metal tipped legs would indeed leave such marks in the silverpoint; these pinprick holes play *no role whatsoever* in the optical projection theory. In light of their dramatic discovery, this European team concluded that van Eyck used such a reducing compass—not an epidiascope. (The "relative shifts" and scaling of the ear can also be easily explaining by the reducing compass, as we discuss below). Again, this newfound evidence fit perfectly with the *mechanical* compass explanation, refuting the optical explanation.

Given this very strong new evidence for mechanical (not optical) copying, Falco then claimed that this evidence was irrelevant to the oil copy because such holes could not be carbon dated and one could not know when or why they were placed in the silverpoint [24, quoted in]. (He also claimed, without evidence, that the nine pinprick holes were "too few" to enable an artist to achieve the fidelity found in the oil copy—a claim that is contradicted by the experimental evidence based on the work of several realist artists [5].) Our point here is to highlight the fact that rather than incorporating and explaining this newfound distinctive pinprick evidence in the optical explanation, the promoters of the projection theory were forced to retreat and claim the evidence is irrelevant in an *ad hoc*, *ex post facto* way. Nor, too, did proponents provide evidence of another copy of the silverpoint study that might bolster their additional claim that the pinprick holes were used for another copy.

Hockney's inspiration for his theory came when he marveled at the "photographic" quality in the works of French Neoclassical painter Jean Auguste Dominique Ingres (1780–1867) evident in an exhibition of this artist's works. Hockney concludes, based in large part on the quality of Ingres's lines, that Ingres used some form of optical aid, specifically a camera lucida, a simple device that allows the artist to see the subject optically superposed on his support such as paper [8]. But is optics the only technique that would yield a change in the quality of line stressed by Hockney?

The following is by the artist A. S. Hartrick, who trained in Paris under the academic master Fernand Cormon, a student of Alexandre Cabanel, Eugène Fromentin, and Jean-François Portaels. Hartrick describes a 19th-century non-optical method from French that Ingres might have used:

> Fernand Cormon, the master from whom I learnt most in my student days in Paris, had a method which I believe may prove valuable to others... Setting himself at that distance from his model which would give him approximately the same scale as that on which his figure would appear when the whole decoration was viewed at once, and also that at which he could see the whole figure of his model and of his drawing as of about the same size, he would sketch in the main movement and construction of his figure with a few bold lines, and fix the main distribution, as well as the weight of the accents. Over this drawing he then placed a sheet of tracing paper. Moving nearer the model if necessary, he next searched the character of the contour and all details of the features and extremities most thoroughly, working with pencil and modelling all up as far as they could be carried, till finally he had a finished study, usually no more than a foot high.
>
> By this means much of the freshness of a sketch was retained, because the drawing could be completed in a reasonable time before the model or the artist was tired. Afterwards these drawings were carefully squared [copied using a grid] and enlarged to any size he desired [10, pp. 72–73].

Our goal here is not to argue Ingres used the method described by Hartrick. Rather, it is to offer evidence that it is a plausible alternative in order to re-emphasize the need to consider multiple explanations, and especially the need to reject through evidence—or better yet, disprove—competing explanations. It is simply methodologically unsatisfactory to create an explanation, selecting assumptions consistent with it and ignore alternative explanations and contradictory evidence,

Consider, too, more closely the key philosophical issue of the burden of proof. The projection theory is clearly a revisionist theory, intended to overturn the standard view that the precision of the paintings was due to the talent of the artist, new media such as oil paints, etc., as the proponents themselves often stress. As such, the burden of proof for the new theory lies foursquare upon the proponents' shoulders. They cannot point to a painting, advance the claim it was executed by tracing optical projections and then demand others "disprove" their claim, of course. (This would be like claiming that the exceptional works were painted by aliens from another galaxy, then saying it must be so unless others produce evidence against such aliens.) Rather, they must show that it is far more plausible that the work was executed using optics than it was using traditional, non-optical methods, such as rulers, reducing compasses, grid constructions, "eyeballing," and so on. In the absence of such compelling evidence and reasoning, we must reject their claim. Likewise it is not sufficient for proponents to somehow "fit" the visual evidence using an optical model (especially when there are many optical degrees of freedom) or even "re-enact" or "demonstrate" that an optical procedure might *conceivably* have been used. They must also show that one cannot fit the evidence with non-optical explanation. In most of their papers, proponents do not acknowledge even the possibility of specific alternative, non-optical explanations, and in no cases do they rule out such alternative explanations, as we shall see below.

## 8.3 Image Evidence

We begin with a bit of background, then move to specific paintings, claims, and counter-claims.

### 8.3.1 Background

As shown in Section 8.4, below, a wide range of experts conclude that there is no persuasive documentary evidence that artists of the early Renaissance saw images projected onto a screen, traced them during the execution of their works, certainly nothing one would expect for a procedure claimed to have fundamentally transformed art and art praxis. Nor is there is a persuasive explanation why artists in guilds or ateliers devoted to developing and sharing technical information, such as van Eyck and Campin, would have kept this important information as a confidential "trade secret." Quite the contrary: such artists freely advertised their discoveries—or hints *about* their discoveries—in order to attract patrons and apprentices [21]. As such, then, the theory's proponents have focused on visual evidence within the paintings themselves. In this Section we summarize the image evidence and the arguments for and against the optical projection claims, organized very roughly according to their importance to the theory. This Section can be considered an updated version of, and indeed confirmation of, an earlier overview [35]. We give merely a brief summary of the evidence and arguments; interested readers should consult the original papers, as cited, for more details.

An immediate question arises about the timing of the change in style that is Hockney's chief evidence for the use of optical projection, because the putative "sudden transition" has a very fluid boundary variously attributed as occurring within decades as much as 100 years apart, pinned by time boundaries that are mutually contradictory in many cases [64]. Thus, given the pictorial evidence for the boundary as between 1423 (Fabriano) and 1436 (van Eyck), between 1438 (Pisanello) and 1553 (Moroni), in depictions of armour between 1450 (Pisanello) and 1460 (Mantegna), between 1475 (Melozzo da Forlì) and 1514 (Raphael), between 1514 (Cranach) and 1560 (Moroni) and finally between 1525 and 1595. Incidentally, in making this latter transnational comparison, Hockney somewhat implausibly defines the transition as occurring at the same time in both Northern and Southern Europe. In summary, Hockney makes no comment on the floating discrepancy in the mutually contradictory timings of his evidence. One could suppose that different artists picked up the ideas at different times from each other, but all explicit statements of the hypothesis are that it was a nearly universal transition that occurred suddenly (with subsequent evolution of the optical technologies). Rather than sort through these conflicting dates, we shall focus on the period 1430–1550.

### 8.3.2 Lorenzo Lotto, *Husband and wife* (1543)

Hockney and Falco claim their central evidence in the entire debate centers on the carpet pattern in Lorenzo Lotto's *Husband and wife* (1543) in the Hermitage Museum in St. Petersburg Russia, which they call their "Rosetta Stone," claiming that it is "simply not possible" that the painting was executed without optics, and that this visual evidence "proves" that Lotto used optics. The painting was the subject of the first technical paper on the projection theory [12], and is discussed in several of the proponents' other papers and presentations.

**Claim**

In brief, these proponents point to perspective anomalies in the pattern of the carpet which they explain by Lotto secretly building a concave-mirror projector and projecting the image of a (nearly symmetric) carpet onto his canvas. They claim he traced the pattern in three sections, refocussing his projector between these "exposures" to overcome its limited depth-of-field [12]. The proponents adjust a number of parameters in their putative projector (mirror focal length, facial area, locations, etc.) to "fit" the image evidence. Proponents also pointed to an "indistinct" or "blurry" passage at the top of the keyhole which they explain with the highly unorthodox claim that Lotto traced this passage "blurry" to reconcile the current sharp projected image there with his memory of the previously out-of-focus image there—a claim they apply to no other painting, and not even to other equivalent passages within this painting.

The proponents also point to a passage in Lotto's *Libro di spese* or personal notebook as documentary support for their claim about Lotto, as we discuss immediately below [15].

**Rebuttal**

The Hockney and Falco claim rested on their unstated and untestable assumption that the specific physical carpet in Lotto's studio was symmetric, at least to about 2%—roughly their claimed precision of fit. Stork pointed out, however, that such handmade and transported "Lotto carpets" (later named for this artist) were typically asymmetric upon creation and would have become even more asymmetric by the time they arrived in Lotto's studio [23, 34]. After all, such carpets were hand-knotted in what became present-day rural Turkey by uneducated young girls working side-by-side. After the girls tied knots for months, the carpets were then taken down from vertical looms, thus relieving months-long stresses in the weave and hence altering the shapes of the decorative patterns. Then, the carpets were rolled and transported in donkey carts hundreds of miles over dirt roads, loaded onto ships for the rough journey to Venice, then likely unrolled and displayed and moved in shops for extended periods. Indeed, such carpets surviving in museum collections are usually asymmetric—far more than is consistent with proponents' stated precision of tracing. There seems to be no way, even in principle, for anyone to prove that Lotto's *particular* carpet in his studio was symmetric to less than 2%, as would be a necessary first step in supporting the tracing claim for this painting.

If Lotto traced within an "exposure," then the perspective in the corresponding passage should be accurate or coherent to Hockney and Falco's stated precision of three significant figures. Tyler showed conclusively, however, that the perspective in the carpet deviated by more than that precision, even within a single putative "exposure" [62]. He also showed that the carpet had global coherence but local incoherence—precisely the opposite of what we would expect had the carpet been traced under multiple projections.

The Hockney and Falco claim about the origin of the "blurry" region at the top of the keyhole is unorthodox, and used in no other painting in the debate—indeed not even elsewhere in this painting where we might expect it, specifically the transition from the closest putative "exposure" to the middle "exposure." Close inspection of the painting shows that the passage was executed with a somewhat large brush. There is no visual evidence in that passage for marks indicating that Lotto traced an image—no pencil marks, no incisions, no alternative colors, no partially hidden drawings, and so forth.

Robinson and Stork levied an even more serious challenge to the projection claim for this painting. They showed that the setup in the Hockney and Falco concave mirror projector was fundamentally flawed as it did not include Lotto's 116-cm-wide canvas. When the canvas is included into their setup, however, the light from the carpet would be blocked and not reach the mirror; the full optical setup in the putative projector simply cannot work [25, 43]. Using sophisticated ray-tracing software, Robinson and Stork showed that when Lotto's canvas is included into the setup, as required, the light from the carpet is forced to strike the putative concave projection mirror at a large angle, leading to the significant off-axis aberrations of astigmatism and coma—images too blurry to reveal the fine detail in the painting. Most importantly, the blurriness is nearly the same at different distances into the tableau. The ray tracing simulations show that putative images would not have gone in and out of focus, as is central to the proponents' arguments. In short, these aberrations preclude the kinds of depth-of-field phenomena central to the optical claim: the fundamental phenomenon (refocusing to overcome limited depth-of-field) underlying the Hockney and Falco explanation simply would not occur in the setup they presented.

Finally, every technical aspect of the specific documentary passage adduced as support by Hockney and Falco in fact contradicts their image evidence and optical claims. Whereas Lotto's *Libro di spese* (personal notebook) refers to a "big" mirror, the projection proponents infer a mirror *small* indeed (diameter roughly 2.5 cm); whereas the *Libro* refers to a *breakable crystal* or glass mirror, the proponents claim instead an *unbreakable metal* mirror; whereas the *Libro* states the mirror cost an "enormous sum," the proponents' small metal mirror would have been *inexpensive*. Further, there is no textual evidence in the *Libro* to indicate the mirror was concave (or could even produce an image) as needed for a projector, rather than the much more common convex or plane mirrors. Nor is there any description of the complicated tracing procedure. Indeed, had such a mirror had such a remarkable projection capability, there is every reason to believe that Lotto would have made extensive comments about its wondrous capabilities in his private notebook, as did Giambattista della Porta about a century later when he discovered the "magical" image-projection effects of concave mirrors. Surely Lotto was not afraid of revealing trade secrets through his personal notebooks.

For these reasons, these and other independent scholars rejected the optical projection claim for this, the central evidence in the debate—Hockney and Falco's "Rosetta Stone."

### 8.3.3 Jan van Eyck, *Portrait of Giovanni Arnolfini and his wife* (1434)

The second-most important work in the debate is van Eyck's *Portrait of Giovanni Arnolfini and his wife* of 1434 in the National Gallery London, striking for its heightened realism and for appearing near one of the dates Hockney ascribed to the change in realism in Western art (Figure 8.3.3). This painting figures prominently in Hockney's book [11], appears on the home page of the New York Institute for the Humanities *Art and optics* website, and on the cover of the issue of *Optics and Photonics News* carrying the first scholarly article by the proponents [12].

**Claim**

As we saw in Section 8.2.1, Hockney and Falco have retreated and altered their claims about this work, first claiming the entire painting—"this image"—was executed by tracing an image projected by the convex mirror depicted within the work ("the very mirror"), then that just the full chandelier was traced, and finally that just the chandelier arms were traced but not the decorative structures. Although Hockney and Falco place error bars around the purported two-dimensional location of the *bobeches* or candle holders, they do not explain how they calculate those locations, what assumptions they needed to make, nor do they cite the rigorous methods of Computer Vision relevant to that claim [3]. Most importantly, they give no evidence that

**FIGURE 8.1**
Jan van Eyck, *Portrait of Giovanni Arnolfini and his wife* (1434), 82.2 × 60 cm, oil on oak panel. National Gallery London.

achieving such purported accuracy demands the use of optics—merely one of many steps needed to rule out the default non-optical claim.

**Rebuttal**

As mentioned in Section 8.2.1, above, the full Arnolfini room could not have been executed from a projection from the depicted convex mirror, turned around because: 1) the perspective within putative "exposures" is incoherent, 2) the estimated focal length of that mirror is too short, 3) there is not enough light in the room, 4) such a large mirror produces a blur spot too large to reveal the fine detail found in the painting, 5) such a hand-blown distorted mirror would produce an even blurrier image, and 6) such a convex mirror was lined witha rough, unpolished coating of molten lead or other metals, so it could not function as a mirror anyway (cf. Section 8.5).

Stork and Criminisi showed that the image of the full chandelier is not in perspective—not even close [3, 5, 31, 38, 39]. It was the fact that led Hockney and Falco to retreat from the claim "that chandelier is in perfect perspective."

If we jump over the proponents' several intermediate claims to their final, much amended claim—that just the arms of the chandelier were executed from projections—we confront a number of problems, unanswered questions, and even apparently unanswerable questions. Other artists, and of course Hockney himself on CBS *60 minutes*, feel van Eyck would have traced the decorative structures, so we must ask: why would the proponents' final claim be that van Eyck have traced the tiny, mostly hidden arms at the back of the chandelier but not the crockets that appear so prominently at the left? By what objective principle or independent evidence can we decide such matters? Further, Hockney and Falco give no evidence that an artist getting those portions in good perspective would require an artist to use projections. In fact, Stork and Criminisi showed that at least one realist artist could execute two complex chandeliers entirely "by eye" in better perspective than van Eyck. While hundreds of millions of modern people have seen photographs and television images in excellent perspective, it seems that artists trained the way we know that artists of the early Renaissance were trained can paint such an image in good perspective by eye. In the Renaissance, such artists were selected in youth according to their talents, then apprenticed to masters, and spent endless years of apprenticeship studying life drawing and copying works (without optical aids).

### 8.3.4 Jan van Eyck, *Portrait of Niccolò Albergati* (1431 & 1432)

The next works we consider are also by van Eyck: a small study portrait in silverpoint of Cardinal Niccolò Albergati from 1431 in the Kupferstich Kabinet in Dresden Germany and a larger copy in oil from 1432 in the Kunsthistorisches Museum in Vienna. Here that the proponents claim that the artist copied the silverpoint study by means of an *epidiascope*.

**Claim**

The proponents claim that van Eyck secretly used an *epidiascope*, or simple opaque projector, to copy and enlarge the silverpoint study. (The epidiascope was unknown from that era [16].) There are two classes of evidence proponents highlight. First, the fidelity of contours is high: portions of contours, suitably scaled, overlap quite accurately. Second, proponents report that for a given relative shift or offset of the images, only a *portion* of the contours have good correspondence, but if the contours are then shifted with respect to each other, then a different portion of contours overlap. To explain this "relative shift" or "relative offset" evidence, Hockney and Falco claim van Eyck traced part of the projected image and then "accidentally bumped" the projector—thereby shifting one image with respect to the other—and then continued tracing. They explicitly claim van Eyck "made a mistake" in this regard.

**Rebuttal**

The proponents claim that the fidelity found in the van Eyck works and that the evidence of relative shifts demands that an optical aid. However, experiments show that modern professional realist artists can achieve the fidelity found in the van Eyck works without using optics [5]. Moreover, the experimental evidence shows that the "relative shift" evidence can be easily explained as van Eyck merely placing and scaling the ear such as we find in the final work, presumably by eye and for purely artistic reasons. In fact, realist artists and teachers of life drawing point out that novice drawers and even some accomplished artists working rapidly, frequently place the ear "too close" to the front of the face—just as we find in van Eyck's silverpoint [30, 31, 38, 39].

There is, further, an immediate problem with the Hockney and Falco claim that van Eyck made a "mistake" by bumping his putative epidiascope. The artist would *surely* have noticed any resulting mismatch or misalignment of image contours, the projected image and the traced contours already committed to the support as well as the contours that border different traced passages. We built a simple epidiascope of the type proposed by Hockney and Falco and deliberately "bumped" the mirror to shift the alignment of the two images. The mismatch between the contours was extremely conspicuous, especially for the few moments that one of the images was *moving*. It seems inconceivable that van Eyck, working closely on an important commission, would not have noticed such a bump "mistake." Instead, it is all but certain that van Eyck deliberately shifted the position of the ear for artistic or compositional reasons.

As mentioned above, Thomas Ketelsen and his team, which included two physicists, discovered the first truly new evidence in the debate over these works, that is, evidence that was not available to proponents when they created their explanation: tiny pinprick holes along the contours in the silverpoint study [19, 20]. Such distinctive physical evidence is entirely consistent with the use of a reducing compass as the mark of a tip of the compass; such evidence plays no role whatsoever in the optical explanation. Indeed, Ketelsen and his team conclude van Eyck used a reducing compass, not a concave mirror epidiascope. Note too that the reducing compass dates from Roman times whereas the epidiascope was unknown at that time of van Eyck [16].

Falco later tried to explain away this key pinprick evidence, claiming it was irrelevant to the oil copy and stating without evidence that nine holes are "too few" for van Eyck to have achieved the fidelity found in the works [24]. However, experimental evidence from realist painters shows that excellent fidelity can be achieved "by eye," and even better fidelity with a few measurements provided by a reducing compass [5]. Furthermore, the nine holes found by Ketelsen and his team indicate a lower limit to the number of measurements made by van Eyck; it is quite possible that the artist made *many* measurements with a reducing compass by choosing different pairs of the nine holes, or only *lightly* touching the device to the silverpoint study, thus leaving no additional pinprick holes. In short, Ketelsen et al.'s mechanical explanation fits all the visual and contextual evidence better than does the Hockney and Falco epidiascope explanation.

We note in passing another work by van Eyck: his highly realistic or "optical" *Portrait of a man in a turban* of 1433 in the National Gallery London, a work widely believed to be a self portrait. As Hockney himself admits, self portraits cannot be executed by tracing optical projections and thus we can confidently conclude that van Eyck did not need to use a complex, secret optical device to attain the realism that characterizes his œuvre. There are numerous highly realistic self portraits from the Renaissance and later—from Albrecht Dürer to Carracci to Diego Velàzquez—and we can likely be confident none of them were executed by the secret use of optical projections.

### 8.3.5 Robert Campin, *The Mérode altarpiece* (1430)

The Hockney and Falco claim for the *Mérode altarpiece* is significant in that, if verified, this triptych would be the earliest recording of the image of an illuminated object projected by an optical element such as a concave mirror or converging lens—the first step toward the chemical recording of an image in photography, nearly four centuries later.

**Claim**

In brief, the projection theory proponents claim that Campin secretly built a concave mirror projector, took St. Joseph's bench and its trellis out into the sunlight, projected its image onto this panel support, and traced the trellis in "exposures," each refocusing to overcome the purported projector's limited depth-of-field. Hockney and Falco initially pointed to a single-break change in perspective between the front of the trellis and the back, which they attribute to Campin repositioning his mirror. Later, they favored a two-break explanation and pointed to tiny "kinks" in the upper-left-to-lower-right (UL-to-LR) slats, and claimed such kinks would not have arisen had Campin used a straightedge.

**Rebuttal**

In the first place, it seems quite improbable that Campin would have needed to go to the elaborate lengths of an optical projection to draw the very simple trellis consisting of crossing parallel slats. It seems that any artist of the *trecento* would know how to draw frame and connect across the diagonals to make a trellis. Moreover, Stork showed that a very simple geometrical or mechanical construction, specifically that the artist merely traced diagonal slats, could explain the change in vanishing points between the front and back of the trellis [33]. The same form of mechanical construction could explain a "three-exposure" model as well. Hockney and Falco claimed that kinks in some individual slat images were due to abutting exposures under different focus conditions, and that the kinks precluded the use of a straightedge. However, they showed only the visual evidence from the UL-to-LR slats. Kulkarni and Stork reasoned that if Campin refocused, there would likely be kinks in the *other* slats, the lower-left-to-upper-right slats (LL-to-UR), at that same depth; it would be very unlikely that orthogonal slats would just happen to be straight there. However, when Kulkarni and Stork checked those LL-to-UR slats, they found that there were no kinks at that depth of the orthogonal kinks [21]. Kulkarni and Stork also discovered that there was an alignment of kinks in the LL-to-UR slats *into a range of depths* in the scene, rather than at a single putative refocusing depth. Such an alignment is incompatible with Hockney and Falco's projection claim.

Kulkarni and Stork argued, moreover, that it would have been quite difficult for Campin to trace the dim projected slat images *between* the kinks as straight, such as we find them. Finally, they showed that all the kink evidence—angles and separations—could be explained by Campin using a subtly kinked straightedge or mahl stick, both widely used at that time. All the evidence—changes in perspective, teeny kinks—fit naturally into the mechanical explanation, where Campin would have used a slightly kinked ruler or mahl stick.

### 8.3.6 Georges de la Tour, *Christ in the carpenter's studio* (1645)

To support his projection claim about de la Tour (and Caravaggio, Section 8.3.7, below) Hockney relies on evidence of *lighting* rather than of perspective. As such, a new range of Computer Vision techniques have been brought to bear on the analysis of his claim.

**Claim**

The de la Tour painting, now in the Louvre Museum in Paris, is so realistic to Hockney that he believes that de la Tour must have secretly traced an optical projection [11, pp. 128–129]. Because the single candle depicted in the work simply cannot produce enough light (as Hockney and Falco themselves admit), Hockney claims that when de la Tour painted Christ, some very bright light source was "in place of" St. Joseph, and when St. Joseph was painted, some very bright light source was "in place of" Christ [29]. Hockney shows two "half paintings," as we could call them, illustrating his claim.

**Rebuttal**

Beyond the issue of identifying a portable light source available in 1645 bright enough to provide sufficient light for an optical projection (for which Hockney and Falco provide no suggestion), the problem of whether de la Tour traced projected images thus comes down to answering the question: where is the source of illumination in the tableau? If it is a dual source—one each "outside the picture," or "in place of the other figure"—then the evidence would be at least *consistent* with Hockney's claim. If however, it is somewhere else—for instance the candle, the location Hockney explicitly rejects—then Hockney's optical claim would fail for this work.

A number of Computer Vision methods all yield a plausible answer to this question. The simplest method is cast-shadow analysis. The single, best-defined cast shadow in the entire tableau—the shadow of St. Joseph's right hand cast onto the beam below—clearly shows the the *candle* (not Christ) is the location of the illumination. When the full set of identifiable cast shadows is used, the shadow lines overlap strongly at the position of the candle [36]. Indeed, when the cast-shadow evidence is pooled or integrated by Bayesian statistical methods, the probability is far higher in the location of the candle than in place of either the other figures [6]. Finally, a few cast shadows of woodworking tools beneath St. Joseph point to a source in place of St. Joseph—the precise *opposite* of Hockney's claim. (Such shadows are completely consistent with de la Tour painting the tableau without projections but with an assistant holding a source above those tools.)

Another, independent, Computer Vision method for inferring the location of the illumination is based on the occluding-contour algorithm, which takes as input the pattern of lightness along the outer or occluding contour of an object, such as Christ's knee, shin, and so on [24]. Stork and Johnson applied this algorithm to the figures in this painting and showed that, as before, the best single location for the light source was at the location of the candle [51].

Yet another method for inferring the position of the illuminant is based on the pattern of lightness on planar surfaces, here the floor. Stork and Kale solved the equations for the appearance model of a planar Lambertian surface (such as the floor in this painting) and used it to estimate the position of a point source consistent with the pattern of lightness found on the floor [15, 50]. Although the results here were not as definitive as in the previous cases, the results were more consistent with the light being in place of the candle than in place of the other figures, thus supporting the rebuttal of Hockney's claim.

There is other lighting evidence in the tableau that cannot be analyzed by the above methods, for instance the pattern of light on solid surfaces such as Christ's chest or St. Joseph's thigh. To exploit such information one must assume a three-dimensional model of these objects [49]. To this end, Stork and Furuichi build a full three-dimensional Computer Graphics model of the tableau and adjusted the position of a virtual light source "in place of" St. Joseph and also "in place of" the candle. They found that the setup with the light in place of the candle led to a rendering that matched the painting—lightness on surfaces, directions of cast shadows, etc.—far better than if the light was in place of St. Joseph. In this way they corroborated the other conclusions from other methods that the candle was the source of illumination.

In short, the bulk of the visual lighting evidence is inconsistent with Hockney's claim about the location of the source, and thus contradicts his tracing theory as applied to this painting.

### 8.3.7 Caravaggio, *The calling of St. Matthew* (1599–1600)

As with the de la Tour painting just described, Hockney's projection claim for this Caravaggio painting in the Contarelli Chapel in San Luigi dei Francesi in Rome, seems to be that Caravaggio traced an optical projection. Again as in the case of the de la Tour, the claim's resolution centers on whether the illumination is direct sunlight (needed for a projector) or instead a local, and hence artificial, source (and insufficient for an optical projector) [17, 20].

**Claim**

Hockney gives little visual evidence concerning this work, save for his informal impressions. He states: "Instead, there is a single light source, very strong from the right " [11]. By referring to "source," rather than explicitly "sun," Hockney seems to be inferring that the source was local (a conclusion consistent with some technical analyses, below). What Hockney apparently did not realize when he wrote that statement is that such a local source cannot provide enough illumination for a projector and thus Caravaggio would not have traced a projection.

**Rebuttal**

As in the case of the de la Tour, simple cast-shadow analysis of the shadows on the rear wall suggests that the light source is local in this painting, though this conclusion is based on reasonable (but untestable) assumptions about objects outside the frame of the picture. Model-based analysis of the pattern of light on the rear wall is not quite definitive [50, 51]. The dramatic reduction of luminance on the left of the wall is hard to reconcile with solar illumination. Sophisticated Computer Graphics modeling suggests a local illuminant for the rear wall, but a more distant source for the individual figures or that each figure was executed individually, making it relatively easy to give them all the same overall brightness.

In summary, the lighting evidence shows that the illumination may have been local and hence not compatible with Hockney's optical claim.

### 8.3.8 Hans Memling, *Flower still-life* (c. 1490)

Given his motivations for the tracing theory as an explanation for the rise in realism, it is a bit unusual that Hockney would claim this Memling work in the Thyssen-Bornemisza Museum in Madrid was executed using optics. After all, the carpet pattern is extremely simple, especially for an artist of Memling's stature and abilities and the painting surely lacks the "optical look" touted by Hockney. Why would an artist—*any* artist—employ a complicated optical system to draw such a simple pattern, one devoid of the subtleties and visual richness that motivated the projection theory?

**Claim**

Hockney's claims for *Flower still-life* follow the arguments for Campin's *Mérode altarpiece* (cf., Section 8.3.5). That is, he believes Memling built a projector based on a concave mirror or converging lens, projected the image of the front of his table onto this canvas, and traced that image. Because such an optical system would have a limited depth-of-field (range of objects acceptably in focus), the artist might have had to then refocus his projector for the back half of the carpet. In doing so, he might have tipped and moved his mirror, thereby moving the horizon line and vanishing points. In support of this explanation, Hockney shows that the central vanishing points defined by the front half of the carpet is slightly higher than that defined by the back half. He shows no other vanishing points.

**Rebuttal**

Hockney failed to test the coherence of perspective in the front half of the carpet by drawing perspective lines at an angle to the direction of view, that is, construct additional vanishing points. Stork performed that additional perspective analysis and revealed that both the front half and the back half of the carpet are not in good perspective [37]. In fact, the angular deviations from perfect perspective within each half of the carpet are roughly twice those of the *change* in angle for lines from the front and the back halves defining the central vanishing points. In short, the evidence *against* the claim the carpet was in perspective is twice as salient as the evidence that is —at best—consistent with Hockney's optical claim. As such, we must reject the optical projection claim.

### 8.3.9 Hans Holbein, *The Ambassadors* (1533)

Hockney uses Holbein's *The Ambassadors* (1533) in the National Gallery London as a compelling example of the interest in optics, at least by the early sixteenth century.

**Claim**

Hockney and Falco point to two sources of visual evidence in support of their claim that Holbein traced an optical projection in this work. First, they point to perspective anomalies in the books depicted on the lower shelf in the painting, anomalies Hockney claims show that each was traced under a different optical projection. Second, they point to the famous anamorphic skull in this painting and claim Holbein refocussed a projector to overcome its limited depth of field. They find that with careful selection of optical parameters, particularly refocussing positions, they can find a line on the skull's jaw whose shape repeats [13].

**Rebuttal**

We can easily dismiss the optical explanation for the first source of visual evidence. Perspective anomalies of the sort found in the books on the shelf appear very frequently throughout art of the time and before, including Medieval frescos (which of course could not be executed using optics). In short, perspective anomalies of this sort prove nothing whatsoever about the possible use of optics.

However, the original anamorphic projection could have been readily realized by viewing a skull in a slanted mirror and reaching out to outline the features of the skull on the mirrors slanted surface. The outlines could then be traced onto transparent paper and transferred to the painting without any understanding of the geometry of the optical projection [62]. The other interesting feature of this painting is that, despite its central feature of a sidetable displaying numerous astronomical and geometrical instruments, not a single mirror, lens or optical device is depicted. This does not give much support to the idea that Holbein was enamoured with the use of optics for the depiction of difficult objects. Surely he would have shown at least a few of the lenses and curved mirrors that Hockney and Falco supposed to have been in such vogue among the artists of this time. Hockney also claims that the globes in this painting are "marvellously accurate in their foreshortening," "perfect" and "precise," providing further evidence for Holbein's use of the optical projection method. Yet it is clear from inspection of longitude lines as they converge towards the handle that the longitude lines of the terrestrial globe are distorted, and the reconstruction of this geometry shows numerous inaccuracies consistent with brilliant painting "by eye" rather than accurate optical projection [65]. Thus the idea that Holbein was demonstrating a newfound infatuation with the use of optics with the anamorphic skull and other features of this painting becomes implausible on examination of the details of the painting.

### 8.3.10 Hans Holbein, *Georg Gisze* (1532)

Hockney mentions briefly and in passing Memling's portrait of Georg Gisze [11] in the Gemäldegalerie, Staatliche Museen, Berlin. Given the context, however, it appears that he is claiming that Memling traced projections for this painting too.

**Claim**

Hockney points to a break in the perspective line in the carpet in the work, and suggests that such a break is consistent with Holbein refocusing a mirror projector, much as in the case of Memling's *Flower still-life* (Section 8.3.8). Moreover, the coin box on the table is rendered in a different perspective than the front edge of the table, perhaps because it was executed under a projection from a different mirror position.

**Rebuttal**

Recall again that the essence of the Hockney and Falco theory is that the perspective in paintings based on optical projection should be locally consistent yet globally disrupted. A telling feature of the tapestry carpet in this painting is that, transforming the perspective as if it were viewed directly from above reveals that many of the components of the rug and the objects upon it have distorted local perspective. None of the rosettes have consistent symmetry: one is strongly rhomboidal, one is rectangular rather than square, and one has inconsistent symmetry. This, of course, means that there were in fact no consistent cues by which the transformation could be rigorously performed, but it was done to the best compromise by eye. It should be clear that many of the features have inconsistent distortions, and that the circular bases of the glass and the sand shaker are particularly distorted (again in inconsistent directions). Note that Hockney's entire analysis of the global inconsistency in this painting consists of just two white lines, the upper of which has no relation to any feature of the rug, and especially nothing that would align with the feature identified by the lower line. Thus the idea that it reveals a global inconsistency is not supportable [62]. We are arguing that all the features exhibit local inconsistency, and that the global organization is, in fact largely consistent (as indicated by the straightness of the border lines in the rug). Thus the pattern of perspective disruptions is exactly the opposite from that expected on the Hockney and Falco hypothesis, and is completely consistent with what would be expected of an artist with an excellent eye (as Holbein undoubtedly was) attempting to approximate the design of a complex object viewed in extreme perspective without the use of any mechanical aids.

Another bizarre feature of the painting is that the table has the shape of a narrow triangle (after perspective correction), rather than having rectangular sides. The possibility that this construction being the actual shape of a real table is excluded by the fact that the book at upper left would fall off the table if the far side did not have a corner.

In attributing the change in fabric depictions to optical projection, Hockney neglects both 250 years of intensive development of the artistic culture, comparable to neglecting the difference between Joshua Reynolds and the artists of today. More tellingly, he neglects the classic work of Gentile da Fabriano's *The Adoration of the Magi* (1423), which incorporates fabrics even more complex than those shown on pp. 37, 39 and 41 of ***Secret Knowledge***. Fabriano's fabrics are reproduced on p. 70 of the book, where Hockney argues that they remain "essentially flat" and are judged as non-optical. Yet the complexity of their design is just the sort of thing that Hockney is offering as evidence for the use of optical projection. Moreover, close inspection of the cape of the kneeling Magi Melchior in this painting reveals that the texture is indeed strongly folded, though not as heavily shadowed as the van Eyck painting with which it is compared. The Fabriano work thus shows that artists before the supposed "transition" could paint complex fabric patterns without optics, and tends to support the idea of a gradual evolution of the painting style for fabrics from 1300 to 1600, as opposed to the concept of a sudden stylistic change attributable to optics in as early as 1420 [62].

## 8.4 Documentary Evidence

It has been noted by the projection theory proponents, and widely by experts in history of optics and art, that there is no documentary evidence that any artist saw an image of an illuminated object projected onto a screen and traced one during the execution of any of their works during the early Renaissance. A four-day symposium and accompanying proceedings devoted to examining Hockney's theory, especially the matter of documentary evidence, unanimously rejected the tracing claim, in large part for this reason [7, 40]. As workshop organizer Christof Lüthy summarized, "With respect to the 15-th century, the idea that the Flemish Realism could be derived from the use of mirrors was roundly rejected." Likewise, "Taken together, the material, the visual and the textual evidence presented in these articles, makes the Hockney-Falco thesis extremely unlikely as far as its application for the period before the first textual reference to image projection around 1550 is concerned. The material evidence flatly contradicts the Hockney-Falco thesis, and while the textual evidence on its own cannot fully exclude the discovery of image projection, taken together with the material evidence of poor quality mirrors, the painterly use of image projection becomes extremely unlikely" [22].

The earliest scholar cited by Hockney and Falco in support of the tracing theory is the great Arab optical scientist, Ibn al-Haytham, often Latinized to Alhacen [1, 31]. A. I. Sabra, who has translated all of Ibn al-

Haytham's works, rejects any suggestion that this scientist created such projections: "And yet, as already noted by M. Nazif, there is no report in [al-Haytham's] *Optics* of a composed picture inside the dark room" [23, in, p. 54]. Surely this optical scientist, who wrote more than 100 books, did not hide some projection experiment to preserve a "trade secret," nor was this Arab fearful of Inquisition, which would nevertheless arise centuries later! Lefèvre summarizes the evidence that artists traced projected images as early as claimed by Hockney and Falco:

> "But there's a problem. There is, to date, not a single piece of direct evidence to support this [tracing] suggestion: there is not one example of a camera obscura or even a single part of one that dates from the 17th century, there are no written documents to confirm such devices were employed by artists of this time, no receipts for related materials or other unambiguous hints " [23, p. 5].

Saint Paul referred to the poor quality of Roman metal mirrors when he compared the flawed, dark view people have of this world to the clear knowledge of God that awaits them: "For now we see in a mirror dimly, but then face to face." Sara Schechner, an historian of science specializing in early instruments, has further shown that other cultural and documentary evidence reinforces what we find in analyzing the extant mirrors of antiquity through the early Renaissance—that the images were crude and dim [28].

The earliest documentary evidence to support the possibility of tracing comes well over a century after Hockney and Falco claim the procedure revolutionized art, specifically in the 1558 writings of Giambattista della Porta—the well-funded and highly connected magician and optical experimenter in Italy.

It pays to take a moment to clarify a possibly misleading reference to mirrors and projections from before the time of van Eyck. Falco has pointed to a number of passages in ***Le Roman de la Rose*** which discuss concave mirrors and the images they form. However, every one of these passages describes an image projected into space between the mirror and the viewer, rather than the far more difficult procedure of projecting an image onto a screen such as a canvas. ***Le Roman de la Rose*** bears no description of such projection—the type needed by the projection theory.

Hockney and Falco speculate that this lack of evidence was because artists sought to preserve "trade secrets" but Pamela O. Long's study shows, instead, that in the early Renaissance artisans and artists freely announced their discoveries in order to attract patrons and apprentices [21]. In the very rare cases of true secrets, such as those of the Venetian glass makers, knowledge *about* the existence of such secrets was well known. We have no credible documentary evidence *about* mirror projection "trade secrets." In short, the proponents' speculation for the lack of purported tracing procedure is not supported by expert scholarship.

## 8.5 Material Culture and Re-enactments

Hockney and Falco assert that master painters of the early 15-th century, such as Robert Campin and Jan van Eyck, used glass or metal mirrors to project images onto canvas where they could be easily traced to give lifelike detail. In particular, Hockney has pointed to the convex glass mirror in van Eyck's *Arnolfini portrait* (1434) and asserted in the *Art and optics* website, "If you were to reverse the silvering, and then turn it round, this would be all the optical equipment you would need for the meticulous and natural-looking detail in the picture." Elsewhere Hockney claims that van Eyck also used a concave glass mirror to enlarge or reduce drawings and that later artists employed good quality, flat, glass mirrors to reverse images while retaining details. Falco, for his part, has made similar arguments for small concave metal mirrors. However, inspection of surviving mirrors and related objects shows that they were too crude to offer the early Renaissance painter an optical short-cut to a naturalistic image of his subject. Moreover, there was no mention of using mirrors to project an image in medieval optical works, and no material evidence survives that could have performed the task even if artisans or scholars had thought of doing this [28].

Bronze was the most common material for ancient mirrors which were cast into a slightly convex disk and polished by hand. The principal challenge was to prevent air holes and blisters, or the oxidization of impurities or threads of unmixed metal, which would cause pockmarks, cracks, or veins in the surface of the casting. Medieval mirrors of metal were also small, dark, and convex and their reflectivity was limited by the rough casting and being hand-polished. Moreover, such mirrors were extremely rare. Concave metal mirrors were seldom mentioned outside of the context of burning mirrors. Burning mirrors had very short focal lengths and were not figured or shaped accurately enough to project an image even at that range. Note that a deformed concave mirror yields a blurry, useless image—not a deformed sharp one [8].

Progress in the manufacture of glass mirrors was very slow and stymied by the difficulties in preparing the glass, making it transparent, shaping it, and foliating it. Glass made in Europe in the 14-th and 15-th centuries was tinted dark green or brown and filled with numerous air bubbles. The "broad" technique of forming glass panes produced a thick, almost opaque, uneven sheet of glass. The reflection off its surface was very distorted and mirrors made by backing it with lead were poor. The newer "crown" technique developed around 1330 produced thin disks of glass that had deep furrows and ridges, which could not be foliated with lead to make a mirror. The striations and bubbles in glass panes formed by either technique refracted light in a very irregular manner thus yielding poor images.

Crude spheres were much easier to form than plate glass. Consequently, glass mirrors that date from the 14-th and 15-th centuries were indeed convex like those seen in the Renaissance paintings that fascinate Hockney. The glass blower gathered molten glass on the end of his blowpipe and blew a bubble. While still on the blowpipe, small, thin spheres of glass were coated inside with molten lead, tin, antimony, or a mixture of these metals. When the metal and glass cooled, the sphere was cut into pieces to form convex mirrors. The reflected image from these convex mirrors was blurry since these were far from perfect spheres. Hockney's assertion that the convex mirrors (like that depicted in the *Arnolfini portrait*) could be reversed in their frames in order to serve as concave mirrors is false. The metal-coated interior would not be smooth, polished, or shiny, nor could it stand up to polishing. No method existed to coat the outer surface of the sphere. In fact, no concave, converging glass mirrors are known from this period; there was no method to make them. Thus, Hockney's claim that van Eyck and others used concave mirrors to project images onto canvas is moot.

### 8.5.1 Re-enactments

There is also a problem with Hockney and Falco's modern re-enactments of purported early Renaissance procedures. One practical mistake that Hockney made was to assume that a modern shaving mirror has optical characteristics similar to mirrors from the past. Our cheapest mirror today is at least 1000 times better than any mirror from 500 years ago, even accounting for the well-understood effects of rusting, corrosion, and so on. We cannot expect modern qualities of reflectivity or image production from old apparatus. Historical arguments that do not take this into account are prone to error [28].

Falco sets out to make a "suitable concave mirror using only technology that would have been available in the 15-th century, with the goal of producing a 'mirror lens' of the specifications we calculated from Lotto's painting." For this he uses modern aluminum and brass stock and five grades of grinding/polishing compound. He writes, "Historians tell us that artisans were grinding glass spectacles by the 14-th century, so they certainly had abrasive compounds at that time." While artisans did have abrasives, they were not as pure as modern ones. Moreover, they did not work in aluminum, and 15-th century brass was a different alloy than modern brass. So, this purported reenactment is not using materials comparable to historical ones.

Second, Falco uses a technique of grinding two pieces of metal together to generate a matched pair of concave and convex spherical surfaces. This is a well-known modern technique used by makers of telescope mirrors. However, this technique for grinding spherical surfaces was not introduced until the 17-th century when astronomers required better telescope lenses (and later mirrors) than spectacle makers (and metal workers) were producing. Prior to that, lens makers ground their lenses in concave molds that were created by hammering copper into a rough, curved shape. They did not even use a template as a spherical control, much less a file to remove the hammer blows. As for metal mirrors, they were convex and made by casting. Two metal surfaces were not ground together in their creation. This means that lenses and mirrors were aspheric in Lotto's day and not made by Falco's method. In short, Falco's reenactment is anachronistic.

There is, further, a significant logical problem underlying Falco's "re-enactments." Even if we grant that 15-th century artisans had the "right" raw materials or tools at their disposal, there is no logical reason to conclude that they would have put these together in the same way and for the same purpose that later individuals have thought to put them together. To claim otherwise leads to silly conclusions—e.g., a claim that Aristotle could have discovered electrical current in the fourth century B.C. because he had coins of dissimilar metals, parchment, gold wire, and salt water; in short, all the ingredients of Volta's electrical pile of 1800. The progression from "could have" to "did" is even more logically suspect.

Thus, Falco's conclusion— "It is quite easy to fabricate concave mirrors of suitable focal length, diameter, and resolution for 15-th century artists to have used to project images"—is fallaciously made [32].

## 8.6 Non-Optical Contexts

We mention that it may be no coincidence that the transition to the "optical look" Hockney identifies near 1430 is the same date as the emergence of the use of oil paints. Indeed, Jan van Eyck is sometimes called the "father of modern oil painting," though oil paints were used in a few cases before him. Oil paints afford a wider range of lightness—whiter whites and blacker blacks—richer, more saturated colors, and a number of layering and glazing techniques, which reach the apotheosis in the works of Rembrandt, who would apply as many as 50 layers of oil paint in a given passage. Much of the "optical look" is due to shading, sfumato, chiaroscuro, unrelated to the accuracy of contours related to any putative tracing of projected images. Moreover, this is also the time in the well-documented rise in the use of spectacles [15]. Spectacles would allow an artist, especially one over 35 or so, to see distant subjects and close painting.

We note in passing that sculpture changed dramatically during this period as well and become far more "realistic." Consider, for instance the evolution in style from the anonymous architectural statuary in Western (Royal) Portal of Chartres Cathedral (c. 1145) to Donatello's *David* (c. 1440) to Michelangelo's *Pietà* (1499). This remarkable rise in sculptural realism and expressiveness was, of course, unrelated to any development of technical tools analogous to those in the optical claim.

## 8.7 The "Value" in Tracing

Although the above discussion centered on the tracing claim, we must not forget the full extent of Hockney and Falco's claim: that tracing itself helped lead to the heightened realism or "optical look" of the *ars nova*.

Tracing surely aids in capturing contours, of course, but it does not help in capturing subtleties in color, shading, and tone. The sight of a full-color projected image might aid an artist, but the contours alone are much like a child's color book. It is extremely difficult to *paint* directly under projections (as Hockney himself admits), and it would impede rather than aid rendering of color.

As Tyler [62] points out, one important issue is the true nature of the "optical look" that supposedly inspired the change in painting style in the 1420s. In the book, Hockney shows examples of the true "optical look" that is obtained by projection of the still-life scene of a bowl of fruit through an optical lens of about 15 cm diameter. With this large lens made to modern standards, the scene has a depth of field of only an inch or so of sharp focus. All the rest of the objects are heavily blurred. Hockney argues explicitly that it is precisely the characteristics of the projection of the optical image, including out-of-focus regions, that should have appeared in paintings at the time that optical projection first came into play.

In contrast, the large-scale van Eyck paintings that supposedly represent the style inspired by the "optical look" are almost preternaturally sharp throughout the scene. This property of ubiquitous clarity had, in fact, been characteristic of paintings since Greek and Roman times. It was nothing new. Conversely, as one can see from the projected image of still life [11, p. 104], the true "optical look" is extremely fuzzy, and would have been more likely to have inspired French Impressionism than the Renaissance precision. In fact, there have been compelling suggestions that the looseness of the late paintings of Impressionists such as Claude Monet was due to the reduced optical quality of their own eyes over time [61]. Thus, the look of the paintings is essentially the opposite of what would be predicted from Hockney's "optical look" hypothesis [62].

## 8.8 Scholarly Consensus

As far as we know from published scholarly works (rather than websites, blogs, letters to the editor, YouTube videos, and so on), the independent scholarly consensus—indeed unanimous consensus—is to reject the Hockney direct tracing claim. Nearly a dozen scientists or technologists, eight historians of optics and art, and two curators have published scholarly works rejecting the theory, at least for the works in question. Participants in the four-day workshop devoted to testing the tracing theory unanimously, and in no uncertain terms, rejected its claims [40].

Consider, too, the more informal literature of book reviews. A few early reviews of ***Secret knowledge*** expressed intrigue with the bold tracing theory, but the vast majority—especially those by experts in art of the period—were strongly critical. We are aware of but a single book review to date (by an English professor) that finds Hockney's evidence and arguments persuasive [60]. Even Hockney's long-time collaborator and broad contributor to Hockney's book, art historian Martin Kemp of Oxford University, recently acknowledged his skepticism about the central claim of the optical projection theory:

> "My own view is that Campin and van Eyck may well have been inspired by optically generated images—the camera obscura was well known to mediaeval natural philosophers—but *probably did not actually use them directly at any stage in the making of their pictures*" [18].

In short, Kemp too is skeptical about the central and explicit claim of the Hockney theory: that the "optical look" arose in western art circa 1430 because some artists traced optically projected images.

## 8.9 Conclusions

We have examined the claim that some European artists secretly traced optically projected images during the execution of passages in some of their works as early as circa 1430 and that such a procedure was key to the rise of a newfound realistic, photographic or "optical" look in the *ars nova* or new art of that time. We find that the theory itself, as stated, relies on subjective and ultimately untestable premises about which portions of an image an artist would or would not have traced. The theory's proponents have exploited this lack of a theoretical foundation to alter and retreat from claims in an *ex post facto* and *ad hoc* way. Further, the theory's proponents rarely explore in adequate depth alternative non-optical explanations for the visual evidence in paintings. We analyze the visual evidence in key paintings adduced in support of the tracing theory and find, without exception, that alternative, non-optical explanations are as plausible—and indeed generally far more plausible—than the optical explanations, especially in light of independent physical evidence and constraints. We also review briefly the documentary record for the period in question (c. 1430–1550) and find no persuasive evidence to support the direct artistic use of such projections. We examine, and ultimately reject, the speculation that this lack of documentary evidence was due to artists protecting "trade secrets" or fearing the Inquisition.

It is clear that the overwhelming—and to our knowledge unanimous—conclusion of independent scholars writing on this subject is to reject the Hockney direct tracing theory, at least for the period in question (1430–1550). Moreover it has not been demonstrated that tracing was needed for this rise, or in fact helped it at all. Of course, every scholar should be, and to our knowledge indeed is, open to new evidence that may arise, and no rebutter is so irresponsible as to have claimed to have "disproven" the tracing claim. Instead, we—and rebutters more generally—claim merely to have rebutted every aspect of Hockney and Falco's frequent claims to have "proven" their direct tracing claim, at least in the early Renaissance.

Despite this broad scholarly rejection of the direct tracing claim, we reiterate that we do not take a stand—for or against—Hockney's alternative claim of artistic influence, i.e., that some artists *saw* and were indirectly *influenced* by projected images. None of the technical analysis, such as referenced in Section 8.3 shed much light on the influence claim, though the lack of supporting contemporary documentary evidence in the early Renaissance and the issues of burden of proof for a revisionist theory argue against the indirect influence claim.

Although Computer Vision, Pattern Recognition and image analysis long predate Hockney's speculations, his theory has motivated the development of a number of algorithms in particular, and the general acceptance of computer methods in the study of art. We feel that this, then, may be Hockney's most important legacy in this general domain. A number of scholars have moved past the tracing claims to address a wider range of questions in the history of art, research that is leading to new techniques and shedding new light on art and art praxis [41, 42, 44–48, 50, 52].

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