

# Putting Science Back in History of Science<sup>1</sup>

**Hasok Chang**

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## 1. Introduction: where has all the science gone?

I am very honoured to give the Jaakko Suolahti Lecture, and to join in the 50th anniversary celebration of the Finnish Society for the History of Science and Learning. On such a celebratory occasion it is also important to recognize the conflicts that exist within our field,<sup>3</sup> and I would like to begin my remarks today by referring to one particular conflict. This revolves around the question of how we position history of science<sup>4</sup> in relation to science itself. This issue has created some deep divisions, which I think should be recognized, discussed, and healed.

The title of my lecture is a deliberately provocative and controversial one, and I hope you will forgive me for that. It refers to some people's feeling that in much current work in history of science, there is a trend to avoid engaging with the technical content of scientific knowledge — a trend that would be detrimental to the health of our discipline if it became truly dominant. Back in 1980 *Science* magazine attributed the following remark to Charles Gillispie, under the title "History of Science Losing its Science": "Once a highly respected

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<sup>1</sup> I would like to thank the following people for their helpful comments on earlier drafts: Olivier Darrigol, Peter Galison, Marcia Ferraz, John Forrester, Ana Maria Goldfarb, John Heilbron, Gerald Holton, Nick Jardine, Martin Rudwick, Simon Schaffer, Jim Secord, Conevery Valencius, and Andy Warwick.

<sup>2</sup> The portions of the text in square brackets and in footnotes were omitted in the actual lecture, in the interest of time. A version of this lecture was delivered as the BSHS (British Society for the History of Science) Presidential Address, at the International Congress of the History of Science, Technology and Medicine, 22 July 2013, Manchester, United Kingdom.

<sup>3</sup> With a clear recognition of the nature of the differences, we can make a well-informed decision about whether and how to eliminate or cultivate them.

<sup>4</sup> I will say "history of science" without the definite article when I refer to the field of study, and "the history of science" when I refer to the thing that is the development of science over time. And I will not speak of the "sciences" here, because the plural is only a fig-leaf, which still insists that there is a type of thing called science of which there are many diverse instances. The diversity goes without saying, when I say "science".

field that focused on the conceptual evolution of scientific ideas, the history of science is losing its grip on science, leaning heavily on social history, and dabbling with shoddy scholarship.”<sup>5</sup> This was an extreme expression, but similar feelings have been shared by many others. Olivier Darrigol, one of the people I respect most in our field, laments that most historians ignore “the few histories” that still make attempts at “engaging the more abstruse, codified parts of scientific work” and “underestimate them as fossils of an outdated historiographical tradition.”<sup>6</sup> The disgruntled internalists, as I shall call them for a moment, say: if all we want to study is the history of scientists (not of science) and their institutions and their social and cultural determinants and influences, if we want to insist that science is just another socio-cultural phenomenon, if we are not going to deal with the distinctive practices of science producing knowledge, then there is no clear reason why we should have a separate field of history of science, with our own independent departments, societies, or journals — in that case we should join the ranks of general historians, and declare history of science obsolete. However, content-free analysis of science cannot constitute the entirety, or even the core, of a discipline properly called “history of science”.

On the other side of the divide there are, of course, many people who say that joining general history departments is precisely what we should be doing! They feel that historians of science should be first and foremost properly trained historians, with methods and sensibilities shared with other historians. Kathryn Olesko, a recent editor of *Osiris*, declared that the 11 volumes that she edited were “designed to dissolve boundaries between history and the history of

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<sup>5</sup> Broad (1980), p. 389. This report, by the journalist William J. Broad, prompted some qualifications from Gillispie himself, as well as a robust response from Robert Kohler, himself a Ph.D. in chemistry (Gillispie and Kohler 1980).

<sup>6</sup> Darrigol (2007), p. 34. Darrigol’s statement is prefaced by the recognition that “surprisingly much [of value] has been said without engaging the more abstruse, codified parts of scientific work”, though clearly he laments the fact that “few historians of science now have a sufficient background to make sense of these aspects.” Edward Grant (2012, p. 135), while admitting the importance of the social context of science, is still suspicious of social historians: “This shift of emphasis from ideas in scientific texts to the social relations of science is not completely the result of the broadening research interests of historians of science. It is, I would suggest, also a consequence of the diminishing knowledge of science that prevents, or seriously hinders, historians of science from writing about the content of modern sciences”.

science.”<sup>7</sup> On this side people often worry that focusing on the content of past science without attention to its social and cultural contexts prevents a well-rounded understanding of the development and significance of science.<sup>8</sup> Some of the more impatient contextualists have been frustrated that we still cannot seem to get rid of internalist dinosaurs who don’t have a real sense of history.<sup>9</sup>

[Looking more positively toward the future, Olesko and Robert Kohler surveyed the cutting-edge of Anglophone scholarship in history of science in 2012, and detected the following popular themes: “communication and circulation, place and space, publishing and print culture, and lay meanings and uses of science, among others” — they consider these to be thematic concepts with the promise to take our field beyond an aimless profusion of microstudies, in the aftermath of the demise of grand narratives.<sup>10</sup> Now, it would certainly be possible to use these themes to frame content-focused research, but in actuality this does not happen very much. Scientific content is just not a preoccupation here.]

By now the Young Turks have become establishment figures, and social and cultural history of science is the dominant mode of work (at least in some countries — I do not know if this is the case in Finland). But the old argument is not over and the conflict is still real, even though the picture I have just given is an oversimplified caricature. As late as 2007, Kostas Gavroglu and Jürgen Renn still reckoned that the “tension . . . between the focus on content and on context” was “responsible for much of the acrimony presently prevailing in our field.”<sup>11</sup> The causes and consequences of this tension need to be addressed, urgently and at a deep level. [Of course, many people have made attempts to bring the different perspectives together, and I only intend to continue and build on their efforts. For example, Ana Maria Goldfarb and her colleagues have called for an articulation of a distinct identity of the historian of science as a scholar committed to combining three spheres of analysis — internal, contextual and

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<sup>7</sup> Kohler and Olesko (2012), inside cover.

<sup>8</sup> An excessive preoccupation with the content of science may also be a symptom of an unhealthy deference to scientists.

<sup>9</sup> And Gillispie’s insults have been returned in equal measure.

<sup>10</sup> Kohler and Olesko (2012), p. 5. They describe these themes as “midlevel concepts”.

<sup>11</sup> Gavroglu and Renn (2007), p. 1.

historiographical.<sup>12</sup> I don't know how much this call from São Paulo and other comparable ones have been heard around the world.]

## 2. Groundwork: removal of false dichotomies

Before giving my own positive view on the issue, I would like to clear away some widespread misconceptions, which can be expressed as a set of false dichotomies.

### Social vs. intellectual

First of all, there is a persistent notion that social history and intellectual history are mutually exclusive opposites. Against that idea, we need to remember those classic works which demonstrated brilliantly that social and intellectual analyses of science can be co-extensive, including the works of Peter Galison and Andrew Pickering on modern physics, the sociological reductionism of the strong programme in the sociology of scientific knowledge, and Steven Shapin and Simon Schaffer on early modern epistemic-and-social order.<sup>13</sup>

### Cultural vs. intellectual

Cultural history is also often held up as the opposite of intellectual history. This is very puzzling to me. Only people from strangely anti-intellectual cultures could imagine “cultural” as the opposite of “intellectual”. As Robert Darnton put it in *The Great Cat Massacre*, the meaning of “cultural history” is that “it treats our own civilization in the same way that anthropologists study alien cultures”, capturing the “otherness” of the past, to understand it in its own terms, “to unravel an alien system of meaning.”<sup>14</sup> But this is precisely what intellectual historians of science have been doing for many decades!<sup>15</sup> The alleged contrast only comes because one perversely excludes science from culture, and then looks for its so-called “cultural” contexts.

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<sup>12</sup> Alfonso-Goldfarb (2008).

<sup>13</sup> For the best-known and most representative works, see Galison (1997), Pickering (1984), Bloor (1976), and Shapin and Schaffer (1985).

<sup>14</sup> Darnton (1984), pp. 11-15.

<sup>15</sup> If this was fresh new wisdom in general history in 1984, then we historians of science, even the old-fashioned ones, were decades ahead of the game.

### Social/cultural vs. rational

Similarly, there is a widespread view that if we concern ourselves with the rationality of science then we cannot do social or cultural history, and *vice versa*. For this misconception, philosophers of science and the internal historians of science are just as responsible as those allegedly on the opposite side. We need to get beyond this misconception and learn to recognize, with a whole line of thinkers ranging from Robert Merton to Jürgen Habermas to Philip Kitcher,<sup>16</sup> that rationality is something fully embedded in and dependent on social, political and institutional settings, but not meaningless for that reason as a normative guide to practice.

### Practice vs. theory

We also need to lose the habit of equating the content of science with theories, or ideas. Scientific content is embodied in all epistemic aspects of scientific practices, including not only ideas and theories, but experiments, facts, models, arguments, know-how, inventions, technological applications, materials, and instruments.<sup>17</sup> The dichotomy between “theory” and “practice” is generally invalid, as shown most clearly by some excellent works on theoretical practice, for example by Andy Warwick and more recently David Kaiser.<sup>18</sup>

### Return to external vs. internal

The greatest harm comes from all these false dichotomies being lined up together, reinforcing each other and creating a misleading binary view of our business. And all of these dichotomies have been mapped on to the external–internal distinction.<sup>19</sup> But I think the internal–external distinction can actually be cogent and useful. Dudley Shapere’s re-formulation of the distinction is helpful. Shapere takes the internal as what has been “internalized” in a particular epistemic community. Internal considerations are based on a body of

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<sup>16</sup> A good start would be Merton (1973), Habermas (1984), and Kitcher (2011). We should also take note of the works of Max Weber, Michael Polanyi, Thomas Kuhn, and Ludwig Wittgenstein in this regard.

<sup>17</sup> The study of all these elements would be included in the “history of knowledge” that Darrigol (2007) calls for.

<sup>18</sup> Warwick (2003), Kaiser (2005).

<sup>19</sup> See Shapin (1992) for a detailed and instructive discussion of the history of the internal–external distinction.

beliefs that have come to be accepted beyond specific and practical doubt,<sup>20</sup> due to the success and coherence of scientific inquiries made on their basis. Such beliefs “constitute a basis on which science can alter its domains and build further hypotheses, methods, rules of reasoning, and goals.” I would broaden this notion of the internal to incorporate other aspects of scientific practice rather than just beliefs, but the important point for now is Shapere’s insight that the internal–external distinction is “forged in the very process of investigation of nature, not laid down in some edict from heaven or philosophy which determines what counts as scientific and what does not.”<sup>21</sup> If we take the distinction in this way, and free it from the mis-association with the false dichotomies discussed earlier, it can help us distinguish different kinds of questions in history of science that demand different modes of inquiry.<sup>22</sup> This revised sense of “internal” and “external” is what I shall mean when I use those terms in the rest of this lecture.

### 3. How history of science demands a critical engagement with the content of science

#### What is critical engagement?

Having prepared the ground by dissolving some false dichotomies (and resurrecting an apparently defunct one), let me now come to my main message: history of science has some important functions that cannot be served unless we engage with the content of science, and do so with independent critical

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<sup>20</sup> Here Shapere is acknowledging that *anything* is subject to global skeptical doubt, which is irrelevant to the practices of inquiry.

<sup>21</sup> Shapere (1986), p. 6.

<sup>22</sup> How so? Alan Chalmers (1990) gives a nice homely analogy on that question. Suppose that a football player kicks a ball toward the goal; that is easily understood as a routine move within the rules of football — in other words, internally. But if the player rushed to the ball, sat down in front of it, pulled out a knife and a fork and attempted to eat it, we would need to seek an external explanation referring to factors that go outside the normal rules of football. In an analogous way, historians of science need to have the conceptual tools with which to distinguish the routine occurrences at a particular stage and location of science from outcomes determined by factors that are considered external. Contrary to the demands of the symmetry principle of the strong program, it is instructive to have different modes of understanding and explanation appealing to internal and external factors.

judgment. (This is not to say that it does not have other kinds of valuable functions, only that the functions I am going to address today are not sufficiently emphasized usually.) Engaging with the content of science while exercising our own judgment is what I really mean by “putting science back” in history of science.

In order to realize the full potential of history of science, we historians need to be people of scientific discernment. As an instructive comparison, take Peter Winch’s view on the history of art: “A historian of art must have some aesthetic sense if he is to understand the problems confronting the artists of his period; and without this he will have left out of his account precisely what would have made it a history of *art*, as opposed to a rather puzzling external account of certain motions which certain people have been perceived to go through.”<sup>23</sup> There is every reason to apply this insight to history of science, too.

Among today’s historians of science, the conventional wisdom is that our business is to *describe* past scientific knowledge, and perhaps *explain* how it came to be the way it was, but we do not and should not pass judgment on it. (Many a student wrist is slapped every day in history-of-science classrooms for violating this taboo.) I think this is an incoherent, untenable, misleading and harmful attitude, which we need to abandon before we can talk seriously about the functions of history of science.<sup>24</sup> And I think we ended up with it as a result of a misguided over-reaction against traditional whiggish history of science. It is often assumed that the sins of whiggism in judging past science according to the criteria of present science can only be avoided by suspending judgment altogether — through the kind of epistemic neutrality advocated in Kuhnian internal history or in the impartiality principle of the strong programme. But this is an extreme response. As Nick Jardine says, “All too often recent historians of science have abandoned common sense in their flight from presentism.”<sup>25</sup>

To fight whiggism, we only need to make sure that our view of past science is not dictated by current scientific orthodoxy. It is not necessary to abandon all judgment. And neutrality is in fact a judgmental stance disguised as

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<sup>23</sup> Winch (1990), p. 88.

<sup>24</sup> See Chang (2009) for a more detailed argument.

<sup>25</sup> Jardine (2003), p. 134.

non-judgment. (Nobody would consider that it was not a political act for Switzerland or Austria to declare itself a neutral nation; perhaps something similar can be said about how Finland positioned itself in the Cold War.) In the historiography of science, the *refusal* to endorse the more modern-sounding view can be a powerful tool of pluralism, or even of dissent. Personally I remember feeling that forceful liberating effect of neutrality when I first read Kuhn's account of the Copernican Revolution with his sympathetic description of how the Ptolemaic geocentric system of astronomy worked and made sense.<sup>26</sup>

The claim of neutrality has worked as a useful defensive shield for those historians of science whose dedication to their chosen topics in fact showed a strong critical judgment, namely that there was value in the dusty old systems of science that scientists themselves had discarded as outdated and worthless. But history of science as an academic discipline is by now strong enough to throw off the shield of neutrality against the shallow and harsh judgments of scientists. What we need instead, as Paul Forman put it, is a declaration of *independence*.<sup>27</sup> We can have a self-confident conception of historiography as practiced by free and responsible agents. We should start by admitting our own inevitable rootedness in present society and science. The question is: which part and which version of the present do we choose to take as our platform?<sup>28</sup>

I find inspiration in Forman's exhortation to historians of science to embrace "the obligation to decide for ourselves what is the good of science, and by our historical research and writing to advance that good."<sup>29</sup> He lamented the extraordinary "intellectual subservience accepted by historians of science" in comparison to the situation in history of philosophy, literature, visual art and music, where the historian also functions "as a critic exercising independent

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<sup>26</sup> Kuhn (1957). I do not know whether Kuhn himself intended this effect or not.

<sup>27</sup> Forman (1991).

<sup>28</sup> The critics of whiggism are correct that a history of science that simply adopts current scientific judgment as a framework is not worth having, and such "history" can be left to scientists themselves. But what we need to do is start exercising our own judgment, not pretend that we have no judgment to make.

<sup>29</sup> Forman (1991), p. 86.



judgment".<sup>30</sup> It is important not to mistake this as an anti-science stance; that would be like saying that a movie critic is against movies.

### The functions of history of science requiring critical judgment

Now let me come to those functions of history of science that require engagement with scientific content, with independent judgment. By "functions" here I mean both the inherent aims of our business (the first four on my list), and the applications of the business for other aims (the last three).<sup>31</sup> The point, really, is that there are *many* such functions.

#### (1) Understanding the contingent development of scientific knowledge

First, here is an aim of history of science that maybe all of us can agree on: to make sense of scientific knowledge as a dynamic entity that develops over time through contingencies.<sup>32</sup> This aim is so obvious to historians today that it may not even seem worth stating. But we must not take it for granted. Out there in the wider world, the enlightened common sense still says that scientific knowledge advances inexorably toward the truth. Many scientists, philosophers, journalists and others continue to expound the inevitability of scientific progress and the absoluteness of scientific truth, once it is found. (As Herbert Butterfield said concerning political history: "We must teach history . . . precisely because so much bad history exists in the world already."<sup>33</sup>) The notion of the contingency of scientific knowledge requires continual defence and re-assessment, based on epistemic judgments concerning a growing body of concrete historical episodes.<sup>34</sup>

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<sup>30</sup> Forman (1991), p. 77. I do not know if Forman would approve of the direction of my thought entirely, as he seems to imply that a critical stance in history of science requires a social focus of analysis. However, I do believe that he would be in favor of making independent judgements regarding the quality of scientific knowledge, as he has himself done in his early work, including the classic papers on Weimar physics and quantum electronics.

<sup>31</sup> I am taking seriously the "applied history of science" that John Heilbron (1987) called for, as well as what history of science can do for its own sake.

<sup>32</sup> This is certainly consonant with the aims of the Edinburgh School, the Bath School, and Bruno Latour, and contextualists in general.

<sup>33</sup> Butterfield (1951), p. 171

<sup>34</sup> If, instead, we carry on with blanket statements about the social construction of science and such, we can only end up with a futile and losing battle like the Science Wars.

For myself, a formative experience in this regard was making my own detailed study of (the experiments and arguments that formed the substance of) the Chemical Revolution.<sup>35</sup> Despite my honest efforts, I could not convince myself that there were good enough reasons in the late 18th century to abandon the phlogiston theory; and here I mean “good reasons” according to the standards of judgment that were shared among chemists at the time. Nor could I see any deterministic external causes which could explain Lavoisier’s victory. Therefore I had to face up to the judgment that formed within myself, that the majority of scientists in the late 18th century made a misjudgment concerning phlogiston, whether they were influenced by Lavoisier’s rhetoric, or by a metaphysical bias, or by a kind of group-think, or by anything else. Recognizing this complex contingency of a key event in the history of science had significant effects on my thinking and on my practice as a historian.

## (2) Learning about scientific methods

Next on my list of the functions of history of science is the study of scientific methodology (and its evolution). Historians these days tend to avoid even *mentioning* the scientific method. This is unfortunate, though understandable as an allergic reaction to some very bad philosophy of science. (With my philosopher hat on, I can assure you that many of us in the philosophy of science have been working very hard to move beyond the kind of philosophizing that is repellent to historians.<sup>36</sup>) And this work can connect well with discussions of styles and ways of knowing as discussed by historians such as Alistair Crombie and John Pickstone.<sup>37</sup> It is important that we acquire sensible ideas about the methods of science — partly, again, because there are such bad ideas about them already out there, and they harm science education and science policy. Critical judgment is again essential here, because we cannot discern good and valid methods without making judgments about which instances of scientific work were successful and which weren’t.

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<sup>35</sup> Chang (2012), chapter 1.

<sup>36</sup> For instance, take note of the work of the Society for Philosophy of Science in Practice (<http://www.philosophy-science-practice.org/>).

<sup>37</sup> Crombie (1994); Pickstone (2000).

### (3) Appreciating past scientific knowledge

Now I want to come to two functions of history of science that I think are very unjustly neglected in usual historiographical discussions. Kuhn used to talk about “getting into someone’s head”: aside from being an injunction for historians to lose modern prejudices, I believe that this idiom expressed the sheer joy of understanding how someone from the distant past saw the world. John Heilbron has been explicit in stating that a historian of science should be a *connoisseur*.<sup>38</sup> The renunciation of the pleasures of a connoisseur would be a huge loss to our culture.

What we have here is the business of appreciating past scientific knowledge in itself, for its own sake. In the mode of work that I have called “complementary science”,<sup>39</sup> the recovery of forgotten knowledge is a major aim of historical work. And this covers facts as well as ideas. There is no time to go through this in detail, so I will give you just one brief glimpse at what sort of thing one could learn from long-forgotten past science. When I was doing my research on the history of thermometry, I repeatedly came across 18th- and 19th-century texts reporting that the temperature of boiling water depended on a whole variety of circumstances: even pure water under standard atmospheric pressure would boil very differently, depending on how exactly it was boiled.<sup>40</sup>

After finishing my historical research, I could not resist the temptation to do my own experiments to see if these reports were true, and I was very surprised to learn that I could replicate the reported phenomena quite easily. For example, I followed up on a question posed by Jean-André De Luc of Geneva in 1772, though clearly not his exact experimental arrangement: what happens if one boils water with a low-temperature heat source, which is made possible by cutting down the surface area of water (as a great deal of heat is lost through the surface)?<sup>41</sup> As the temperature of the water approaches 100°C, it starts to boil in

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<sup>38</sup> Heilbron (2007), p. 75. He holds up Sam Schweber as an example of such a connoisseur. See also my discussion of Clifford Truesdell in Chang (2008).

<sup>39</sup> Chang (2004), chapter 6.

<sup>40</sup> Chang (2004), chapter 1.

<sup>41</sup> Like De Luc, I used a flask with a long thin neck, filled up to the lower part of the neck. De Luc’s heat source was an oil bath; I used a hotplate. This can be seen in a video clip (Experiment 5) in my online paper “The Myth of the Boiling Point”

a normal way. As boiling continues, however, the temperature continues to rise, while the bubbles get bigger but less frequent; they also come more irregularly, often in bursts. The temperature goes over 100°C, easily reaching 101–102°C while the boiling is reasonably steady. With continued heating, the bubbles can become even less frequent, while temperature creeps up further. At this stage we can observe what De Luc called "puffing", with long quiet periods punctuated by very large bubbles. Sometimes the puffs are explosive, throwing water out of the flask. The trial shown in this video produced a temperature of 104°C during puffing. This is nothing like boiling as we normally know it, but entirely consistent with what De Luc reported in 1772.<sup>42</sup>

#### (4) Stimulating new scientific knowledge

In addition to recovering neglected knowledge from the past, historical research can *extend* scientific knowledge by stimulating new thinking. Perhaps the most illustrious example of this is in the work of Martin Rudwick, who took methodological ideas from Cuvier to help him in "reconstructing the mode of life of extinct invertebrates, of brachiopods", which was successful despite "hostility and opposition from paleontologists who could not think that anything good could come from someone pre-Darwin, especially an anti-evolutionist whom I was using for evolutionary purposes."<sup>43</sup> In my own work I have noted how recovered scientific knowledge can be extended, as I will illustrate, again, with a very brief example. In my current research on the early history of electrochemistry, I was very interested to learn that Volta used salt water, rather than acids or anything else more exciting, as the electrolyte in his battery. This mundane piece of recovery stimulated my (modern) curiosity about the electrochemical properties of sodium chloride (NaCl) solution, and led to some interesting new experiments. For example, while attempting to learn more about the mechanism of electrical conduction in the solution, I discovered that a gold anode (positive electrode) will dissolve in salt water on applying an

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(<http://www.hps.cam.ac.uk/people/chang/boiling/>), where much further detail and also a discussion of other experiments can be found.

<sup>42</sup> The increasing "bumpiness" of boiling is most likely the result of having more and more dissolved air taken out of the water.

<sup>43</sup> See Meyer (2008) for this quotation and a very brief introductory discussion.

external voltage roughly between 2V and 3V.<sup>44</sup> This phenomenon has surprised every chemist that I have shown it to, and its investigation may yield some interesting new chemical knowledge.

### (5) Enriching science education

Once all of these aims are met, there is much else that we can *do* with history of science. Many people have advocated the introduction of serious history of science in science education; I am aware that this is an active direction of work in the Nordic countries. This is an ideal easily advocated and seldom practiced, despite the successes of some initiatives starting with the Project Physics course by Gerald Holton and colleagues.<sup>45</sup> This is a function of history of science that we cannot afford to neglect. By not connecting better with science education, we historians of science are abandoning millions and millions of science students at all levels being tortured with bad history.<sup>46</sup> And we must also remember that most science teaching is directed to those who will not become professional scientists; it is a civic mission for us to consider how the teaching of science can be improved for the education of better citizens. It should go without saying that such educational functions of history cannot be served without a close critical attention to the content of past science.

### (6) Bridging the “two cultures” gap

History of science can also facilitate the bridging of the gap between different ways of scholarly thinking, so memorably caricatured by C. P. Snow in his description of the “two cultures”, which still has a resonance despite the severe critique from David Edgerton and others.<sup>47</sup> Situating science in its cultural contexts helps, but that needs to be complemented by a content-side strategy. A true cultural understanding of the worth of science requires an empathetic appreciation of its content; the history of science provides the most

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<sup>44</sup> At a *higher* voltage, the gold anode stays intact and chlorine gas is produced on it. For a more detailed description of the line of research leading up to this experiment, see Chang (2011), sections 4 and 5.

<sup>45</sup> This work is described in a retrospective by Holton (2003).

<sup>46</sup> Although it may be too optimistic to think that history can help the education of future scientists in a straightforward way, it would also be foolish to declare at the outset that historical awareness cannot be helpful to the work of scientists.

<sup>47</sup> Edgerton (2005).

suitable material for such scientific education of non-specialists. In the other direction, what better humanist education of scientific people could there be, than a convincing demonstration that the very content of long-established scientific knowledge embodies contingent human actions and strivings?

### **(7) Challenging the authority of scientists**

Finally, an important function of history of science, and other areas of science studies in general, is to challenge the authority of scientists where necessary (and help enhance it where appropriate). Scientists, like all other figures of authority, need external scrutiny by well-informed people with good judgment. But questioning scientific orthodoxy is not something one can do lightly, and judgment must always be made responsibly. If we want to be critical of scientists, it is all the more necessary that our work should be grounded in a sound understanding of the content and methods of science. We need to get into the heart of the content of the relevant science, and command the respect of those whose authority we may feel compelled to challenge. For example, witness James Cushing's painstaking technical historical work showing the contingency involved in the establishment of the Copenhagen interpretation of quantum mechanics.<sup>48</sup> Even when the debate is about present science rather than past science, the knowledge of history gives us perspective and judgment, and exactly the kind of judgment that practicing scientists tend to lack.

### **Neglect of these functions lead to self-imposed impoverishment**

All seven of the functions I have discussed are important, and they can only be served through detailed attention to scientific content. By neglecting these functions I think history of science impoverishes itself and diminishes its own significance unnecessarily. We cannot let methodological fashion or dogmatism limit the potential of our field to benefit scholarship and society. There is too much at stake. And it is not impossible to learn the science that we need. (Surely anyone can learn the phlogiston theory, much more easily than learning Latin or Chinese, the sort of thing historians do routinely.<sup>49</sup>)

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<sup>48</sup> Cushing (1994).

<sup>49</sup> It is just defeatist nonsense to say that science is "too hard" and that it is therefore unreasonable to expect historians with no previous scientific training to engage with its

I admit, of course, that there are other functions of history of science that do not revolve around scientific content. But attention to content is not useless or futile even for those who want to concern themselves exclusively with the study of science as a social, cultural, political, economic or material phenomenon. Even a purely anthropological study would need to be informed by some understanding of the beliefs and practices of the tribe of scientists at the heart of one's study. An anthropologist studying another society's religious practices, for example, would never suggest that the doctrinal content of the religion, and the aspects of the rituals tied to the doctrines, were irrelevant to their study. In this way, interestingly, the content of science becomes a crucial context — for the study of the context! I say, *content is the new context*. I call it the “knowledge context”: when we are learning about science as a socio-cultural activity, it is surely relevant contextual information that the chief aim of that activity on the part of the actors themselves is the acquisition of knowledge (and that they consider themselves to be learning such-and-such).<sup>50</sup>

#### 4. Reasons for doing history, applied to history of science

Having stressed the distinctive functions of history of science, let me now briefly offer a perspective from another angle. History of science is, after all, a kind of history, so let's first think about how to do good history in general, and apply that thinking to history of science in particular. [In other words, let's now follow those who would make history of science part of general history, and consider what good history would look like, when it is about science.] And we cannot rationally<sup>51</sup> discuss how to do good history, without considering *why* we

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content. Historians are smart people, and they learn and deal with hard things all the time. Not everyone is going to learn quantum field theory overnight, but there is plenty of science in its long history that is not technically impossible for serious humanists to engage meaningfully with. To handle the parts that are technically more demanding, we do need to train up those who can handle them; a sensible division of labor is needed here, as it is elsewhere.

<sup>50</sup> See Warwick (2003), p. 44.

<sup>51</sup> Here I only mean the “thin” or instrumental sense of rationality which demands that we ought to do what is conducive to the achievement of our aims, whatever they might be.

do history. [Surveying the writings of various historians,<sup>52</sup> I have identified five general reasons for doing history. Each of those reasons dictates that when the object of our historical study is science, we must engage with its content. So I will come to the same broad conclusion again, starting from general historiographical concerns.] It will be seen that all the purposes of history that I have identified, when they are applied to history of science, dictate a close engagement with scientific content.

### (1) We want to describe the past

So, why do we want to do history? First of all, it may be because we want to describe the past, as faithfully or truthfully as we can (setting aside the deeper question of what “truth” shall mean). [This may be “for the record”, or for the sake of having a kind of esoteric knowledge. Either way, it should be uncontroversial that a full description of the past of science must include a description of the state of scientific *knowledge*, at each stage and location of its development.]

### (2) We want to understand the past

Going beyond description, we may take the purpose of history to be reaching an *understanding* of the past. [This may be broadly in two different senses. For an empathetic understanding (perhaps best expressed in the German notion of *Verstehen*), which involves seeing the world fully in the actor’s categories, it goes without saying that knowing the content of past science is crucial. For a more externalist, explanatory type of understanding, looking not so much for empathy but for causes and influences, the content of science may not be so central. But content must still constitute an important part of the *explanandum* (what we want to explain).]

### (3) We want to use the past

Description and understanding aside, there is a persistent impulse to *use* the past and its knowledge for other purposes. This is risky business, but all we can do is try to do it well, rather than avoid it and let unprofessional or

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<sup>52</sup> Vaughn, ed. (1985) and Tosh, ed. (2009) are useful anthologies for this purpose.



unscrupulous people do it badly. [There is much debate about what kind of “lessons” or at least “experience” can be gained from the past, and it is widely accepted that history *is* routinely used to give individuals and groups a sense of identity. Applying these thoughts to history of science brings us back to science policy and science education, and also the place of history of science in the life of practicing scientists, whether it be for their self-identity or for drawing any lessons for their practice. For myself, as mentioned earlier, another important use of history is to improve scientific knowledge itself. It is not difficult to see that the usefulness of history of science would be enhanced by a serious engagement with the content of science.]

#### **(4) We want to overcome the past**

A particular use of history, which I set aside under a separate heading here, is to allow the understanding of the past to *liberate* us from the past itself. Contingent decisions made in the past shape our present; identifying those elements of past contingency allows us to see that what may seem like an oppressive necessity or inevitability in our present situation actually was, and can be again, a matter of human choice. This rather psychoanalytic insight is perhaps not a mainstream view in historiography, but it has a very respectable pedigree, including Johann Wolfgang von Goethe, Wilhelm Dilthey, Henri-Irénée Marrou, and Benedetto Croce, who gave us the famous dictum: “Only historical judgment liberates the spirit from the pressure of the past.”<sup>53</sup> Our scientific life, too, can benefit from understanding the apparent necessity of today as a consequence of past contingency. And for this the historian must delve into the details of the arguments that took place among past scientists.

#### **(5) We want to revive and appreciate the past**

Finally, there is a particular kind of understanding of the past that I want to set aside as a separate purpose. [An exuberant expression of this purpose comes from Henri Marrou: “I will assign to history, as one of its essential

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<sup>53</sup> Croce (1941), p. 48. This was quoted by Kuhn *et al.* (1967) as a guiding thought in the preface to their description of the Archive for the History of Quantum Physics. They did not elaborate on what kind of liberation concerning quantum physics they were seeking, but I think this vision is expressed clearly in such works as Cushing (1994) and Heilbron (1985).

functions, the enrichment of my internal universe by recapturing cultural values [valuables] salvaged from the past”; they exist “in the bosom of lost societies or civilizations. But to the extent that we are capable of grasping and understanding them, they again come to life in us. In a sense, they acquire a new reality and a second historical existence, in the womb of the historian’s thought and in the contemporary culture to which he reintroduces them.”<sup>54</sup> More briefly,] Barthold Niebuhr says: “he who calls what has vanished back again into being, enjoys a bliss like that of creating.”<sup>55</sup> There is no reason why these insights should not apply in history of science, as I discussed earlier in terms of recovery and connoisseurship.

## Concluding thoughts

I would like to end on a pluralistic note. In this lecture I have pushed strongly a particular vision of the functions of the discipline of history of science, and tried to argue that *critical* attention to the *content* of science is required for serving those functions. But there are many diverse modes of study included within my vision, and I also certainly do not mean to suggest that those modes of study falling outside my vision are invalid or inferior. There are no enemies here, except for those who are in the habit of making enemies, declaring that anything that doesn’t fit into their own narrow-minded view of good scholarship is “not history” or “not real history of science”, and therefore not worth pursuing. The general plea I would like to make is for more conscious deliberation and debate about why we do history of science (and more broadly, history of learning), and how we should do it in order to achieve whatever aims we have. Such considerations will inevitably have to examine the relationship between history of science and science itself, and I think a much more vigorous debate of this issue is crucial to the health of our discipline. My own view on that crucial relationship is that history of science should approach science with understanding and respect, even love, but also with critical independence and confidence.

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<sup>54</sup> Marrou (1959), 260–261. I thank David d’Avray for pointing me to Marrou’s work.

<sup>55</sup> Niebuhr, *History of Rome*, quoted by Charles Lyell, quoted by Roy Porter, quoted by Jim Secord (2004), p. 672.

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