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Authenticity and the Scientific Method

Past Approaches, Present Problems and Future Promise

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The use of scientific techniques to unmask forgeries and fakes of paintings is much vaunted and has a high public profile. Since the earliest applications of X-radiography and pigment analysis in such renowned cases as the Wacker Van Gogh forgeries of the 1930s and the Van Meegeren Vermeers, science has seemingly held a key role in the popular imagination, the expert in analysis unmasking the master faker. There is though by now a degree of maturity to the field, with significant demands for such analysis ranging from due diligence questions during acquisition in the public and private sectors, through resolution of legal disputes, to 'aspirational' owners seeking to validate their discoveries. Nevertheless there is no generally accepted methodological approach or universally agreed set of benchmarks for carrying out such studies, with the lack of such agreed parameters potentially threatening to undermine what we do. This paper explores some of the key background history, the currently prevalent methodology, some associated problems and possible solutions presented by the use of science in authenticity studies of paintings

1. Introduction

In June 1927 the Burlington Magazine published a letter from A.P. Laurie. The critical focus of this letter was a review by Roger Fry of an exhibition that had appeared in a previous edition. Laurie was exercised by Fry's apparently straying into what he clearly felt was his own territory:

"Mr Fry having abandoned his claims as an art critic and based the question of authenticity on the examination of the surface of the picture, is it evident that the right person to consult is the chemist?" [Laurie 1927]

Fry's response goes unrecorded, but the Editor of the Burlington Magazine nonetheless added his own comment:

"Does not one's past experience of the scientist's decisions force us to accept [this] with some degree of hesitation…the question which many of our readers will be inclined to ask is, 'How reliable are the tests on which Professor Laurie is prepared to rely?'" [Tatlock 1927]

While we might initially read this as yet another, minor, example of the interminable 'Two Cultures' debate of C.P. Snow, in fact closer examination reveals a much richer context. This exchange, and the events surrounding it, is symptomatic of the emergence of a 'scientific' approach to questions of authenticity. If we were to look for origins of such scientific rationalisation, then the 1920s were the point at which methods and approaches coalesced into a coherent discipline.

Arthur Pilans Laurie (1861-1949) remains well known within this community for his book *The Painter's Methods and Materials*, still in print some 80-odd years since it first appeared just before Laurie was writing so acerbically to the *Burlington*. Laurie was a chemist and principal of Heriot-Watt College in Edinburgh who had reputedly been encouraged into the analysis of paintings by the artist William

Holman Hunt (1827-1910). Laurie attended the University of Edinburgh, then King's College, Cambridge, from where he graduated with first class honours in the science tripos of 1884. Laurie's subsequent career saw him split his time between academic work in chemistry, roles in government committees and book writing. His academic work included a lecturer's position in chemistry and physics at the St Mary's Hospital Medical School in London, the chair in chemistry at the Royal Academy of Arts and, latterly, an advisory post at the Courtauld Institute of Art. The crowning achievement of his career was probably his appointment as principal of Heriot-Watt College, Edinburgh, in 1900 though, of course, his most enduring legacy must be his studies of the materials and techniques of art (*Who's Who 1938* 1937; Forbes 1949-1950; NAHTSE).

Roger Fry (1866-1934) is perhaps best known today for his invention of the term 'Post-Impressionism', a somewhat salacious private life and (maybe) his own painting, but he was also a noted art historian and critic of his time, firstly of the Italian Renaissance, later of contemporary movements in French art. What makes Fry an especially interesting figure in our present context however is that his first grounding was as a scientist – he read Natural Sciences, like Laurie at King's College, University of Cambridge. He was drawn though to art during his undergraduate years, turning to this as his principal field of study immediately after (Woolf 1940; Spalding 1980; Bruneau). Such early training must nonetheless have remained with Fry throughout his life because we find him commenting not long before his death that:

"If ever there was a study…needing as it does the co-operation of so many sciences…it is surely that of Art-history, and I would make the claim that the benefits it would confer would be at least equal to those it would receive. […] We have such a crying need for systematic study in which scientific methods will be followed wherever possible."
[Fry 1939, 3]

In fact a notable feature of Fry's way of looking at art was his focus on the object and things internal to it, as opposed, say, to those strands in art history in the late nineteenth century where documentary sources were paramount. It is not surprising to find for example Fry taking Giovanni Morelli's archly analytical study of Italian painters with him on an early trip to Italy (Spalding 1980, 49; Morelli 1893; Sorensen). Fry had also studied 'Old Master' techniques, using them in his paintings and advising others on their application. He was even known to dabble as a restorer, notably while he was on the staff of the Metropolitan Museum in New York and later, on the much-damaged Mantegna *Triumphs of Caesar* at Hampton Court Palace in England.

That Laurie was taking Fry to task for straying from art to science might consequently start to look a little peculiar, with an almost dogmatic insistence on separation of the art historian from the scientist. In fact Laurie and Fry had had contact beforehand, both sitting as panel members of a 'committee of experts' in one of the most celebrated cases of authenticity of the time, that of the so-called Hahn *Belle Ferronière*.

The Hahn *Belle Ferronière* is a version of the work by Leonardo da Vinci in the Louvre and we can summarise the case as follows: Harry Hahn, an American, apparently acquired the painting in France. However, in 1920, just as Hahn was about to sell the painting to the Kansas City Art Institute, the renowned London dealer Sir Joseph Duveen denounced the Hahn *Belle Ferronière* as a fake. Hahn promptly sued Duveen for half a million dollars. Duveen, in a countermove, used all his influence to set up a confrontation between the two paintings, establishing a panel of 10 experts to pronounce judgement. The panel, selected by Duveen, included the renowned art historian Bernard Berenson, various leading museum directors, Roger Fry and, as a late and self-offered addition, A.P. Laurie. On viewing the Hahn and Louvre versions together these experts largely dismissed the Hahn version. Nonetheless, during the following six years Hahn's lawyers systematically undermined the expert testimony in the trial, such that by 1929 Duveen felt it necessary to settle out of court with Hahn.

As John Brewer has recently written of this case:

"Harry Hahn contrasted 'the air-spun conjectures, subjective guessings, sixth-sense flairs, and, in certain instances, downright dishonesty produced by members of the Duveen clan' with 'reliable historical documentation' founded on the 'objective and scientific nature of accurate historical research.' […] The case of Hahn versus Duveen not only raised questions about the authenticity of a particular (and potentially extraordinarily valuable) picture, it raised, in stark terms, the question of whether the 20th-century art world was to be governed by the aesthetic opinions of a selfanointed elite of connoisseurs, or by the rigorous strictures of modern science. Duveen set out to vindicate not only his condemnation of the Hahn picture, but the entire system of attribution and connoisseurship on which his hugely profitable business depended." [Brewer 2005]

This background makes Laurie's attack on Fry's methodology all the more remarkable. Clearly they had both been present during discussions on the Belle Ferronière case four years earlier and, moreover, the trial was ongoing,

dragging through the American courts. That Laurie would choose this particular moment (1927) to launch a public attack on Fry, in a journal the latter had founded, is consequently at first sight surprising. In fact Laurie's muscle flexing should instead be seen as more of a reflection of the strength of analysis of paintings at that time – Laurie would probably have felt that at no point previously was there such a promising set of scientific tools and methods with which to tackle questions of attribution and authenticity. If we look at the range of techniques developed by and through the 1920s, it forms a template for much practice since.

2. The Disciplinary Matrix

It is always an instructive exercise in any field to reflect upon the features that define it. Who is in the community? What are our common practices? How do we describe our field to others? A helpful framework in these circumstances is what is known as a 'disciplinary matrix', largely popularised through the philosopher of science Thomas S. Kuhn (Kuhn 1970). This is the set of features – the theories, assumptions, methods and case studies – that we would all largely agree upon as constituting the basic knowledge of the field. To understand what Laurie must have perceived as occurring we must examine what features we (actually or instinctively) recognize as being present in the field.

Although we are focusing on the 1920s we should not lose sight of the fact that the use of scientific techniques to analyse paintings was, even then, by no means new. Nadolny, for example, has traced the use of simple methods of investigation on historical paintings as far back as the late eighteenth century, with relatively sophisticated studies certainly being employed by the early nineteenth century (Nadolny 2003). Important figures such as Mrs. Merrifield and Charles Lock Eastlake researching historical materials and techniques in the mid-nineteenth century, and A.H. Church, the chemist, writer on artists' materials and first professor of chemistry at the Royal Academy in London (thus standing as a direct precursor to Laurie) laid foundations in critical areas for the subsequent development of this field during the later nineteenth and early twentieth centuries. Use of X-rays for the examination of paintings occurred not long after the discovery of the X-rays themselves – in fact in 1896, less than a year after Roentgen announced his findings (Bridgeman 1964) – while all the basic elements of the approaches practiced today were certainly in place by the 1930s. A group of names stands out from this latter period (including but certainly not limited to) such luminaries of the field as A.M. de Wild in Holland, Kurt Wehlte in Germany and, of course, A.P. Laurie in Britain. Edward Forbes, renowned himself in the annals of this field as well as for his pigment collection, writing Laurie's obituary in 1949, explicitly credited him with being a pioneer of "… research into the methods and material (sic) of the old masters by chemical study as well as the use of the X-ray, ultra violet and infra red rays, microscopic examination, micro photographs, and the perusal of the early documents which bore on these studies" (Forbes 1949-1950). While Laurie did not establish this field single-handedly, Forbes' summary nonetheless serves as a useful reference point for the essential methodological toolkit for the scientist investigating art. It can be summarized as follows:

- General examination by imaging techniques such as X-radiography, infrared imaging, and Ultra Violet fluorescence.
- Detailed surface observations of the painting using low-power microscopy.
- Analysis of the constituent materials by chemical testing.
- Examination of 'technique' – the way in which these materials were used as well as aspects such as what may be loosely termed 'brushwork'.
- Comparative study of historical documents for information on materials stated to have been used by artists in the past.

It could plausibly be argued at this point that all modern scientific analysis of paintings derives from this basic pattern and, while more advanced analytical instruments such as the electron microscope and the gas chromatograph have been developed, in essence the same common threads of method are still being followed.

At the same time we should also be aware that this toolkit encompasses more than just the physical processes required – the sampling and direct analysis of samples – it also encapsulates a philosophical approach (which we will discuss shortly) as well as a rôle, for example, for historical studies and comparative analysis. It also contains a specific conception of what science 'is' in this context: that of the analytical chemist, this being quite distinct from, say, the process of the art historian.

To complete our disciplinary matrix of scientific methods for authenticity we need to add two other components, the socalled 'paradigm' cases the field cites, and a philosophy of science argument that allows us to 'test' for authenticity.

Kuhn's narrower meaning of paradigm – case studies

presented as exemplars – allows us to highlight that, repeatedly, certain cases come up when people discuss science and authenticity, features that we want to emphasise as discipline markers. Apart from the Belle Ferronière, we might therefore cite the Otto Wacker Van Gogh case and, of course, Van Meegeren and his forged Vermeers and de Hoochs.

The elements of the Wacker case can be briefly outlined. Otto Wacker, a dancer, took up dealing in art in 1925 and quickly developed a 'sound' reputation with Van Gogh experts. Such was his renown that the 1928 De la Faille Catalogue Raisonné of Van Gogh listed some 33 paintings from Wacker. Four of these, however, were subsequently included in an exhibition where they were recognised as fakes, thereby casting doubt on the entire group. Legal proceedings were instituted against Wacker in December 1928 and, when it came to trial in 1932, there was expert testimony from two technical specialists. These were A.M. de Wild and Kurt Wehlte, both names still remembered. De Wild reported the presence of 'resin' in the paint which was 'not used by Van Gogh', while Wehlte presented comparison X-rays to show differences between real and fake Van Goghs.

Second, the notorious Van Meegeren case has of course been discussed extensively before, but we may again summarise (Coremans 1949; Dutton). During the 1930s the art historian Bredius authenticated the painting known as the Supper at Emmaus. In the late 1940s, however, to avoid the more serious charge of wartime collaboration, Van Meegeren confessed to having created this and other paintings. Analysis by Paul Coremans at this time showed anomalous features were present, such as that the natural ultramarine Van Meegeren had used was actually contaminated with cobalt blue, a pigment unavailable to Vermeer and his contemporaries, while cracks sometimes did not pass through upper paint layers.

As stated earlier, such paradigm cases are important as exemplars used by a field to define its scope, practices and theoretical framework. Each of these cases therefore illustrates the role of science in the authentication process and they are commonly cited for just such purposes. Importantly, however, the contemporary field view of them is not necessarily exactly what happened and there is commonly simplification, selectivity and inaccuracy in the retelling. For example, in the Van Meegeren case it was not until the period 1967-1973 that researchers such as Keisch used lead 210 decay and isotope analysis to show that the lead white was modern (Keisch 1968) and Breek and Froentjes identified the paint medium as a phenol formaldehyde resin (Breek and Froentjes 1975). For the purposes of promoting the field, its contribution is also 'played up' – while scientists participated in these particular cases, it is not so clear that their testimony necessarily brought about the specific legal conclusion. (With Van Meegeren it was instead probably his confession and recreation of a painting for the court.)

Our other component of the disciplinary matrix was a specific 'authenticity' test, a criterion for deciding when something is or is not genuine. When a philosophical basis for the methodology of 'analytical authenticity' is explicitly mentioned, it is invariably that of falsification, a concept adapted from the philosopher Sir Karl Popper that has become embedded in day-to-day consciousness, especially amongst the 'hard' scientific community to whom it was addressed (Popper 1934).[1] Briefly expressed we may state Popper's position as being that no number of positive outcomes at the level of experimental testing can confirm a scientific theory, but that a single genuine counter-instance is logically decisive in rejecting it. Re-phrasing, if we have a theory about something – the existence of the atom, planetary motion – then however many times we check it against some observation and get a 'positive' result, we are only not disproving it; on the other hand any contradictory result should cause us to abandon our theory. Crucial to Popper's method, any truly scientific theory is required to make testable claims, otherwise (according to Popper) the theory is 'non-scientific.'

Translating this into our present context, then according to the Popperian conception of analytical authenticity scientists take a hypothesis, in this case the claim that a painting P is by a specific artist X. This claim is then tested by taking samples and analysing them, declaring if there is any counter-evidence, such as anachronisms or features alien to artist X, that the hypothesis that the painting is by X, is false. Thus we have a very clear statement of methodology.

In this way we have a set of protocols that allow us to use scientific methods to determine authenticity. Analytical tools such as X-rays and pigment analysis are combined with knowledge of what is and is not appropriate to find historically through an unambiguous test with sound philosophical foundations.

3. Problems …

The disciplinary matrix just outlined provides a clearly established methodological approach to questions of scientific authenticity. Since the time of A.P. Laurie and others in the 1920s and 30s, successive generations have seemingly built on this, giving us an ever deepening

knowledge of artists and their materials as well as new analytical solutions to problems of identification (such as non-invasive methods, advanced organic analysis and new imaging techniques) that mean that we can go further and deeper than ever before. There are well-demarcated spheres of expertise – art historian, conservator and scientist – working together symbiotically to solve problems. There is also an increasing awareness and use, in both institutional and commercial arenas, of the contributions of scientific analysis.

Or is this really a true picture of the discipline? A counter-view might run:

In fact there has been little critical examination of the procedures for decades. The apparent stability of methods applied actually hides a lack of rigorous assessment of the protocols used or whether they truly tell us what we want to know. Accurate and reproducible results are held back by a lack of knowledge, access to equipment, and reliable sources of reference data. Interdisciplinarity is often a fiction, with ultimate judgement deferred to non-scientific dogmas, a process that technical specialists connive with. Nor has there been any serious research into either how the overall and detailed process functions or how we can really do it better.

The truth probably lies somewhere in between, but if we want to properly disentangle the state of the discipline, then we need to examine such questions as:

- How well defined and reliable actually is our methodology?
- What are the questions really being asked?
- Who is defining these questions and what impact does this have on the processes we use and the conclusions we reach?
- What are the unstated assumptions we use and what impact do these have?
- Are we working within a system that fails to let us answer legitimate questions?
- Are there unstated methodologies that we use without justification?
- What is the effect of pragmatic constraints, such as time, money, and confidentiality?
- What is the nature of any interdisciplinary process?

In partial answer we should perhaps first make some clarification of what is done when practising 'scientific authenticity'. For example, we generally grasp that there is a distinction between the process of determining authenticity and that of making an attribution. The application of science to the determination of authenticity is widely practiced and generally accepted as a legitimate and useful process. It involves the application of analytical methods to uncover contradictory evidence for what something is believed to be. Attribution on the other hand is both pro-active and controversial. It involves taking a painting without prior assumptions and, through analytical means, determining authorship. Few scientists currently make such claims, and those who do tend to be those using mathematical analyses of images rather than chemical analyses of materials, these being identifiable and separate sub-disciplines.

To authenticity and attribution studies a third process should in practice be added: the determination of date. While clearly connected to issues of authenticity and attribution it can nonetheless be a process carried out quite independently of any specific authorial question. Through use of direct dating techniques like radiocarbon or dendrochronology, or by applying a detailed knowledge of changing patterns of use of materials, it is possible to give estimates of when something was created. Such evidence can therefore be used to underpin a more specific belief, such as that of authorship. Moreover the creation of a date specific test that can be used for our falsification process – explicitly that of anachronism – has been much easier than, say, finding a material or technique always or never used by an artist. An assessment of date is thus the most oft-stated approach to scientific authenticity.

At the expense of discussing some of the broad issues mentioned above (and some that were not) we will instead take Popperian falsification as an exemplar of the problems that exist. Falsification theory has a number of well recognized problems, such as the so-called underdetermination problem and the Quine-Duhem problem. There is also a failing specific to the application of analysis to falsification of art works, which will be called here the 'hypothesis test' problem. From this we shall see that it is possible to critically examine what we do, identify specific difficulties that we may or may not be aware of, and find solutions where necessary.

The underdetermination problem concerns the making of unwitting assumptions by choosing one hypothesis to test rather than another. Simply put, for every theory imaginable, another, contradictory, one may also be conceived that would explain the same facts just as well. So, we might ask, why this hypothesis and not another? For example, why choose 'This is a painting by X painted in 1626' over 'This is a painting by somebody who liked X in the 19th century and wanted to emulate him,' or even 'This is a disinformation exercise perpetrated by a secret committee determined to bring down the art establishment'? Who decides which

'reasonable' hypotheses (whatever 'reasonable' means) should be considered? Significant argument often seems to rest on conflicting ideas of what people think the (sole) hypothesis should be rather than, say, determining an appropriate set of hypotheses and then testing these side by side.

Another difficulty with Popper, known as the Quine-Duhem Problem, illustrates the extent of care needed in applying scientific methods to questions of authenticity. The Quine-Duhem problem essentially states that there are inevitably multiple components to any hypothesis. For example, when testing the hypothesis that 'This painting is by X,' a range of other aspects are also being tested, including the fundamentals of analytical methods ('elements are identifiable on the basis of their characteristic X-ray emissions'), the reliability of comparative information (from 'this is how X painted' to 'these were the pigments available to X'), investigative skills ('this paint sample comes from an original area of paint and not later restoration') and so forth. If the test 'fails,' it could be due to any one of these parts. Philosophy of science has several strong responses to this problem, mainly aimed at determining the analyst's confidence in each element of these component hypotheses, but these have not commonly been appealed to in our field.[2] Usually (in this author's experience at least) the communities involved in these decisions accept the reliability of analytical science and tend to blame either the analyst or the presence of restoration. These are of course actually informal, tacit, specifications of component hypothesis likelihoods. The true challenge however is to ascertain what these component hypotheses really are and then establish objective measures of reliability for each, something that requires a profound knowledge of the domain(s). Moreover, the use of ad hominem arguments for example – those that seek to dismiss an argument on the basis of who is making the argument rather than the merits of the argument itself, such as referring to what institution they do or don't belong to – is simply unacceptable.

Lastly, as we saw earlier, a fundamental aspect of Popperian falsification is the provision of a 'test,' the experiment that is applied to a hypothesis in an attempt to falsify a prediction of it. Our specific exemplar is anachronism, since it is the most easily defined and widely used of these tests. Commonly the 'anachronism test' is expressed through the concept of terminal dates, the dates before and after which a pigment or other material was not available. Such data have been collated in a number of forms, such as the important pioneering paper by Kühn (Kühn 1973), though this author has also not infrequently seen use of supposed lists of 'terminal dates' that are actually nothing of a sort.

In practice these might better be called 'pseudo-termini,' as they use what initially appears to be a clear statement of cut-off points, but which, in practice, are not. A good example is patent dates, which might at first seem to present strong termini – dates before which they had not yet been invented but after which they were a product on the open market. Whereas one could expect the flaw to be that pigments were available to be used at an unknown date earlier than the patent date, in fact the converse will be argued here (Eastaugh 2006). A good illustration is the case of the phthalocyanine pigments. Phthalocyanines form a highly important class of organic pigments of twentieth century introduction that provide stable and strongly chromatic blues and greens used for everything from paint to the coating on CD-ROMs. An accidental discovery apparently took place in 1928 at the Grangemouth works of Scottish Dyes Ltd., which directly led to the recognition and development of phthalocyanines; the first patent, British Patent 322,169, was to Dandridge, Drescher and Thomas of Scottish Dyes in 1929. Detailed studies of the chemistry and commercialization of the manufacturing processes took place in the earlier 1930s, with formal public announcement in the press in 1935. According to most histories of the discovery of these compounds, however, there were plausibly two prior reported syntheses of phthalocyanines, first Braun and Tcherniac in 1907 at the South Metropolitan Gas Co. in London, and then Diesbach and von der Weid during 1927 at the University of Fribourg. But neither group recognized their discoveries as the potential commercial success phthalocyanines they would become. (Eastaugh et al. 2004)

So, what is the terminal date? A hard line approach would surely argue for the earliest demonstrable identification as the terminus, which would place the date for phthalocyanines at 1907. Others might argue that it has to be the first explicit discovery and characterisation, in 1928. Then again, why not argue for the full-scale commercialization, sometime in the mid-1930s?

In fact if one examines the history of painting materials one finds a strikingly similar situation in every case. Even if it is believed that there must be a 'magic moment' of discovery (such as supposedly happened with Prussian blue) there is often not only uncertainty as to precisely when this 'magic moment' took place, but also questions about how long it took for the discovery to have any practical impact. This phenomenon is well understood, though not apparently so in painting analysis circles, even if the better review articles implicitly demonstrate it (Laver 1997; Keijzer 2002). So-called 'diffusion of innovation' studies, a field with a surprisingly long history of which the chief modern exponent has been Everett Rogers in his classic *Diffusion of Innovation* (Rogers 1962), provide a fuller understanding

of this idea. Inventions and their impact on society follow a distinct and almost invariable pattern. A small group known as 'innovators' are the primary discoverers of things, followed by a larger group of 'early adopters,' followed by the main 'early majority,' the similarly large 'late majority' and then the 'laggards.' The theory provides a useful model of how things actually come to be used, few at the beginning, with a cumulative uptake over time, and all those who are going to use it by the end.

Unfortunately this view fundamentally undermines our use of Popper and his falsification method since we no longer have a clear test; there is no obvious cut-off point with which to define any 'before' and 'after', leading us into a falsificationist nightmare.

4. … And Solutions

It would be unfortunate if we were forced to abandon the use of scientific methods in studies of authenticity, especially as (apart from putting us out of a job!) it seems intuitively clear that there is a role for such processes. How, therefore, can we resolve this situation? In practice there seem to be a number of strategies, of which we will look at two. The first to be described here is based on a revision of how we apply and use chronologies; the second is a brief exploration of how we might approach formalising comparative analysis.

As a concept, diffusion of innovation helps us towards our first solution. We find that we can deconstruct the terminal date problem such that instead of giving a dichotomous solution it provides us with a measure of 'reasonableness' across the period of introduction. In the case of phthalocyanines discussed earlier, the 'discoveries' that took place prior to the grant of patent in 1929 were made by the group we call 'innovators'; then, once the managers at Scottish Dyes Ltd. were persuaded of the significance of the discovery and they moved towards commercialisation we see the 'early adopters' arrive, and so forth. More importantly we can see that there is a) no sharp cut-off representing a terminal date and b) a pattern of increasing use over time that represents the gross uptake of the innovation. This can be usefully re-expressed here as the likelihood of finding a product in use at any particular chronological period. Imagine that something is invented and someone wants to monitor how widely it is being used. As time passes, surveys should indicate that increasingly more people are using the invention. Similarly, the analysis of paintings will confirm that as time elapses from the date of introduction of a new pigment, more and more paintings will be found to include that pigment. Typically the same diffusion of innovation pattern will be manifest in all new introductions, including not just different materials such as supports and paint media, but also techniques – comparable patterns can be found in such features as red/grey double grounds popular in the seventeenth century for example.

When applied to paintings, the core implication of this is that it is far more likely that we will encounter a material in a painting when that material is available in abundance. Conversely, it is extremely unlikely that the pigment will be encountered when it had just been discovered, or even for some period of time after it is initially introduced commercially. The probability is that one has to wait until well after, say, a patent, to see the invention widely employed. Such probabilistic judgements are used widely and reliably in life in general and can also be used in matters of authenticity. I would be far more sceptical of a painting for example if it required me to believe in the earliest known use of a material or technique than if it was just a run-of-the-mill occurrence. Exclusive, yes-or-no judgements respond poorly to real-life situations whereas formalised 'is-this-reasonable?' type statements based on sound knowledge and methods of analysis are both informative and useful.[3] A 'likelihoodist' solution therefore provides us with a means of specifying, quite precisely, whether a proposition such as 'Liubov Popova used titanium dioxide white in the 1910s' is reasonable or not (it is not, the likelihood being close to zero).

In fact the approach is richly productive, as it opens up a range of other practical steps. In particular it encourages a far more complex view of the history of materials. If we can escape from the notion that we have to find clearly unambiguous situations, like Rembrandt and synthetic ultramarine say, and use marginal probabilities, then we can make use of information on many aspects of pigment history. A simple (and simplified) case might be Prussian blue: in its early life it was made from crude starting materials such as blood, but was later synthesised from purer chemicals. If we can differentiate these types analytically, then we have not one dating pigment but two. And so forth.

We are currently exploring ways of taking these kinds of data and combining multiple results to give overall date estimates, with interesting and promising results. Such approaches are also opening up the possibility of incorporating different types of data, notably those that might be considered as coming from the 'art historical' domain, such as the availability of a source image or the structure of a workshop. An important future development is likely to be sophisticated model building, such as already

takes place in archaeological applications.

Refining dating methods, however, is not the only solution. In practice there is commonly informal use of comparative methods based on what may broadly be called 'similarity' where statements about 'alikeness' are used. As with the falsification issue, little systematic research has been made into the formal methods both required and available.

We can unpack the area to some extent nonetheless. For a start there is a basic justification of process in that when an artist is expressing his ideas through the medium of paint he is making a series of choices that are significantly constrained by the time and place in which he works, as well as his own situation and predilections within that. Thus the set of materials chosen, how these are combined and then used, represents the physical manifestation of both the artist's creativity and the broader socio-economic context. Typically we thus get questions such as: What pigments, paint media and artists' techniques does one expect to find at a given time and place, or used by a specific artist? Are the pigments, paint media, artists' techniques found in the painting examined sufficiently similar to what would be expected for that time, place or artist to justify a claim to authorship? What is of actual consequence here is the specific 'uniqueness' of an artist and his situation. Can we discover physical attributes of paintings that are characteristic and, therefore, potentially diagnostic at different levels of resolution, from broad time and place down to (ideally) an individual practitioner? Broadly speaking the answer should be yes, though for such approaches to work two elements are essentially required: first, sufficient data of high quality that is relevant to the problem and, second, a well-characterised method of comparison between cases.

A common practice for any field is the presentation of case studies and surveys of differing extents. For us this ranges from conservation-related examinations of a single painting to entire oeuvre-busting exercises such as the Rembrandt Research Project. Apart from their intrinsic interest regarding the works concerned, they also act as important benchmarking exercises providing raw data for comparative evaluation. Unfortunately such studies also tend to be arbitrary and difficult to use as reference points for our purposes since there is a tendency to use what is available rather than what is ideal, have poor coverage, inconsistent methodology, restricted analysis, provide interpretation rather than data, and so forth. Up until the present time there has been no satisfactory system of data consolidation for such studies, with relevant information often published piecemeal or not at all. Moreover, such intra- and inter institutional databases of painting technical data that currently exist are also largely inadequate as a result of poor access, standardization and interoperability issues. This is a problem not only for scientific authenticity studies but also its sister discipline of technical art history, one that needs to be seriously addressed if we are to progress in these areas.

The nature of the comparative process is perhaps even more open at the present time. Objectively, we are looking for a means capable of taking diverse information such as number and types of pigments, features of technique, perhaps even aspects of pictorial composition, and converting it into a robust measure of similarity or difference. Many of these in fact exist, so much so that the choice can be confusing. For example, so-called 'discriminant analysis' sounds appealing, since it is designed to predict membership assignment into dichotomous groups (such as 'Democrat voter/Republican voter', or 'real/fake') on the basis of potentially diagnostic features of the individuals to be assigned. Unfortunately in practice there are significant difficulties for our application, including the major issue that you need to have a set of data where the outcomes (group memberships) are clearly and unambiguously known, something not necessarily easy to define with authenticity questions. Instead, on the basis of studies we have made, neutral data exploration approaches where there is no such knowledge prerequisite seem to perform better. Clustering techniques for example allow differentiation and grouping of cases from which a critical evaluation of the meaning of the groups formed, as well as the extent of their distinctness, can take over.

We have also been exploring a technique known as *Case Based Reasoning* ('CBR'). In CBR reasoning is based on remembering: 'remindings' facilitate human reasoning in many contexts and for many tasks, ranging from children's simple reasoning to expert decision-making (Leake 1996). As a technique, however, CBR compares the decisions taken in past cases to guide choice in a current, undecided, case. Such cases may be closely related to the original cases, or else a novel one. A key benefit for us is that as an approach it is tolerant of smaller case groups than, say, clustering, while at the same time offering the possibility of 'learning' – the simple addition of newly decided cases add to the overall discriminatory power of the system. We expect to be able to report on these results in the near future.

5. Conclusion

Day-to-day experience often raises questions about one's basic ideas and approaches. How do you deal with

issues that seem intuitively important but are irresolvable with the techniques available? Or quantify the residual uncertainty that one may feel after a judgment has been made? Many of the areas explored in this paper have been framed in direct response to questions such as these that the author has faced in his own practice. Systematic reflection for this paper led to some interesting lines of thought. For example, it was quite surprising to realise that A.P. Laurie would probably recognise so much of the way science is applied to questions of authenticity today, some 80 years after he was writing to the Burlington Magazine about the respective roles of the chemist and the art historian. That the methodology has been so constant would most likely have pleased Laurie. He was operating in an environment where Bernard Berenson could in all seriousness during the Belle Ferronière trial repeatedly pour scorn on technical knowledge of pigments, X-rays, and chemical analysis as 'matters beneath a gentleman connoisseur' (Brewer 2005, 38). To Laurie, we might imagine, the widespread acceptance of his analytical approach and its apparent integration today into decision-making about paintings would count as a vindication. At the same time we must also recognise from our own perspective that, while study of the physical does not, as Berenson believed, put us into the lower classes, we should nonetheless continue to critically and systematically examine the methods that we use. Stasis does not necessarily equal reliability.

The central part of our discussion here additionally concerned some subtle but important points about how we take the data we derive from paintings analysis and then use it to arrive at decisions. This focus was deliberate in that maybe we can at times become over-enamoured of the technology we employ, at the expense of due consideration of interpretation and its methods. There are evident flaws in oft-cited rationales for determining authenticity and so we must seek solutions. One of the most important points however was that viable approaches that can resolve such problems do already exist. These approaches (notably statistical data analysis, diffusion of innovation, and case-based reasoning) have been developed and tested widely in numerous disciplines, their behaviour and robustness well understood through study and use. While we need to find the most appropriate of these techniques for our situation, the formalism that they bring can actually help us identify strengths and weaknesses in our own approaches. To be able to assess the reliability of the judgements we make in these ways is of fundamental importance. In the process we also find that such methods can also radically open out the way in which we view systematic approaches to authenticity questions.

At the present time it is unclear where these studies will lead us. The growth of technical art history, the study of physical aspects of paintings, for example is bound to feed new and important information into the field as a whole. The need for reliable data for broader scientific studies makes revisiting questions of how to determine 'authorship' and date essential. The way these things are achieved in practice though will surely depend on us as a discipline continuing to ask pragmatic, self-reflective questions about our common assumptions. We must not take things for granted.

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[1] There are various introductions to Popper and the philosophy of science; two good recent overviews placing him in context are Curd and Cover (1998) and Ladyman (2001).
[2] For instance when we look at the various steps in a piece of analysis we might be much more confident about, say, that the basic technique works (since many people have developed and used it) than the interpretation of the results. The two main solutions are the use of so-called 'Bayesian' approaches and 'error statistics,' both of which assign probabilities to each component. See, for example Mayo (1997).
[3] It is not the place here to describe the methodologies in detail, though they are based on established approaches to defining likelihood and manipulating such values mathematically. In essence we can apply techniques such as the Bayesian approaches mentioned earlier.