

Art History without Theory. A Case Study in 20th Century Scholarship

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This paper aims to demonstrate that art history's need for theory remains relevant as the process of research advances. The paper rests on a case study from 1950s Hungary. Lajos Fülep composed an interesting opponent's review on the 1955 doctoral thesis of Hungarian Renaissance scholar, Jolán Balogh. Fülep disapproves not of the lack of theory in Balogh's scholarly work, but of her theoretical encroachments without an awareness of a basic need for theorizing. Behind Fülep's critical review there apparently stands the instinctive idea of a Lakatosian scientific research programme. If a historian of art does not pursue a research programme, her work could easily lose its coherence and resonance. Without a research programme, there is no room left either for internal, or for external histories. One also has to consider, whether in the case of art, internal-normative history is governed by the problem of aesthetic value and whether the external-empirical history could be only formulated in these terms. If so, then a theory-unaware history of art would fail to reconstruct how different art-making individuals conceived of aesthetic properties. In line with this idea, the second part of this paper reflects on the status of research programmes in art historical practice.

Keywords: Scientific research programmes, Hungarian intellectual history, theory of art history, need for theory, internal and external histories, Renaissance.

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1. *Problems and theses*

A need for theory (NT) is one of art history's most peculiar characteristics and remains relevant as the process of research advances. To illustrate this, I will embark upon a seemingly trifling case study in 20th century Hungarian scholarship on East-Central European Renaissance: Lajos Fülep's (1974) critique of Jolán Balogh's doctoral thesis (Balogh 1955) on a chapel of Esztergom in Hungary.

Lajos Fülep (1885–1970) was a regular but odd member of the 'Sunday Circle', a loose group of progressive-minded, philosophically idealist Hungarian intellectuals meeting at Béla Balázs's apartment in Budapest between 1915 and 1919. The group was discussing a wide range of problems in philosophy, theory and history of art, literature, and cultural criticism. It was a free association of intellectuals under the leadership of Balázs himself, and of Georg Lukács, assessing the then-actual problems of European culture. Although, when looking upon the Circle's discussions reported by its visiting fellows, Lukács's halo was evidently discernible, Fülep delivered a typical example of this grouping's heterogeneity. His intellectual pedigree was significantly influenced by the pre-war years spent in Italy, had a critical approach to *Geistesgeschichte*, but was almost the single theorist of his generation that had never capitalized on his idealist background to build up a genre of critical social thought (for details see Congdon 1991). This development could have been motivated by the fact that Fülep never went to exile, he only undertook a self-imposed solitude in rural Hungary, while after the second world war he contributed to the vast project of writing the history of Hungarian art by his managerial qualities and his sweeping theoretical insights.

Jolán Balogh (1900–1988), coming from a different generation of young art historians, has been a student of Budapest art history professor Antal Hekler. Between 1926–28 she was an intern at Hungary's freshly established cultural institute in Rome, carried out a vast activity in Austrian and Italian archives and, afterwards, became a decent and acknowledged expert of Renaissance art and culture in Hungary.¹ Without entering the minutiae of this complicated history, it is important to remark that Balogh's apprenticeship in art history was made in the interwar years giving a fresh start to Italo-Hungarian cultural exchange, and to the re-invention of an age-long tradition of Hungarian Renaissance.² Her seminal work in reconstructing the Italianate aspects of 15th–16th century Hungary constituted the basis of her doctoral

¹ For a relevant token of her activity see Balogh (1975).

² Such as her master, Hekler, Balogh was working with the hypothesis that Renaissance was no periodic occurrence in Hungarian Art History (thriving for only good 40 years) but had a parallel evolution with Northern Italian tendencies from the late 14th century to the end of the 16th. A synoptic view on Hungarian advances in art history of the Renaissance, the different Hungarian schools of broader Renaissance studies, and, respectively, Balogh position in these schools has been delivered by Born (2015).

thesis in the mid-1950s. This is the point where her academic agenda intersected with that of Fülep's.

In his critique Fülep disapproves not of the lack of theory in Balogh's scholarly work, but of her theoretical encroachments, and sometimes even of her theory-laden conclusions without the awareness of a basic need for theorizing. Without such an awareness, the message of a historical work would be inconspicuous. Fülep's critique clearly introduces a meta-theoretical tier: what is the envisaged level of theory in historical research in general and why is it needed? This call "into question both the practice [of art history] and fragmented theory [also known as: NT]" (Elkins 1988: 375).

Fülep's critical review is led by an instinctive, not clearly elaborated idea of a *scientific research programme* (RP) in art history. According to this, if an historian of art pursues no research programme or, to invoke Imre Lakatos' words, a normative methodology, her work would lose coherence and, respectively, the whole enterprise is liable to forfeit its scholarly importance. Without a research programme, there is no room left either for internal-normative, or for external-empirical histories. One also has to consider, whether in the case of art, internal histories would be assessing the issue of aesthetic value, whether aesthetic value defines the choice of normative problems for a historian and, accordingly, whether aesthetic value yields the framework for considering the socio-psychological circumstances in which works of art emerge. If so, then a theory-free, or theory-laden but theory-unaware history of art would fail to properly reconstruct the relation of normative and empirical histories of art. In this way, it would be unclear how different art-making individuals, who were coming from different historical, social, and material circumstances, conceived of aesthetic value and of the basic (or extended)³ spectrum of aesthetic properties. Accordingly, Balogh's work, notwithstanding with its clear erudite nature, could be labelled historically "blind" (in the sense of the Kantian dictum, and its paraphrase provided by Lakatos).

In any case, it is useful to ponder introducing the concept of research programmes into the study of art history. With this, I do not mean to suggest that no research programme had been ever used in art historical practice. I rather want to consider whether Lakatosian conceptual tools could add an extra layer to this very practice of relevant reflection. I believe the answer is yes. Reflection on doing art history under the auspices of a certain set of theories would, assumedly, tell us something about their quality. If these theories are research programmes, then their quality will revolve around the two markers of the "progressive", respectively, of the "degenerative". Research programmes are progressive when the normative-theoretical growth of the chosen problems could anticipate the empirical growth of the research process. If, to anticipate this empirical growth, too many auxiliary hypotheses has to be added to the initial theoretical framework, then research pro-

³ For details see Levinson (2005).

grammes will slowly begin to degenerate (Lakatos 1980: 102). At the same time, it seems that one could doubt whether the notions of progression and degeneration are (or are in an usual way) key features of research programmes in art history. I will come back to this topic later.

Finally, I will not, in this text, give a more elaborate taxonomy of art historical and meta-historical genres than their experts (Elkins 1988; see also Iversen and Melville 2010 and Verstegen 2013) have already conveyed, nor will I reflect on the many differences of the history of science and the history of art by emphasizing the role of the visual in the latter (Danto 2014). What is at stake here is the question of what exactly makes an art historical investigation scientific, or how could art history achieve its scientific goals. One conclusive answer is that this goal should be achieved by deploying viable research programmes in art historical practice and, probably, also in reflecting on goals and scopes of art history itself.⁴

2. Fülep's case with Balogh

Balogh's book presents at least five important theses. Two of them are explicitly formulated, while the other three need a careful reconstructive work. Balogh's *explicit* claims are:

- (1) The Bakócz chapel (henceforth: Chapel) is a salient example of Tuscan, and a unique token of sacral Cinquecento architecture beyond the Alps. It is an admirable piece of construction: it is perfectly proportionate; uses only types of local marble for the entire inner decoration; its former bronze dome with relief ornate constituted its *differentia specifica*, which was unparalleled even in Italy at the time;
- (2) It is a direct heir to the space-shaping ideal represented by Brunelleschi's *Cappella Pazzi* in Florence.

These first two points are mixed with three *implicit*, but equally important theses:

- (3) The Chapel was a highly expensive local Renaissance edifice started and finished in an age of financial crisis and economic depression, when all the pillars of the medieval Hungarian state were in decline;
- (4) It successfully survived four sieges, a stark denominational shift, and one complete structure relocation in the 19th century for "its beauty deflected Barbarism" (Balogh 1955: 17);

⁴ The concept of research programmes is *ab ovo* self-reflecting. While one considers the history of science through the lens of conflicting research programs the historiography deployed would also conflict with other kinds of historiographies in determining the basic values of inquiry. To cite Lakatos: "The methodology of scientific research programmes constitutes, like any other methodology, a historiographical research programme. A historian that accepts this methodology as a guide will look in history for rival research programmes [...]" (Lakatos 1980: 114).

- (5) This survival was facilitated mainly not by the supreme technical knowledge of its artisans, but by its aesthetic value.

Fülep's critique was clearly ignited by the number and importance of such implicit theses. Fülep calls Balogh to account for not delivering a "synthetic-theoretic outlook" of the era she investigates. Without this "general outlook", the "air of the epoch" would not transpire. Balogh's work, Fülep says, is painfully documented and scholarly accurate, but it fails to picture "the historical reality that produced the artworks", respectively "what is essential to understand the historical determinations conditioning its emergence" (Fülep 1974: 457, 461, 670). Fülep misses, in Balogh's book, the "representation of the milieu", i.e. the social environment that lets the artwork emerge. On a final account, Balogh's work conveys no "Geistesgeschichtlich" framework (just a few promising traces of a fragmented theory) which could be the warrant of understanding the concrete, singular artwork. She could be regarded as an astute researcher, who collects all the traceable facts then neatly presenting them in kind of a catalogue raisonné without providing the bigger picture the story would require.

3. *Methodology, theory, and Lakatosian RP*

I endorse the view that one can have an elaborate methodology of treating and unravelling the aspects of artistic creation, without having a complex theory that could be applied everywhere, that is, a framework to accommodate *all* the facts discovered. I will use "theory" here in a minimal sense, as a selective point of view. A minimal need for theory is, therefore, *a need for a selective point of view*.

Theory, as a selective point of view, had an important role in the philosophy of historiography. In his classical essay, Karl Popper pleaded for a theoretically informed way of historical inquiry: He wrote, "[...] there can be no history without a point of view. Like the natural sciences history must be *selective* unless it is to be choked by a flood of poor and unrelated material. The attempt to follow causal chains [...] has little interest for us. [...] The only way out of this difficulty is to introduce a preconceived selective point of view [...] that is to write a *history which interests us*." (Popper 1957: 150).

One of the most creative appropriations of this idea goes back to Imre Lakatos. At the beginning, Lakatos interpreted the Kantian dictum ("history of science without philosophy of science is blind" (Lakatos 1980: 102)), to emphasize the constant interaction of a history of ideas with the history of implementing them. Afterwards he applied this twin-focused analysis to Popper's diagnosis on the selectivity criterion of writing history:

Some historians look for the discovery of hard facts, inductive generalizations, others for bold theories and crucial negative experiments, yet others for great simplifications, or for progressive and degenerating problem shifts; all of them have some theoretical 'bias'. This bias, of course, may

be obscured by an eclectic variation of theories or by theoretical confusion: but neither eclecticism nor confusion amounts to an atheoretical outlook. (Lakatos 1980: 120)

Whereas research programmes are not theories, but a series of interconnected theories that have the goal of constituting scientific objectivity, Lakatos would have only been speaking about a series of theories, which are implemented from a selective point of view. Accordingly, the minimal need for a research programme proves itself to be a minimal need for a selectivity-driven series of theories, which have the goal of constituting scientific objectivity.

4. *Towards the concept of research programs in art history. Brief comparison of Fülep and Lakatos*

I think that there are more than *prima facie* similarities between the positions of Fülep and Lakatos. At the same time, Fülep's stances are clearly less mature and less elaborate.⁵ In what follows, I present the blueprint of their parallel agendas and I do so by staking out four similarities (S) and two differences (D) between them. The last two will, hopefully, outline how a research programme in art history should be understood.

- (S1) Inner history (intellectual history) and outer history (social history) are complementary (Lakatos 1980: 102). Rational reconstruction of this inner discourse enhances the relevance of certain empirical data.
- (S2) Inner history is primary in understanding what the specificity of the discourse is. Therefore, "aesthetic value" is primary to the (n.b.: necessary) outer conditions under which aesthetic value emerged (Fülep 1974: 458).
- (S3) There is no historical discourse without a communal theoretical bias (Lakatos 1980: 15, 120). In addition, it is easier to admit one's minimal theoretical commitments, then to let them return unconsciously (Fülep 1971: 23).
- (S4) Therefore, one must impute to art history a *hard core* of theory and a *positive heuristic*, "which defines problems and delimits anomalies" and, accordingly, outlines a plan how the integrity of this theory should be kept.
- (D1) As already mentioned, there is at least one crucial concern about the decisive status of *progression* and *degeneration* in art historical research programmes.⁶ Lakatos says, if we consider the

⁵ Their similarities could be traced back to a common Hungarian intellectual background. The general idea, that an inner, intellectual and an outer, social history should be interconnected, were both emphasized in the young Georg Lukács's writings, respectively in Lakatos' view on the development of scientific knowledge. For the outline of such a comparison see Demeter (2008).

⁶ For the role of progressive and degenerative problem shifts in aesthetics see Nanay (2017: esp. Chapter 4). This is, to my knowledge, the most complex essay on framing aesthetics through Lakatosian concepts. A programme is thus degenerating

research programme to be the “unit of mature science” (Lakatos 1980: 179), then we will also have to stipulate the conditions of appraising them. In pursuing a research programme, we regularly modify some elements of the inner history to protect the hard core of the theorem, and to keep in balance the normative and empirical side of research. The hard core is a limited set of main ideas that our research is built upon. If these ideas need to be repeatedly multiplied to yield a minimal empirical growth, then it is very likely that our research programme has started to degenerate.

But, Lakatos is mainly committed to analyse the evolution of natural science. A research programme in the natural sciences also has to keep the balance of inner-normative and outer-empirical histories. If the empirical growth is staggering, then a modification of the inner-normative hard core will constitute a legitimate move to improve its performance. Such a research programme could be theoretically progressive if “each [such] modification leads to new unexpected predictions” and could be empirically progressive “if at least some of these predictions are corroborated” (Lakatos 1980: 179). The status of (corroborated) predictions and of the ability to explain new facts both depend upon the central role, which Lakatos confers to cognitive value.

when adding too many *ad-hoc* hypotheses to protect the hard core of the theory. Nanay clearly allows for degenerativity and progression in theories of art, because he is less worried about the methodological distinction I still make between cognitive and aesthetic value. His quest for a new discourse on aesthetically relevant properties, clearly, calls for no such distinction. Aesthetically relevant properties yield a difference in our experience, and are usually more accurate than the, so Nanay, sometimes puzzling notions of beauty, ugliness, serene, moving, and sublime (Nanay 2017: 70). More accurate and straightforward (Nanay 2017: 71) concepts can multiply the number of fruitful analyses in the complicated relation of the perceptive and the aesthetic regarding the whole universe of artistic production. The difference in our experience is thus delivered by new evaluative and critical practices helping us to understand aesthetic phenomena: “Hence, if a research programme in aesthetics is consistent with and can explain our critical and evaluative practice, we have some (not necessarily conclusive) reason to consider it to be progressive. If it can’t do that, it is likely to be degenerative.” (Nanay 2017: 78) Something very similar is stated a few pages later: “A research programme is likely to be progressive and not degenerative if it can explain new phenomena and open up new research directions.” (Nanay 2017: 83). If progression or degeneration hinges on introducing new and more accurate evaluations and critiques of the matter discussed, then one will instantly comply to the thesis that art historical research could also be both degenerative and progressive. But, if this also includes the acceptance of a second thesis on the less accurate nature of aesthetic properties, one would also beg to differ. Classical pieces of art could be accurately grasped by their aesthetical properties for they have an outer history of conforming and opposing to regularly changing standards of beauty and ugliness. How to hold and, respectively, how to lose the aesthetic property of beauty would be a question underlying research programs that have not overspent their budget. They are not cognitively compelling but still evaluatively very rich. As long as art history can raise interest in this traditional discourse through a research program, it would not have to be replaced.

According to these principles, predictive-explanatory progression guarantees the sustainability of research programmes in the natural sciences. But, I think it would be more accurate to adopt a different agenda in specifying the sustainability of art historical research programmes. I think that, in order to be sustainable, art historical research programmes need only to just be *viable*, where viability means to be efficient in arousing interest in equally old and new problems and values by not necessarily demanding epistemic novelty.

Viable research programmes in art history are concerned less with making predictions, because they are less concerned with cognitive value. They usually make inquiries into how the cooperation of artists and commissioners succeeded to materialize seemingly abstract aesthetic qualities. This is why progressive or degenerating problem shifts could be unintelligible to art historians. So long as inner history usually reflects aesthetic value, its purport would depend neither on epistemic novelty, nor on the success of predictions. This informs us about a second major difference between scientific and art historical research programmes.

(D2) Lakatos' concept of *heuristic power* also remains problematic here. If one investigates the history of materialized aesthetic properties, it will be less important how *many* ensuing new facts could be produced, regardless of these fact's "capacity to explain their refutations" (Lakatos 1980: 52). While an art historian constitutes a normative-theoretical history she is less focused on the *amount* of new facts. She is, or has to be, concerned with the most plausible, and sufficiently interesting connection between aesthetic properties and the existing artworks reflecting these properties.

But let us just imagine a case, where epistemic novelty keeps its central role. That is, let us imagine a case where, in the context of predictability, the issue of new data still remains highly relevant. However, new data could be defined in, at least, three different, ways: (a) as empirical information or new sources of empirical information; (b) as new diagnoses of a given constellation (c) as new evaluative and critical practices pertaining to a set of phenomena. But none of them could, strictly speaking, be predicted. No such new data could be predicted to occur unexpectedly, let alone repeatedly or regularly in a determinate future. There are no new art historical discoveries that are guaranteed to be perfectly in line with the initial conditions reconstructed by specially trained scholars. No theorist of art historical practice could predict which relevant stone-fragment of the original façade an archaeological excavation would reveal. She can only suggest that a decisive finding—without determining its exact condition, shape, or colour—is very likely to occur.

If a research program is a normative methodology that fulfils the basic need for coupling inner and outer histories in art historical research, then it has no need to prove itself as progressive or as degen-

erating, for it does not claim to be able to predict the future, or if it does so, in a very limited sense, something like: it can foreshadow that new evaluations of the matter will be available soon, although the exact markers of that novelty are still uncertain (c); it can conjecture that new proofs will be given, if the researcher follows a given direction (b), or, and most possibly, the research will be more sensitive to phenomena resembling the case that has just been studied (a). This sensitivity-conditioned enterprise could equally be positive and negative. Positive, if it facilitates interesting discoveries or evaluations of them and, respectively, negative, if it makes this evaluation too easy, schematic and, consequently, uninteresting.⁷

What would constitute, in conclusion, a viable research program for the practice of art history? A good recipe is joining inner and outer history so that it would rouse interest in the scholarly achievement. But there is one more factor that determines our definition.

If we define the practice of art history by the conditions under which it could be classified as a scholarly achievement, we have to stipulate these conditions, partly, as conditions of resonance. The quality of a scholarly achievement could be evaluated according to the level of its intellectual resonance, or to put it other words according to how it resonates with different people and communities over time. Sociologically speaking, the extent of the population constituting the reception of an intellectual work (also an inquiry in the history of art) is crucial to the evaluation of this very work. If an intellectual product aroused a sense of urgency in the public and if it was widely received and discussed, it would have more chance to survive as a constituent of the general discourse on a certain topic. It would thus have a far better chance to be evaluated as scholarly relevant, disputable or even thought-provoking. But what conveys those intensional criteria that could trigger an extensive reception? It depends on the nature of the matter disputed. But, indifferent of this specific nature, intensional criteria have to meaningfully reflect the characteristic position of the historian concerning the relation of “intellectual” and “social” history. Art historians usually tell the story of the ubiquitous and necessary outer conditions under which artworks, bearing an aesthetic property, have been made.

Accordingly, RP in art history is determined by *two factors* (a) a (normative) theoretical framework to steer the research process, and (b) an (instructive-sociological) picture that, properly drafted, makes its findings accessible to a learned audience with a relevant, but not

⁷ This resembles the way Clifford Geertz found place for the concept of prediction in his methodology of thick description. A cultural anthropologist could not *strictu sensu* predict the occurrence of phenomena. She can—by using a semi-fledged form of clinical inference—diagnose them or, „the very most”, anticipate that they are very likely to happen (Geertz 2000: 26). This is due to the fact that cultural sciences could not generalize across different cases but can only use a more general idea in the particular case themselves: “The essential task of theory building here is not to codify abstract regularities, but to make thick description possible, not to generalize across cases but to generalize within them” (Geertz 2000: 26).

identical form of scholarly expertise. But, at any rate, great scientific achievements (Lakatos 1980: 110) in art history are research programs because they can accommodate a larger spectrum of problems, answers, and various historical cases of unpredictability.

5. *Framing the case of Balogh.* *Possible research programmes*

In the example conveyed by Balogh, we can discern at least two viable research programmes. These programmes have otherwise no exclusivity. Different possible ways could be also pursued unless they deploy a positive heuristic and rouse interest in the scholarly public, while the research programmes, which are discussed in what follows, could be easily abolished if they prove themselves unviable.

5.1 *Questioning the Prosperity Theory*

Prosperity Theory states that the incontestable achievements of high Renaissance art could be understood against the backdrop of local economic wealth (Burke 1986: 37–38). This was deemed to be wrong by Robert Sabatino Lopez at the beginning of the 1950s, although we have no evidence that Balogh knew about these critical assessments of his Italian-American Colleague, or about the outlines of such an RP in art history.

Lopez's main point consisted in the remark that humanistic culture was a form of investment, even if not a purely economic one. In high Renaissance and Humanism "culture [...] tended to become the highest symbol of nobility, the magic password which admitted a man or a nation to the elite group. Its value rose at the very moment that the value of land fell. Its returns mounted when commercial interest rates declined. Statesmen who had tried to build up their power and prestige by enlarging their estates now vied with one another to gather works of art" (Lopez 1959). Some distinguished statesmen of the Renaissance era in Italy, like the mighty Lorenzo de Medici received their "halo of respectability" through their patronage of art and not through the sustainability of their business matters. As far as we know, Balogh traced a similar path in her naïve treatment of Tamás Bakócz's role as a patron, which makes this core idea useful for the outer history it can adumbrate. When a surplus of power could not have been generated by the rulers of a certain community (e.g. by Lorenzo de Medici in Florence or by Tamás Bakócz in Hungary) or through economic excellence, then it was granted by investment in symbolic means. The outer history would tell us how the patrons themselves were conscious of this endeavour of investing in symbolic means and how they managed to implement them under different conditions. If this research program succeeds, it could also cast new light on the historical role of East-Central European patrons: they were lagging behind their Western col-

leagues probably not due to their economic inferiority, but due to the different consequences of their endeavours to acquire symbolic power.

5.2. *The Wölfflinian way*

Some recent work on Heinrich Wölfflin's *Principles of Art History* have revealed the twofold importance of his contribution to a formalist method of art history (Gaiger 2015). The first issue concerns the so-called history of vision claim, while the other assesses the parallel issue of the notions of pleasure. The pairs of concepts governing a discourse on the history of vision are necessarily coupled with another history of decorative appreciation:

It is dangerous to speak only of certain «states of the eye» by which conception is determined: every artistic conception is, of its very nature, organized according to certain notions of pleasure. Hence our [...] pairs of concepts have an imitative and a decorative significance. (Wölfflin 1959: 16)

To understand this stance on principles of art history, one need not be excessively knowledgeable in the historiography of the discipline. But a short excursus on Alois Riegl's conception on the scientific status of art history could be of some use.

Riegl was loudly concerned with the inflated scholarly meaning of technical inquiries. In his ironic assessment of then-contemporary research in applied arts, he remarked that, according to recent scholarship, all forms of production in art industry could be treated as the outcome of specific technical conditions and, accordingly, could be criticized as such. But to which ends does one act when praising or scolding the technical conditions of applied arts? Does it have to reemphasize the initial idea, that art industry is clearly controlled by the level of technical development (Riegl 1924: vii)? The same could be asked about the psychological path to art. What is the scholarly gain of describing art as the world of artefacts produced by artists, who have a certain set of inner (creative) capacities? What is the scholarly gain of stating that the artists are in good or bad command of their truly existing abilities or that they are adept or not adept at doing something? Riegl rather pursued the goal of scientific art history, which lacked reductive or circular argumentations and, therefore, formulated the question: what fulfils these scientific needs? His answer was a synthetic inquiry into the reconstruction of artistic volition (*Kunstwollen*), which is the reconstruction of what art producing agents *wanted* to express through their activities by opposing technical and psychological burdens (Riegl 1927: 9) Riegl's expressly corrective-scientific goals were equally endorsed by his advocates (e.g. Benjamin) and opponents (e.g. Gombrich), regardless of how they came to terms with *Kunstwollen*. Without entering into a heavily laden discussion of the topic, I just hope to signal that the same question motivated Wölfflin's *Principles*: I wish just to resolve what the science of art history is and which methods it should deploy to secure the specificity of the aesthetic. The answer emerges bluntly. The

envisaged scientific status of art history has to be provided by a careful analysis of historically changing visual capacities. But, this analysis needs to be underscored by a parallel history of the sensitivity for beauty. This latter history clarifies how artists of different generations were reassessing the works of others. When artists of a certain period have found the works of their predecessors' worth discussion, reconstitution, re-enactment, or reproduction, they have also laid bare a certain sensitivity for the aesthetic properties of these works. To settle the most pertinent regularities of these histories should be the main task of an art scholar: "[...] men have at all times seen what they wanted to see, that does not exclude the possibility that a law remains operative throughout all change. To determine this law would be a central problem, the central problem of a history of art" (Wölfflin 1959: 17).

We can also fabricate a viable research programme if we put this thought into effect and analyse the historically changing sensitivity for aesthetic properties in the works of others. Along these lines we could have some robust tools to deal with the issue of how works of art could survive the most problematic ages. *Mutatis mutandis*, we will understand how the Chapel has managed to survive. This case of the Chapel is modelling a key aspect of the autonomy of art, that is, the historical development of the conception of beauty. Following this reading, the long-lasting success of the Chapel could be understood as a history of how it was adapted to various views and conceptions on being "decorative". To put it differently, in various ages there were various ways of "longing for beauty", and some distinguished artworks were able to similarly fulfil these changing endeavours and to connect artist of different pedigrees and sensibilities. The Chapel proved itself worth re-assessment in different ages and among different social and material conditions.

Through both of these examples, we can make the above-mentioned features of art historical research programmes understandable. One of these features is the basic conundrum of predictability. Let us see how this all works here.

In the case of high Renaissance Hungarian commissioners of artworks, one could not predict that three more tokens of investment in symbolic means of power would be discovered. No one could necessarily state that financiers kept on commissioning marble chapels since they were unable to purchase marble quarries. In this case, as in the vast majority of such cases, it is enough to be clear and consequent in joining inner and outer histories. The rate of symbolic investments in culture rises when economic growth is on the wane. This is the inner core, which is exemplified by various historical cases (that prove or refute it), that will conjure a viable research program. It is important though, not to generalize across data recorded before, but rather to use inner history as a useful tool in understanding these discoveries.

The position of predictability in the Wölfflinian programme is very similar. We cannot foresee that a highly acknowledged piece of Renaissance architecture will survive another siege or another structural relocation thanks to the “beauty” it is endowed with, but we can conjecture that the pleasure it causes could be a serious topic of discussion when locals would have to decide whether keeping, destroying, or rebuilding it. If the physical existence of an artwork is closely attached to a discussion on its aesthetic properties (which it can possess or not possess), one could deploy at least a research programme, which joins the history of ideas with the history of wanting them materialized. This programme would prove itself viable if new ways of joining inner and outer histories kept the interest in it alive.⁸

6. *Conclusions*

After careful reconstruction and reconsideration, it can be concluded that Balogh’s work can be employed to infer in both a descriptive and a normative manner. But, due to a complete lack of clearly formulated viable research programmes, her theses are still relevant while less defensible against refuting facts and concurrent (i.e. coherent) research programmes.

As a sociological corollary, without research programmes, art historical works have less chances to be received, discussed, and, consequently, will be unable to enter a wider community. To prevent this, one has to redeem art history’s basic need for theorizing. In addition, the constitution of viable research programmes puts historians of art in an even better posture. By deploying these programmes, historians would be able to join both characteristic sides of art scholarship: an endeavour for facts and, respectively, the predilection for well-known but sometimes vaguely presented aesthetic properties. One cannot prove the specificity of the aesthetic, if its values are not checked against the changing material conditions under which they were put into practice. Viable research programmes could be extremely helpful in fulfilling this task.

⁸ More recent scholarly work on the Chapel has never reached a wider audience probably due to lacking a viable research programme. But, it has never forsaken theory in general. E.g. Miklós Horler (1990) followed a decent Marxist interpretation of Renaissance man’s urge to create or to finance creating artworks (see Heller 1978). However, to render this theoretical commitment into a viable programme, he would have had to couple inner history informed by the Marxist theory of estrangement with an adequately reconstructed outer history. In other words, he would have had to trace the historically changing patterns of dealing with this very phenomenon of estrangement, which he did not.

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