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Statement by Ron Spronk

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Ladies and gentlemen, dear colleagues, the program committee asked me to speak to the importance of expertise from technical art history. I regard that discipline as a distinctly art historical discourse, an approach that utilizes the research of artists' materials and techniques as a tool for art historical inquiry rather than, for example, iconography, provenance, or critical theory.

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Here you see connoisseurship between intuition and science. From the program notes, one gets the impression that connoisseurship and technical expertise are very different concepts, as if they are the opposing ends of the broad spectrum of knowledge on artworks. Although such an "either/or" model might serve to shape our discussions today, I do not think of connoisseurship and technical expertise in that way. Technical art history should obviously not be equated with science, and it is also certainly not deprived of intuition. If we position connoisseurship between intuition and science, we can place technical examinations there as well.

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Rather than being mutually exclusive methods, connoisseurship and technical expertise are actually surprisingly alike in many aspects, as I hope to illustrate to you in this brief introduction and in the discussion that will follow. Both concepts are very much affected by intuition and by science, for example.

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It is probably only within the Humanities that the notion survives that science is somehow separated from intuition. But numerous scientists, among which Albert Einstein, Max Planck and Louis Pasteur, have long recognized the important role of intuition in their work, just as

that of imagination and serendipity. As Einstein put it: “There is no logical way to the discovery of elemental laws. There is only the way of intuition, which is helped by a feeling for the order lying behind the appearance.”

In short, intuition and science are not at all diametrically opposed, but closely interconnected. And neither the connoisseur nor the technical art historian should be placed between these points. In my opinion, intuition and science are important for any scholarly activity, including connoisseurship. It is a stubborn misconception among scholars in the Humanities that scientific data somehow constitute a hard, infallible truth, reached upon by unbiased, blank-slated scientists. But scientists themselves will be the first to tell us that their research simply does not work that way, and that their findings should not be used from this preconception. Rather, such analyses are typically the result of multiple and sequential interpretational processes. As any diagnostic radiologist will attest, it is difficult to interpret X-radiographs, and the large number of ‘false positives’ in mammograms is notorious.

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And even if highly sophisticated equipment such as this GC-MS provides us with highly precise analyses of the compounds of paint medium, we will still have to make sense of the findings, and whenever we interpret data, there will be room for subjectivity and for error. To give just one example: for over a century, it has been known that protein is often present in the binding medium of Early Netherlandish paintings. This phenomenon has been traditionally explained from the fact that Netherlandish painters like Jan van Eyck used either an emulsion as a medium (thus an actual mixture of watery and oily binders, like mayonnaise) or the so-called mixed technique, in which both egg-tempera and oils were used as binder, either side by side for specific colours, or sequentially, for different stages of the layered structure. Tempera is for example believed to have been especially used with natural ultramarine, since this precious pigment could darken in oil. But, as we have only recently learned, proteins can actually be formed as a by-product of the degradation of ultramarine. Similarly, red lakes were often produced out of died textile rags that contain fragments of wool shavings, which in turn

contained protein. Thus, when we find protein with these blue or red colours, we may not simply conclude that it is part of the binder! Similar 'false positives' for protein have now also been identified that are related to the process that is known as saponification, where oil from the binder reacts with metals in pigments. It is especially common in lead white, where it can result in lead soap protrusions, causing tiny craters in the paint surfaces, centuries after the paint had set. In short, the origins of proteins can easily be wrongly interpreted. Interpretational processes, subjective by nature, are front and center in science as well as in the technical examination of art works and in connoisseurship. There is nothing wrong with that, as long as we are all aware of those mechanisms.

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Let me give you one more example: dendrochronology. At first sight, this appears to be a straightforward tool for art historians studying panel paintings, be it connoisseurs or not, but it is actually a highly complex field, and a discipline that is much in flux. The principle is indeed simple: oak trees form one growth ring per year, and the width of the rings varies with climatological circumstances. Virtual all Netherlandish paintings were painted on Baltic oak. Through matching the specific pattern of the growth rings of a panel support with the master chronology for Baltic oak we can determine the year of the most recent growth ring, which provides a clear and hard post quem for the earliest possible felling date of the tree that was used, and thus for the painting. This can obviously be a great aid in issues of attributions, especially to disprove those: if a tree was still growing while a painter has already passed away, this particular painter cannot have painted that panel. But there are several pitfalls here that you should know about before using this data. Did you know that there are multiple master chronologies for Baltic oak? Or that the specific match with the master chronology is indicated with a percentage? Would you not want to know that percentage or what master chronology was applied before using the data?

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A bigger problem is that we need to adjust the year of the most recent ring for two aspects: for drying and transportation and for the sapwood rings that were cut off by the panel makers. Let's start with drying and transportation. The timber had to be transported from the Baltic area to the Netherlands, and it had to dry before it could be used. Peter Klein typically adds 10 to 15 years for this for panels from the 15th century, and 2 to 8 years for panels from the 16th and 17th centuries. But these numbers are not set in stone. They are based on a very small sample set, since only few dated paintings have been analyzed with dendrochronology. Also, the difference for the 15th and later centuries should be a sliding one, and could be affected by climatological circumstances, since timber would dry much faster during harsh winters or hot, dry summers.

High-quality, quarter sawn oak was expensive, and constituted a major capital investment for wood merchants and panel makers. The sooner it was to be used, the better it would be from a commercial point of view. Panel makers would typically only use boards from a tree's heartwood, and remove the outer sapwood that is softer and thus more vulnerable for wood-boring insects. But we do not know how many sapwood rings there were in the first place, since the number of such rings varies with Baltic oak.

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Peter Klein often uses this graph, which shows the number of sapwood rings in 174 living oak trees in Poland. These trees have a minimum of nine sapwood rings, here indicated with a red arrow. On the vertical axis is the number of trees: four trees had 9 sapwood rings. The median value, with the blue arrow, is fifteen rings. In the literature this median of fifteen sapwood rings is regularly but erroneously interpreted as a hard or absolute number that gets added to the youngest heartwood ring, but that is obviously not correct. Only 12 trees, less than 7% of the examined trees, had 15 sapwood rings, and eighty-one trees had fewer than fifteen.

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More importantly, the number of 9 sapwood rings that we have always used as the minimum value for Baltic oak is also not set in stone. According to this article, which was published four years ago, this needs to be revised from 9 to 6 rings.

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This small but important difference is caused by the origins of the data set. The research on which Klein based his minimum value of 9 sapwood rings used data from Poland, slightly further west, and it has now been established that sapwood ring values decrease towards the east. Peter Klein has opted to keep using the minimum of 9 sapwood rings, while Pascal Fraiture and Ian Tyers now systematically apply the lower minimum of 6 rings, and refrain altogether from suggesting a value for seasoning and transportation. These complexities do not make the findings from dendrochronology less important, but we do need to know about them to interpret that data prudently.

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This is especially important for the dating of works that are painted late in a painter's career, such as the Haywain, that we believe to be autograph Bosch. Both Fritz Korény and especially Lorne Campbell have used the dendrochronological dating of the Haywain, which has a most recent growth ring of 1499, to argue for removing this work from the core oeuvre of Bosch. But that is not correct: it is late, but even with the minimum of 9 rather than 6 rings it is not too late to have been painted by Bosch. Campbell also concluded that the Prado Haywayn was copied after the version in the Escorial...

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...which you see here, because the earliest possible felling date here is 1498, twelve years earlier than the Prado Haywain. But there can be no doubt that the Prado version is the original composition, after which the Escorial Haywain was copied.

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Here you see a detail from the central panel of the Prado version, and even with the naked eye you can make out differences between the underdrawing and the paint surface, for example in the man's staff, the pouch on his shoulder and the feet of the boy next to him.

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These differences are easier to see with IRR. The triptych is riddled with smaller and larger changes thus like these. This composition is obviously not a copy, as Campbell suggested, but was conceived here. No such changes were found in the Escorial version. This obviously does not prove that the Prado Haywain was indeed painted by Bosch, we used other arguments to reinstate the Haywain as an autograph work after Fritz Korény had de-attributed it.

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Let's look a bit more at underdrawings, since aspects of connoisseurship play such a big role there. Here is the Frankfurt Ecce Homo, also in the exhibition, which is generally accepted as a Bosch, and most authors consider it a relatively early work.

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Let us zoom into this detail, and briefly look at the style and method of the underdrawing.

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This was applied in a highly distinctive style, with heavy contours that were executed with a broad brush and a very watery medium. The darker areas of the red cloak are prepared with dense zones of hatching, some of which with broad brushstrokes that have merged into a wash. In short, we see a highly distinctive, characteristic style of underdrawing. But we need to be careful here to assume that this style is typical for all of Bosch underdrawings, since the underdrawing had a very specific function here, it prepared the darker areas in the red glaze. It actually indicated those areas where the underpainting for the glaze in lead white was not to be applied. We did find this particular manner of preparing transparent glazes in several other paintings by Bosch. So we can actually use this style of underdrawings when we discuss issues

of attribution, but only in the context of its function in regard to the lead white underpainting of glazes.

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Another important aspect of the underdrawing is best shown in a macro IRR, which provides details of the underdrawing in extreme resolution. We will look at this detail of the Frankfurt panel.

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You will recognize the similar heavy horizontal contour at the waistline, underdrawn in a very watery medium with a relatively large brush. But we also see areas of parallel hatching for which different size brushes were used, for example a medium size brush, and also a small brush. It appears that different mediums were also used for the underdrawing, a darker one and a lighter one. All this implies that the underdrawing was not executed in a single campaign, but in distinct stages, using different brushes and apparently also different mediums. There were three, possibly even four distinct stages in this detail alone. We see a similar phased execution in several drawings by Bosch as well. This presents us with a fascinating methodological problem, since it opens up the possibility that multiple hands might have been involved in the production of the underdrawing. We do not conclude that this was the case, but we cannot exclude it either. And if we do assume that these different stages were all executed by a single hand, the stylistic clustering of underdrawings becomes much more complex. That does not mean that we should not use the style or method of the underdrawing for issues of attribution. The phased execution itself can be used as a characteristic, and so can the stylistic aspects of each of these phases. For example, several works have an underdrawing in a single phase, showing very similar broad contours in a watery medium as we saw here in what we assume was the initial phase.

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I want to end my introduction with a last example, a surprising and important new finding that helps illustrate that the painterly quality of a work might not be a decisive factor when we try to determine whether a work was produced in the Bosch workshop or by a follower.

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To do that, I will use another highlight in the splendid exhibition, the Lisbon triptych with the Temptations of Saint Anthony, which we, unlike Fritz Korény, consider to be a core autograph work in Bosch's oeuvre. I want to ask your attention for a detail in the lower middle of the central panel, here indicated with red.

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We see a fish-boat, armoured and harnessed, with two small figures, At the very rear, one figure is rowing or steering the boat with a wooden spoon. A monkey is pulling in a fishing net, I hope you can see it, it is a bit dark.

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But in the X-radiograph of the detail we see a very different picture. Under the present paint surface we see several other figures as well, which are no longer visible for the naked eye. At the top there is an arm waving with a fish, and a bird just below it **(point out).** And below the bird we see a figure blowing on a trumpet-like instrument, we recognize his hands and a sleeve of his tunic. If we look closely to his head, we notice that he does not have any facial features: no nose or mouth or eyes.

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When we zoom out a step, and compare the visible light and the X-radiograph with the IRR we see many more changes, two of which I would like to point out: the larger opening to the vaulted stone plateau and the different shape of a figure at the rear of the boat, which seems to be leaning back with an arm raised**.** The Lisbon painting has been copied numerous times, and all but two of these copies follow the composition on the surface of the Lisbon triptych very

closely. In the next slide I included the same detail from a painting that is in the Barnes Foundation in Philadelphia. Fasten your seatbelts.

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In the Barnes panel we see the same larger opening to the vaulted plateau, and we suddenly understand the initial figure leaning backwards in the rear of the boat, that was visible in the IRR of Lisbon. It is a monkey, yawning, his mouth wide open. And low and behold, we also see our trumpet player, but let me zoom in on the detail from the Philadelphia panel.

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We see the figure holding his trumpet, but he is not playing it from his mouth but from his arse. We now also see the bird that we also saw in the X-radiograph of the Lisbon triptych, clinging on the figure. The fact that the painter of the panel in the Barnes Foundation knew these details, which were painted out in a very early stage, force us to look again at the attribution of this work, here is the overall painting.

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Ever since Unverfehrt's 1980 dissertation, scholars agree that the Barnes panel was the work of a Bosch follower. But since the painter had intimate knowledge of an early stage of the Lisbon panel, we need to consider the possibility that this work originated in, or very close to, the Bosch workshop.

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That immediately raises a very thorny issue, since the quality of this work is so low. If the Bosch workshop did not only produce A and B quality-level paintings so to say, but also in C, D, and E, then we can no longer use quality as a characteristic for such a determination. If this is indeed the case, we will need to closely re-examine the large group of Bosch followers to see if there are other panels like these that could have originated in the shop.

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Clearly, also after our project and our new, two-volume monograph, there is still a lot of work to do on Bosch. I hope to have illustrated that the outcomes from technical examinations are often not clear-cut, just like the outcomes of any other method, be it archival research or instrumental analyses. All good research will question and verify the data its uses, before interpretations are offered. I urge you to do the same with findings from technical art history.

Thank you very much.