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Internal History versus External History

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Abstract

The aim of this paper is to generalize a pair of concepts that are widely used in the history of science, in art history and in historical linguistics – the concept of internal and external history – and to replace the often very vague talk of 'historical narratives' with this conceptual framework of internal versus external history. I argue that this way of framing the problem allows us to see the possible alternatives more clearly – as a limited number of possible relations between internal and external history. Finally, I argue that while external history is metaphysically prior to internal history, when it comes to historical explanations, we need both.

> 'Examined close up, our history looks rather vague and messy, like a morass only partially made safe for pedestrian traffic, though oddly enough in the end there does seem to be a path across it, that very "path of history" of which nobody knows the starting point.' –Robert Musil[1]

1. Introduction

Here is a very general problem about historiography: what historians have access to is a series of seemingly unconnected events unfolding in time. This series of events does not itself provide any historical explanations: it does not say anything about why something happened or about the significance or lack thereof of specific events. If we want historiography to be more than just an enumeration of 'one damn thing after another', we need some coherent narrative that would explain which events were important and which were not and which events explain which other events.

[1] Robert Musil, *The Man without Qualities* (New York: Random House, 1995), 390–391.

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This is not a new point. To put this in terms of the Musil quote above, if we want to find the 'path of history' in the 'vague and messy morass' of historical events, we need something more. The problem is that this 'something more' is not so easy to find. The main candidates of this 'something more' have traditionally been grand historical narratives, like Hegel's overarching narrative of the Absolute Spirit coming to its own full realization in the course of world history. The worry about grand historical narratives is that they will be too grand to explain the entire series of seemingly unconnected events unfolding in time.

So we have a tension. We need more than 'one damn thing after another'. But we need less than grand historical narratives. And it is not clear how one can carve out an intermediary position. We can chop up grand historical narratives into smaller, more local historical narratives,[2] but this move would inherit the genuine problems of fitting seemingly unconnected events into a coherent whole; it simultaneously threatens to leave out those events that do not fit neatly into the narrative (grand or less grand). We can also embrace the 'one damn thing after another' approach and reject all attempts at finding coherence in them as dangerous and inherently distorting ideologizing. Or we can embrace the narratives and ignore those pesky historical events that do not seem to fit – an extreme example of this would be Hayden White's approach.[3]

My aim in this paper is to seek clarity in these questions by generalizing a pair of concepts that are widely used in the history of science, in art history and in historical linguistics – the concepts of internal and external history – and to replace the often very vague talk of 'historical narratives' with this conceptual framework of internal versus external history. I argue that this way of framing the problem allows us to see the possible alternatives more clearly – as a limited number of possible relations between internal and external history. Finally, I conclude that while external history is metaphysically prior to internal history, when it comes to historical explanations, we need both.

[2] See S. Mannava, 'Micro-narratives compensating the omission of grand historical narratives', *Procedia – Social and Behavioral Sciences* **158** (2014): 320–325 for a summary.

[3] H. White, *Metahistory: The Historical Imagination in Nineteenth-Century Europe* (Baltimore: Johns Hopkins University Press, 1973). H. White, 'The value of narrativity in the representation of reality', *Critical Inquiry* **7** (1980): 5–27. H. White, 'The question of narrative in contemporary historical theory', *History and Theory* **23** (1984): 1–33.

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2. The internal vs. external history distinction generalized

The concepts of internal and external history are used widely in three historical subdisciplines: the history of science, art history and historical linguistics. And, to make things a bit more complicated, these concepts are used somewhat differently in each of these fields.

In art history, the distinction is associated with the work of Heinrich Wölfflin, who insisted that when writing the history of art, we need internal history and internal history should be logically prior to external history. By internal history, he meant the history of our mental capacities: the history of human vision, of attention, of imagery. Wölfflin famously held that 'Vision itself has its history, and the revelation of these visual strata must be regarded as the primary task of art history'.[4] He later clarified that: by 'vision' he means not only the sheer uptake of visual information, but rather the complex functioning of our perceptual system and its interaction with our cognition, which includes attention and mental imagery.

What matters from our point of view is that according to Wölfflin, we can only write the history of, say, 16th-Century painting if we take into consideration how people in the 16th Century looked at paintings. His most explicit statement comes from the chapter entitled 'The external and the internal history of art': 'visual perception is a living faculty with its own internal history and many phases behind it'.[5]

This general approach is not specific to Wölfflin and many art historians who would not have wanted to be associated with Wölfflin's project could be described as being engaged in the same project inasmuch as they were trying to write the internal history (of how people in certain periods and region perceived pictures). The most important example is Michael Baxandall's book, *Painting and Experience in Fifteenth Century Italy*, which discusses how 15th-Century educated Italians looked at pictures.[6] The same attitude is present in the current 'postformalist' approaches, which emphasize the twofold interactions between the history of artworks (and artifacts in general) and the history of the ways people engaged with, perceived and created these artworks (and artifacts).[7]

[4] Heinrich Wölfflin, *The Problem of the Development of Style in Early Modern Art* (Los Angeles, CA: Getty Research Institute, 1915/2015), 11.

[5] Ibid., 305.

[6] Michael Baxandall, *Painting and Experience in Fifteenth Century Italy* (Oxford: Oxford University Press, 1972).

[7] See David Summers, *Real Spaces* (London: Phaidon 2003); Whitney Davis, *A General Theory of Visual Culture* (Princeton: Princeton University

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One salient problem with this proposal is that we do not have very good access to the history of the mental capacities of people who lived long ago. We cannot merely use the works of art they created (and looked at) as evidence for making inferences about their mental states (see Ernst Gombrich's argument for this in *Art and Illusion*). But as Baxandall's methodology shows, this problem can be avoided if we pay attention to written sources about how these subjects engaged with artworks.[8]

The concepts of internal and external history are used very differently in historical linguistics: there, the internal history of a language encompasses, for example, the changes in its syntax and grammar, whereas external history pertains to the full series of events of language-users using this language. The external history of a language is vast: it should include all the events of anyone uttering anything in this language. Internal history is much more manageable, but, as in the art historical case, we get an epistemic problem – namely, that the grammar or syntax of a language are not observable entities and changes in them are not observable events.

The most philosophically sophisticated discussion of the concepts of internal and external history emerges in the history of science. Here, the dilemma is the following: Can the history of scientific thought be reduced to the history of the utterances and actions of scientists? Or should we think of the history of scientific thought as, in some sense, independent from the history of scientists?

If the history of scientific thought is just, or is reducible to, the history of scientists, then it seems that philosophy of science has little to do with the history of science. It may (and hopefully does) learn from the history of science, but philosophy does not (and should not) influence the way history of science is done. By describing the actions and motives of scientists, as well as the relevant sociological, institutional and cultural background, we get a full picture of the history of science. (This approach will have little patience for some of the classic topics of the philosophical history of science that dominated the 60s and 70s of the last century, such as the rationality of theory change.)

Press, 2011); Whitney Davis, 'Succession and Recursion in Heinrich Wölfflin's Principles of Art History', *Journal of Aesthetics and Art Criticism* **73** (2015): 157–64, see also Bence Nanay, 'The history of vision', *Journal of Aesthetics and Art Criticism* **73** (2015): 259–271.

[8] See esp. Michael Baxandall, *Giotto and the Orators*. (Oxford: Clarendon, 1971) and M. Baxandall, *Painting and Experience in Fifteenth Century Italy*, Chapter 2.

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At the same time, we might think that the history of scientific thought is (in some sense) independent from the history of scientists. Taking this route often amounts to interpreting science as a system of ideas or thoughts that develops according to its own logic. On this model, we do not have to take into consideration the actual actions and motives of individual scientists in order to write the history of science.

This is where the concepts of internal and external history come in.[9] Imre Lakatos takes the external history of science to be a socio-psychological narrative that describes the utterances and actions of scientists, together with their institutional background. Internal history, in contrast, is taken to be a description of the history of scientific thoughts and ideas and of 'objective scientific growth'.[10]

According to Lakatos, every event in the history of science has two descriptions, an internal and an external one. Take the 'modern synthesis' of evolutionary biology, for example. The external historian would examine the various intellectual influences of specific scientists, for example, of Ronald Fischer, Theodosius Dobzhansky, Ernst Mayr, as well as the interactions between them. The internal historian, in contrast, would talk about the combination of two scientific theories, Darwin's theory of natural selection and population genetics. The specific scientists and their interactions will not play any role in this latter narrative.[11]

[9] Imre Lakatos, 'Falsification and the methodology of scientific research programmes', in: I. Lakatos and A. Musgrave (eds) *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), 91–195. Imre Lakatos, 'History of science and its rational reconstruction', B*oston Studies in the Philosophy of Science* **8** (1971): 91–136. Lakatos's way of using these terms is highly idiosyncratic (see Lakatos: 'History of science and its rational reconstruction', 123, n. 1; Ian Hacking, 'Imre Lakatos's philosophy of science', *British Journal for the Philosophy of Science* **30** (1979): 381–410, at 394. Ian Hacking, *Representing and Intervening* (Cambridge: Cambridge University Press, 1983), 122: it is very different from what intellectual historians mean by internal and external history (see Steven Shapin, 'Discipline and bounding: The history and sociology of science as seen through the externalism-internalism debate', *History of Science* **30** (1992): 333–369 for a good summary of the many ways this distinction is used by intellectual historians).

[10] I. Lakatos, 'Falsification', 180. Note that Lakatos would count the beliefs and motives of individual scientists as part of external history – Wölfflin probably would not (see Wölfflin: *Principles*, 305).

[11] Lakatos had an idiosyncratic view on the relation between the internal and external history of science, which I will return to in Section

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We have seen the differences between the ways in which the concepts of internal and external history are used in art history, historical linguistics and the history of science. On the basis of these, we are now in a position to zero in on the common denominator in the use of these concepts and generalize the concept of internal and external history beyond the scope of these three subdisciplines.

By *external history*, I mean the sum total of observable events that took place at a certain place, in a certain historical period, and that are of a certain kind. So, for instance, the external economic history of South-East Tanzania in the 1960s is the sum total of observable events of an economic nature, which that took place in South-East Tanzania in the 1960s. It is important that external history is the sum total of all observable events (restricted to a time a place and a kind) – not just of all observed events. Lakatos's and Wölfflin's concept of external history as well as the one used in historical linguistics all fall under this concept.

In contrast, by *internal history*, I mean a chain of unobservable events, where the individual events stand in an explanatory relation to one another. Wölfflin's history of vision would qualify as internal history as would the changes in syntax and grammar that historical linguists talk about. And Lakatos's 'growth of objective thought' would also count as internal history in this sense. Crucially, the grand historical narratives of a Hegelian kind would also count as internal history.

The question I now want to turn to concerns the nature of the *relation* between internal and external history.

3. The relation between internal and external history

What do historians do? Focus on external or internal history? I take it to be relatively uncontroversial that they need to focus on both and the main aim of this paper is to try to understand the relation between internal and external history. However, in order to do so, I need to address the possibility of doing without either external or internal history. I take both of these options to be problematic. Equating history with external history only and ignoring internal history altogether would turn historiography into the listing of

5.1. But we do not need to accept his views about the relation between internal and external history to use the concepts themselves.

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meaningless and unconnected events. Even more troublingly, as external history is supposed to be the history of all observable events (at a certain period and place of a certain type), historians will not be in a position to include those events that are observable but not actually observed. In this sense historians are always in a fairly hopeless position to write exhaustive external history – even of the narrowest time period of the smallest location from a very restricted aspect.

It is not too difficult to find examples for external history of science that ignore (or pay little attention to) internal history; most historians working in the tradition of 'sociology of science' follow this methodology. A famous and extreme example is Bruno Latour and Stephen Woolgar's *Laboratory Life*.[12] Latour, who has no training in molecular biology, spent a couple of years observing the research on growth hormones in a molecular biology lab at the Salk Institute in San Diego. In their description of laboratory life, the authors actively and deliberately ignored the content of the research that was undertaken in this lab. Similar (but maybe less radical) methodology has been used in describing more distant episodes in the history of science.[13]

The sociological approach to the history of science can be, and has been, used in more or less radical fashion. The one I have been focusing on here is the more radical version, the one that denies the relevance of internal history.[14] One problem with this radical version is that ignoring internal considerations makes it difficult to describe what is going on in external history. If a theory T implies a claim C, then if we describe a scientist who accepts T, it is easy to explain why she holds C with reference to this piece of internal history. If we cannot use internal history, then the reason why the scientist holds C needs to be explained in terms of the scientist's psychological history. This will look even more difficult if C is not a claim that the scientists holds explicitly, but rather a claim she takes for granted because she (explicitly) accepts T. In this case, internal history can

[12] Bruno Latour and Steve Woolgar, *Laboratory Life* (Beverly Hills, CA: Sage Publications, 1979)

[13] Another famous example is Steven Shapin, 'The history of science and its sociological reconstruction', *History of Science* **20** (1982): 157–211. Shapin, Steven and Simon Schaffer, 'Leviathan and the Air Pump', (Princeton: Princeton University Press, 1985) for an important theoretical/methodological manifesto for the sociology of science approach.

[14] Latour and Woolgar's *Laboratory Life* is a good example.

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help us to explain why she takes C for granted, but it is difficult to see how external history alone can do so.

Ignoring external history and equating history with internal history would lead to similarly implausible results. Given that internal history is a series of unobservable events, we need to postulate these events on the basis of something. If they are not postulated on the basis of observable events (which would be members of the set of observable events that make up external history), this would lead to a way of writing history that is completely detached from the data we have about the place and period the history of which we are trying to write.

One example for this purely internalist approach is implied by some evolutionary approaches of scientific change. It has been suggested that, like evolved organisms, scientific theories compete for survival and reproduction: both science and natural selection proceed by trial and error. In the same way that we can talk about natural selection among organisms and lineages, we can talk about selection among scientific theories. There are various ways of substantiating this evolutionary analogy.[15] But the one that is relevant for our purposes is the meme theoretical reconstruction of the history of science.[16]

[15] Some famous examples include Karl R. Popper, *The Logic of Scientific Discovery* (London Routledge, 1959/2002); Karl R. Popper, *Conjectures and Refutations* (London Routledge, 1963); Karl R. Popper, *Objective Knowledge* (Oxford: Clarendon, 1972); Karl R. Popper, 'The rationality of scientific revolutions', in R. Harré (ed.) *Problems of Scientific Revolutions* (Oxford: Clarendon, 1975), 72–101 (reprinted in Popper: *The Myth of the Framework* (London: Routledge, 1996)); Karl R. Popper, 'Natural selection and the emergence of mind', *Dialectica* **32** (1978): 339–355; S. Toulmin, 'From logical systems to conceptual populations', *Boston Studies in the Philosophy of Science* **8** (1970): 552–564; S. Toulmin, 'The evolutionary development of natural science', *American Scientist* **55** (1967): 456–471; S. Toulmin, *Human Understanding* (Oxford: Clarendon, 1972); Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1972), 172; Bas Van Fraassen, *The Scientific Image* (Oxford: Oxford University Press, 1980), 39–40; D. L. Hull, 'A mechanism and its metaphysics: An evolutionary account of the social and conceptual development of science', *Biology & Philosophy* **3** (1988): 123–155; David L. Hull, *Science and Selection*, (Cambridge University Press, Cambridge, 2001). See Michael Bradie, 'Assessing Evolutionary Epistemology', *Biology & Philosophy* **1** (1986): 401–459 for a typology.

[16] See D. L. Hull, 'A mechanism and its metaphysics'; D. L. Hull, *Science and Selection*; Robert Aunger, *The Electric Meme: A New Theory*

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According to this proposal, the history of science, like other cultural phenomena, can be explained, at least partially, with the help of the following evolutionary model: Memes are pieces of information and they compete for survival in a similar way to genes; the difference is that they compete for mental space. Since the capacity of the human mind is limited, only some of them, the successful ones, manage to get into the minds of numerous people, hence, they survive, whereas the unsuccessful ones die out. A meme can be a tune, the idea of liberalism, or the habit of brushing one's teeth. Those tunes that can get into and stay in many minds will survive. The ones that fail to do so will die out.[17] This general explanatory model can be applied to the history of science as well: scientific theories are memes: they spread from the mind of one scientist to the other.

Meme theory in general has been severely criticized,[18] but even if we assume there are such things as memes, even if we also assume that we can talk about something like selection among them, the meme theoretical reconstruction of the history of science will still look problematic. If the history of science can be described in terms of the selection pressures of meme selection, then scientists themselves are left out of this process altogether. They are the vehicles of memes at best and the important and explanatory relevant

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of How We Think and Communicate (New York: Free Press, 2002); Kate Distin, *The Selfish Meme* (Cambridge: Cambridge University Press, 2005).

[17] Richard Dawkins, *The Selfish Gene* (Second edition, Oxford University Press, Oxford, 1976/1989); Richard Dawkins, *The Extended Phenotype* (Oxford: W. H. Freeman, 1982); Richard Dawkins: 'Replicators and Vehicles', reprinted in: R. N. Brandon and R. M. Burian (eds) *Genes, Organisms, Populations: Controversies over the Units of Selection* (Cambridge, MA: The MIT Press, 1984); Daniel C. Dennett, *Darwin's Dangerous Idea* (New York: Touchstone, 1995); Daniel C. Dennett, *Freedom Evolves* (New York: Viking, 2003); Daniel C. Dennett, *Breaking the Spell: Religion as a Natural Phenomenon* (New York: Viking, 2006).

[18] Dan Sperber, 'An objection to the memetic approach to culture' in Robert Aunger (ed.) *Darwinizing Culture: The Status of Memetics as a Science* (Oxford: Oxford University Press, 2000). W. C. Wimsatt, 'Genes, Memes, and Cultural Heredity', *Biology and Philosophy* **14** (1999), 279–310; Peter J. Richerson and Robert Boyd, *Not by Genes Alone: How Culture Transformed Human Evolution* (Chicago: University of Chicago Press, 2005); Kim Sterelny, 'The Evolution and Evolvability of Culture', *Mind & Language* **21** (2006): 137–165; Kim Sterelny, 'Memes Revisited', *British Journal for the Philosophy of Science* **57** (2006): 145–165.

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causal relations are to be found among memes, not scientists. If we take the history of science to be the history of memes, then this gives us an example of purely internal history that ignores any external factors. The history of science, interpreted this way, bypasses scientists.[19]

The unsurprising upshot of this is that historians should pay attention both to internal and to external history. But the real question is how historians should think about the *relation* between external and internal history. And here, I want to distinguish two questions, one metaphysical and one explanatory. First, we can inquire about the metaphysical relation between internal and external history. Second, we can also inquire about the explanatory relation between them. These two questions are very different and the answer we can give to them can also be very different.

4. The metaphysical relation between internal and external history

The first question I want to raise is about the metaphysical relation between internal and external history. Do both exist? Or should we consider one of them as a theoretical construct while withholding claims about its existence. And if both of them exist, should we consider one of them as metaphysically prior? I will argue that there is a metaphysical asymmetry between internal and external history (but the nature of this asymmetry very much depends on one's more general metaphysical commitments).

From the definition of external history, it follows that external history exists: it is the sum total of events that exist – they are even observable. So we can safely assume that external history exists. A trickier question is whether internal history exists and here it is unlikely that we can decide this question without bringing in very general and extremely contested metaphysical assumptions.

Take the internal history that art historians like Wölfflin talk about: the history of the mental states of people involved in making and consuming artworks. To decide whether these mental states are 'real' in some sense would involve getting entangled in old and difficult debates about the sense in which mental states can be said to exist.

[19] Selectionist explanations of scientific change that do not help themselves to the concept of meme are also subject to the same criticism. See especially Stephen Toulmin's evolutionary account, especially Toulmin, 'From logical systems to conceptual populations', 560–564.

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Settling questions about the metaphysics of internal history in the context of the history of science is even more complicated. Lakatos clearly believes that the unobservable events of the internal history of science *do* exist in the most demanding sense of the term:

> The – rationally reconstructed – growth of science [that is, internal history] takes place essentially in the world of ideas, in Plato's and Popper's third world, in the world of articulated knowledge which is independent of knowing subjects.[20]

I will come back to the last sentence where he claims that internal history is independent from observable events and focus on Lakatos's insistence that internal history is part of Popper's third world.[21] But what does he (or Popper) mean by the third world? According to Popper, 'the third world is man-made and, in a very clear sense, superhuman at the same time. It transcends its makers'.[22] Further, it seems to have causal powers, independent of the causal powers of the second world.[23]

In the quote above (and elsewhere) Lakatos seems to equate the third world with the heaven of Platonic Forms. As he says in one of his last papers, 'the third world is the Platonic world of objective spirit'.[24] Elsewhere, however, he takes the third world to be the world of propositions: 'the "first world" is that of matter, the "second" the world of feelings, beliefs, consciousness, the "third" the world of objective knowledge, articulated in propositions'.[25]

This neatly demonstrates how difficult it is to settle questions about the existence of internal history. But luckily we do not need to do so. If internal history does not exist then there is a clear metaphysical asymmetry between internal and external history – the latter exists, whereas the former does not. But if we assume that internal history *does* exist, we still get a form of metaphysical asymmetry. This takes us to the question of whether or not internal or external history should be considered to be

[20] I. Lakatos, 'Falsification', 180.

[21] Not just in the quoted passage, but also in Lakatos, 'A postscript on history of science and its rational reconstruction', in Imre Lakatos, *The Methodology of Scientific Research Programmes* (eds) John Worrall and Gregory Currie. (Cambridge: Cambridge University Press, 1978), 189–192 and Lakatos, 'History of science', 179.

[22] K. Popper, *Objective Knowledge*, 159.

[23] Ibid.

[24] I. Lakatos, 'A postscript', 128.

[25] I. Lakatos, 'History of science', 127, footnote 61.

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metaphysically prior. And this again takes us to the hotly debated terrain of what it means to say that something is metaphysically prior to something else.

One take would be to focus on grounding and ask whether internal history grounds external history or the other way round. Less controversially, one might ask whether internal history supervenes on external history and whether external history supervenes on internal history. I take it to be uncontroversial that external history does not supervene on internal history: if there is a change in external history, it does not follow that there is also a change in internal history. Even those who attribute serious importance to internal history (for example, Lakatos) would be happy to accept this: internal history abstracts away from some of the details of external history, so changes in these details will not result in any changes in internal history. In short, internal history does not supervene on external history.

But the converse claim is more interesting and more controversial. Can internal history change without any changes in external history? If not, then internal history supervenes on external history. I take it that the standard picture is that internal history supervenes on external history, but on the face of it, one might think that this view is not without opponents.

As we have seen, Lakatos is very clear that internal history is ontologically independent from external history. According to him, internal history is about 'third world' entities: entities that are 'not dependent in the slightest on the scientists' beliefs, personalities or authority'.[26] Much of the discussion about Lakatos's history of science takes this ontological independence of internal history for granted, whether or not they side with Lakatos.[27]

Some defenders of Lakatos's approach to the history of science have embraced his claims about the 'third world'.[28] But it is difficult not to agree with Ian Hacking who writes that these claims about the ontology of internal history are part of the reason why Lakatos's

[26] Ibid, 106.

[27] See, e.g. Y. Elkana, 'Boltzmann's scientific research programme and its alternatives', in Y. Elkana (ed.) *The Interaction between Science and Philosophy* (New York: Free Press, 1974), 242–297, at 245; Tomas Kulka, 'Some problems concerning rational reconstruction: Comments on Elkana and Lakatos', *British Journal for the Philosophy of Science* **28** (1977): 325–344, at 331.

[28] Notably E. Palmer, 'Lakatos's "Internal history" as historiography', *Perspectives on Science* **1** (1993): 603–626.

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entire project has often been treated with suspicion. As Hacking says, 'Lakatos's internal history is […], in short, to be a history of Hegelian alienated knowledge, the history of anonymous and autonomous research programmes'.[29] Hacking's words may seem strong (especially in the light of Lakatos's criticism of Toulmin for his excessive Hegelian vision of history),[30] but some of Lakatos's early writings seem to show that Hacking was right. Here is what Lakatos says in *Proofs and Refutations*:

> Mathematical activity is human activity. Certain aspects of this activity – as of any human activity – can be studied by psychology, others by history. […] But mathematical activity produces mathematics. Mathematics, this product of human activity, 'alienates itself' from the human activity which has been producing it. It becomes a living growing organism that acquires a certain autonomy from the activity which has produced it.[31]

Thinking of objective knowledge as 'a living growing organism' sounds Hegelian indeed, but from our point of view the question is whether it also amounts to denying that internal history supervenes

[29] I. Hacking, *Representing and Intervening*, 122.

[30] Toulmin insisted that evolution should be more than a mere metaphor when we describe the progress of science (Toulmin, 'From logical systems to conceptual populations', 560–564). It is not enough to compare the trial and error method of science to the trial and error method of natural selection. The evolutionary model is indeed explanatory (Toulmin, 'The evolutionary development of natural science', 470): selection among scientific theories explains some of the features of these theories, most importantly, their survival. Lakatos's main problem with Toulmin's account is that this selectionist explanation bypasses scientists and philosophers, very much like the cunning of Hegelian reason. In other words, he seems to be criticizing Toulmin for ignoring external history – something Lakatos himself is often accused of. The most detailed account of Lakatos's problems with Toulmin's evolutionary explanation is in Lakatos, 'Toulmin's Wittgensteinian epicycles' (manuscript in the Lakatos archive, file number 8/4). For a shorter summary, see Lakatos, 'Understanding Toulmin', *Minerva* **14** (1976): 126–43 at 137–138. See also his letter to Jon Cohen, who reviewed Toulmin's book in the *British Journal for the Philosophy of Science* (which Lakatos edited) in 1972. Lakatos here explicitly agrees with Cohen's criticism of Toulmin's Darwinism, 'Lakatos to Jon Cohen', October 22, 1972, Lakatos archive, file number 13/166.

[31] Imre Lakatos, 'Proofs and refutations', *British Journal for the Philosophy of Science* **14** (1963/1964): 1–25, 120–139, 221–243, 296–342 at 146.

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on external history. And I am not aware of any claims by Lakatos that would have implied that internal history fails to supervene on external history. In fact, some of his claims suggest the opposite. He insists repeatedly, for example, that 'since external influences always exist, radical internalism is utopian'.[32] In short, it seems that even Lakatos would admit that there is a metaphysical asymmetry between internal and external history: internal history supervenes on external history but not vice versa.

Although Wölfflin has also been accused of being Hegelian,[33] he never explicitly made any claims that would have implied that internal history would fail to supervene on external history. In fact, calling internal history a 'schema' seems to suggest the opposite.[34] And when he says that 'naturally, we were not able to go about exemplifying [internal history] other than by drawing upon the individual work of art' [i.e. external history],[35] this very much sounds like an implicit endorsement of the supervenience claim (that is, the claim that internal history supervenes on external history).

Finally, it is difficult to pin down Hayden White's idiosyncratic stance towards historical narratives in this respect. He is very explicit that internal history (what he calls 'historical narrative') is irreducible to external history.[36] But this would be consistent with the claim that internal history supervenes on external history (as the 'nonreductive physicalist' literature on the supervenience relation in the domain of mental states shows nicely).[37] A charitable interpretation of White's writings (especially those in the early 1980s) is that he endorsed some form of nonreductive monism: he insisted that we, historians, can ignore the pesky details of external history when we put together historical narratives (an epistemic claim), but these historical

[32] Lakatos, 'History of science', 94, see also his tirades against Toulmin, who he accuses of radical internalism – of what could be rephrased as the claim that internal history fails to supervene on external history.

[33] See especially A. Hauser, *Philosophie der Kunstgeschichte* (Munich: Beck, 1958) and also D. Summers, 'Forms: 19th-Century metaphysics and the problem of art historical descriptions', in D. Preziosi (ed.) *The Art of Art History* (Oxford: Oxford University Press, 1992), 127–142 for a summary.

[34] Wölfflin, *Principles*, 305.

[35] Ibid.

[36] H. White, *Metahistory*; H. White, 'The value of narrativity'.

[37] See e.g. Donald Davidson, 'Mental events', in L. Foster and J. Swanson (eds) *Experience and Theory* (Amherst, MA: University of Massachusetts Press, 1970), 79–101.

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narratives are nonetheless grounded in external history (a metaphysical claim).

White's writings are complex and the strength of his claims shifted considerably throughout his life. So if someone is not convinced by this charitable interpretation of his approach as being consistent with the claim that internal history supervenes on external history (or if someone doesn't find this interpretation charitable at all), she can treat White as an exception. But even in this case, White would count as an exception inasmuch as he denies that internal history supervenes on external history. He would nonetheless not deny the metaphysical asymmetry between internal and external history given his repeated insistence on the fictionality of historical narratives and the parallel between literary and historical narratives. He would still consent to the metaphysical asymmetry between internal and external history.

5. The explanatory relation between internal and external history

We have seen that there are good reasons to posit a metaphysical asymmetry between internal and external history in the sense of the former supervening on the latter. The question I am now turning to is whether there is a similar explanatory asymmetry between internal and external history. The first thing we need to notice is that metaphysical priority is very different from, and does not imply, explanatorily priority. Just because A supervenes on B and B fails to supervene on A, this does not mean that B is *explanatorily* prior to A. Take mental states again. If we accept that mental states supervene on physical states, this does not imply that the physical is explanatorily prior to the mental – in fact, very few people in that domain would take the physical to be explanatorily prior.[38]

I want to argue that the explanatory relation between internal and external history is twofold. External history is very important to explain internal history, but, maybe more surprisingly, internal history also plays a very important role in explaining external history.

[38] I. Hacking, *Representing and Intervening*, 124 alludes to the same analogy briefly.

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5.1. External history as explanatorily prior

The less controversial claim is that external history plays an important role in explaining internal history. If we take internal history to be a theoretical construct that we form on the basis of external history, then this is obviously so. But even if we do not take internal history to be a theoretical construct, external history still plays an inevitable explanatory role, even for those, like Lakatos, who are ardent proponents of the independence of internal history.

According to Lakatos, the first step of writing a history of science must be the reconstruction of internal history. This is the step Lakatos calls 'rational reconstruction' (Popper also talks about the rational reconstruction of the history of science; it is unclear who inherited this concept from whom).[39] As Lakatos says, 'whatever problem the historian of science wishes to solve, he has first to reconstruct the relevant section of the growth of objective scientific knowledge, that is, the relevant section of "internal history"'.[40] What he means by rational reconstruction is perhaps more appropriately described as rational construction: there is no guarantee that the rationally reconstructed internal history will correspond to the actual historical facts. Lakatos explicitly acknowledges this: 'Internal history is not just a *selection* of methodologically interpreted facts: it may be, on occasions, their *radically improved versions*'.[41] In other words, rational reconstruction distorts what we know to be the historical facts and this gives rise to internal history.[42]

Lakatos's writings are full of provocative claims about just how distorted this internal history will look. Here is the most famous (or infamous) quote:

> One way to indicate discrepancies between history and its rational reconstruction is to relate the internal history *in the text* and indicate *in the footnotes* how actual history 'misbehaved' in the light of its rational reconstruction.[43]

[39] See e.g. K. Popper, *Objective Knowledge*, 179.
[40] I. Lakatos, 'Falsification', 106.
[41] Ibid.
[42] See Bence Nanay, 'Rational reconstruction reconsidered', *The Monist* **93** (2010): 595–615.
[43] I. Lakatos, 'History of science', 107. Lakatos reiterates this idea about relegating actual history to the footnotes three times on one page in this same paper.

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This quote as well as Lakatos's seemingly dismissive attitude towards what he calls 'actual history' triggered very strong reactions both from philosophers and historians. Thomas Kuhn writes that 'What Lakatos conceives as history is not history at all but philosophy fabricating examples'.[44] Or, more precisely,

> A *historian* would not include *in his narrative* a factual report which he knows to be false. If he had done so, he would be so sensitive to the offence that he could not conceivably compose a footnote calling attention to it.[45]

Larry Laudan's reaction is equally strong: according to him, Lakatos's methodology is 'consciously and deliberately falsifying the historical record'.[46] Gerard Holton is even more negative when he writes about Lakatos's rational reconstruction of Bohr's early work, which he considers to be 'an ahistorical parody that makes one's hair stand on end'.[47]

Before dismissing Lakatos as a bad historian of science and dismissing Lakatos's vision of history of science as either ahistorical or crazy, it is important to remember that rational reconstruction for Lakatos is just the first step of writing history of science. It is not the end of the story. Internal history is not the finished product, but only the first, preparatory stage of the historian's project. Lakatos is so explicit about this that it is striking how many historians and philosophers misinterpret his account.

For Lakatos, the first step of rational reconstruction needs to be followed by a second phase where 'one tries to compare this rational reconstruction with actual history and to criticize both one's rational reconstruction for lack of historicity and the actual history for lack

[44] T. Kuhn, 'Notes on Lakatos', *Boston Studies in the Philosophy of Science* **8** (1971): 137–146 at 143.

[45] T. Kuhn, 'Reflections on my critics', in I. Lakatos and A. Musgrave (eds) *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970), 237–278 at 256.

[46] Larry Laudan, *Progress and its Problems* (Berkeley: University of California Press, 1977), 170.

[47] Gerald J. Holton, *The scientific imagination: Case studies* (Cambridge: Cambridge University Press, 1978), 106, see also Gerald J. Holton, 'On being caught between Dionysians and Apollonians', *Daedalus* **103** (1974): 65–81 at 75, Noretta Koertge, 'Rational reconstruction', in Robert S. Cohen, Paul K. Feyerabend and Marx W. Wartofsky (eds) *Essays in Memory of Imre Lakatos* (Dordrecht: Reidel, 1976), 359–369, E. McMullin, 'The history and philosophy of science: A taxonomy', *Minnesota Studies in the Philosophy of Science* **5** (1970): 12–67.

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of rationality'.[48] Or, as he reiterates, 'any rational reconstruction of history needs to be supplemented by an empirical (socio-psychological) "external theory"'.[49] In short, the history of science is not identical to its internal history (that is, its rational reconstruction). Lakatos is not a radical internalist. In fact, he is very much against radical internalism. As we have seen, he says, 'since external influences always exist, radical internalism is utopian'.[50] According to him, writing history of science requires attention to both internal and external history as well as to the interaction between the two: 'history of science is always richer than its rational reconstruction'.[51] In short, rather than dismissing external history, 'Lakatos merely suggests a colourful way of doing something quite orthodox'.[52]

So if we follow Lakatos, we need both internal and external history in order to write a history of science (and we should only joke about dismissing the latter into footnotes). What is important is that internal history is constructed (or rationally reconstructed) on the basis of external history. And even after this rational reconstruction is completed, it is constantly revised and adjusted in the light of external history. In this sense, it is clear that even for the most vocal proponents of internal history like Lakatos, external history is explanatorily prior to internal history.

5.2. Internal history as explanatorily prior

The more controversial claim is that internal history plays an important and indispensable role in explaining external history. This claim is often associated with Lakatos and for good reasons. He claims that writing external history presupposes internal history.[53] Why? Because 'the most important problems of external history are

[48] I. Lakatos, 'Falsification', 138, n. 40.

[49] I. Lakatos, 'History of science', 91 – this is not a passing remark, but the summary of the third of the three main claims he argues for in this paper.

[50] Ibid, 94.

[51] Ibid, 105.

[52] Alan Musgrave, 'Facts and values in science studies', in Roderick Weir Home (ed.) *Science under Scrutiny: The Place of History and Philosophy of Science* (Dordrecht: Reidel, 1983), 49–80 at 66 – see also the similarly charitable interpretation of James Robert Brown, *The Rational and the Social* (London: Routledge, 1989), 109–111; Ian Hacking, 'Lakatos's philosophy of science', 396; I. Hacking *Representing and Intervening*, 125.

[53] See e.g. Lakatos, 'History of science', 92, 105.

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defined by internal history'.[54] Or, to use his telling metaphor, 'the internal skeleton of rational history defines the external problems'.[55]

In other words, internal history is far from being the history of science. It is only the skeleton of history of science and a lot of work needs to be done to get from the skeleton to the full body of history of science.[56] Nonetheless, although internal history is merely the skeleton of the history of science, it is necessary to have this skeleton to build on. Internal history is a necessary ingredient of any serious history of science.[57]

One may wonder why Lakatos takes internal history to be so important. As Larry Laudan rightly points out, 'Lakatos nowhere establishes the necessity (or the desirability) of making a reconstruction of the past which involves an intentional warping of the historical record'.[58] He gives only one passing remark that could be construed as an argument, which alludes to the fact that the aim here is to explain the history *of science*, a largely rational social enterprise, which needs to be kept separate from the history of other social phenomena. In other words, it is in order to preserve the special status of science that we need to use rational reconstructions. Doing without rational reconstruction would make 'scientific change a kind of religious change'.[59] And this 'would vindicate, no doubt unintentionally, the basic political *credo* of contemporary religious maniacs'.[60]

I want to argue for a version of the claim that Lakatos endorses, namely, that internal history plays an indispensable role in explaining external history, but without relying on the reasons he gives for this claim. For Lakatos, it is only the external history *of science* that needs to rely on internal history (understood, as we have seen, by him as the growth of objective thought). So, for him, the external history of religion, for example, would not need to rely on internal history. I want to part from Lakatos at this point. I will argue that internal history plays an indispensable role in explaining external history not just when it comes to the history of science, but any branches of history.

[54] Ibid, 105, see also I. Lakatos, 'A postscript', 191 and Lakatos, 'History of science', 92 for similar formulations.
[55] I. Lakatos, 'A postscript', 191.
[56] See, e.g., I. Lakatos, 'History of science', 118, I. Lakatos, 'A postscript', 191–192.
[57] See I. Lakatos, 'A postscript', 192.
[58] I. Laudan, *Progress*, 170.
[59] I. Lakatos, 'Falsification', 93.
[60] Ibid.

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I want to give two considerations in favor of this claim. The first one is about observable but unobserved events in external history. As we have seen, external history is the sum total of all observable events (at a certain place and time and of a certain type). And this includes not only those observable events that were observed, but also those that happened not to be observed by anyone. If we are writing the economic history of South-East Tanzania in the 1960s, some important elements of the external history will be trade agreements and records of prices of crops, but the external economic history of South-East Tanzania in the 1960s also encompasses those trade deals that we have no record of. In general, historians are in the unfortunate position that they never have a complete record of what happened. But then how can they account for those events that there are no records about? What can the historian say about observable but unobserved events in external history?

My answer is that they can and often need to use internal history: the series of unobservable events that stand in explanatory relation to each other. On the basis of the documented trade deals in South-East Tanzania in the 1960s, we can identify a pattern of economic activities.[61] But this pattern of economic activities would be something unobservable – it would amount to internal history. And on the basis of these patterns – on the basis of internal history – we can make inferences with regards to those economic activities that we have no record of.

The second reason why internal history plays an indispensable role in explaining external history has to do with the contrast classes of historical explanations. Explanation is always contrastive: explaining why x is *F* rather than G is a different explanatory task from explaining why x, *rather than y*, is F.[62] In other words, explanation is always relative to a contrast class. I am assuming the same is true for historical explanations.

But then the question is what allows the historian to identify the contrast class for her historical explanations. Which contrast classes are worth taking into consideration and which are not? My answer is that the contrast class of historical explanations comes from internal history. And this is the sense in which internal history plays an indispensable role in external history.

Here is an example. Many historians of biology have been trying to explain why Darwin attributed so much importance to Henry

[61] Felicitas Becker, *Becoming Muslim in Mainland Tanzania* (Oxford: Oxford University Press, 2008).

[62] Van Fraassen, *Scientific Image*, 142–143.

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Charles Fleeming Jenkin's objection to his theory of natural selection.[63] Jenkin's paper was published in 1867 and two years later, Darwin wrote that 'Fleeming Jenkin has given me much trouble, but has been more real use to me than any other essay or review'.[64] Jenkin's objection about blending inheritance became perhaps the most important criticism of *The Origin of Species* and even of the theory of natural selection in general (before the 'modern synthesis').[65] Stated very simplistically, the objection is that natural selection cannot explain real evolutionary change since, because of the 'blending' nature of inheritance, variations from the average will be watered down to be closer to the average in the next generation. The question is: why did Darwin take Jenkin's objection to be so important?

How should the external historian begin to address this question? The first thing they should do (and many historians in fact do) is to see how good Jenkin's objection really is and what aspect of Darwin's theory it jeopardized. The standard interpretation of Jenkin's objection is that it made it clear that Darwin was using a mistaken theory of inheritance (the 'blending' theory) and as long as we take inheritance to be blending the traits of the two parents, then Jenkin is correct to point out that natural selection will not be able

[63] Stephen Jay Gould, 'Fleeming Jenkin revisited' in *Bully for Brontosaurus* (New York: W. W. Norton, 1991), 340–353; Michael Bulmer, *Francis Galton: Pioneer of Heredity and Biometry* (Baltimore: Johns Hopkins University Press, 2003), 141–145; Michael Bulmer, 'Did Jenkin's swamping argument invalidate Darwin's theory of natural selection?', *British Journal for the History of Science* **37** (2004): 281–297; S. W. Morris, 'Fleeming Jenkin and The Origin of Species: A reassessment', *British Journal for the History of Science* **27** (1994): 313–343; Peter J. Bowler, *The Eclipse of Darwinism* (Baltimore: Johns Hopkins University Press, 1983); Peter J. Bowler, *The Non-Darwinian Revolution: Reinterpreting a Historical Myth* (Baltimore: Johns Hopkins University Press, 1988); Peter J. Bowler, *Charles Darwin: The Man and his Influence* (Cambridge: Cambridge University Press, 1990); G. Cookson and C. A. Hempstead, *A Victorian Scientist and Engineer: Fleeming Jenkin and the Birth of Electrical Engineering* (Aldershot: Ashgate, 2000).

[64] Letter to Joseph Hooker, in F. Darwin and A. C. Seward, *More Letters of Charles Darwin*, 2 vols. (London: Murray, 1903, vol. 2), 379, see also a similar claim in his letter to Alfred Russel Wallace also in 1869.

[65] See David L. Hull, *Darwin and His Critics*. (Cambridge: Cambridge University Press, 1973) and P. Vorzimmer, 'Charles Darwin and blending inheritance' *Isis* **54** (1963): 371–390; P. Vorzimmer, *Charles Darwin, The Years of Controversy: The Origin of Species and Its Critics, 1859–1882* (Philadelphia: Temple University Press, 1970), for summaries.

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to explain major evolutionary change.[66] This interpretation of Jenkin's argument is consistent with the textbook narrative of the significance of the 'modern synthesis' that replaced Darwin's original concept of inheritance in the theory of natural selection with a Mendelian one. Thus, Darwin was right to take Jenkin's criticism seriously.

But here is an alternative interpretation. Ernst Mayr argues convincingly that Jenkin's paper is deeply confused and it does not present any good objection.[67] Mayr argues that Jenkin failed to grasp what is, according to Mayr, *the* most important element of Darwin's theory, which he labels 'population thinking': the view that in the biological domain individual variation cannot be ignored and subsumed under some fixed and preexisting type. Individual variation is what drives evolution and we can only talk about types that these variations are instantiations of as statistical abstractions.[68] Jenkin assumes that variation is always variation within a type. According to Mayr, this is an instance of the 'typological thinking' or essentialism that Darwin was strongly opposed to. Thus, Mayr concludes, Jenkin misunderstood the most important claim of the theory of natural selection. Darwin could have easily refuted him, but he didn't. In other words, Darwin was completely mistaken to take Jenkin's criticism seriously.[69]

We have two different historical explanations for Darwin's assessment of Jenkin's argument. What matters for our purposes is not which one is correct, but in what way they differ. They differ in as much as they rely on different internal history of the Darwinian revolution. And, as a result, the external historical narrative will also look very different.

If we accept Mayr's explanation and claim that Darwin was wrong to take Jenkin's criticism seriously, then we also need to interpret the

[66] See, for example, R. C. Lewontin, 'How important is genetics for an understanding of evolution?', *American Zoologist* **26** (1986): 811–820.

[67] Ernst Mayr, *The Growth of Biological Thought* (Cambridge, MA: Harvard University Press, 1982), 512–514, see also Philip Kitcher, *Living with Darwin* (New York: Oxford University Press, 2007), 73–75, who also nicely exposes the racist undertones of Jenkin's paper.

[68] See especially Ernst Mayr, 'Typological versus population thinking', in B. J. Meggers (ed.) *Evolution and Anthropology* (Washington: The Anthropological Society of America, 1959), 409–412, see also Elliott Sober, 'Evolution, population thinking, and essentialism', *Philosophy of Science* **47** (1980): 350–383, and Bence Nanay, 'Population thinking as trope nominalism', *Synthese* **177** (2011): 91–109.

[69] See especially E. Mayr, *Growth*, 514.

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related changes Darwin made in later editions of *The Origin of Species* (especially on page 72 of the 1872 edition of *The Origin of Species*) as insignificant.[70] And if we accept his interpretation, then this sets the agenda for future historical research: the next question the external historian should ask is about why Darwin was so mistaken about the vulnerability of his own theory in the face of Jenkin's objection. Mayr's (sketchy) answer is that it is because he was 'rather confused on the topic of variation'.[71]

If, in contrast, we accept the standard narrative about Jenkin's criticism, then the changes Darwin made in the later editions will be considered to be very significant indeed. The agenda for future historical research will also look very different: the next step is likely to involve the understanding of the reasons why Darwin was mistaken about inheritance.

Crucially, what constitutes the difference between these two (external) historical explanations is the way in which Darwin's theory is being 'rationally reconstructed' (as Lakatos would say). For Mayr, the crucial element of this theory is population thinking. That is why, according to Mayr, Darwin should have dismissed Jenkin's objection without thinking twice about it. According to the standard interpretation, however, Darwin's mistaken ideas about inheritance are part of the most central Darwinian claims (to be contrasted with the correct ideas of the 'modern synthesis'). Thus, the two historical explanations differ in what they take to be the internal history of the theory of evolution. And, as we have seen, depending on what we take this internal history to be, we get different directions for future (external) historical research. Further, depending on what we take internal history to be, the course of external history, for example, the change of Darwin's thinking between the earlier and the later editions of *The Origin of Species*, will also look different.

The aim of this brief case study was to show that internal history plays an indispensable role in the writing of external history. Mayr's and the standard historical explanation differs in the assessment of the internal history of Darwin's theory, and, as a result, as different questions about the influence of Jenkin's criticism on Darwin's thinking. This means that they use different contrast classes when they ask why Darwin took Jenkin's criticism seriously. Mayr asks why Darwin took it seriously, rather than just dismissed it as a piece of old essentialist typological thinking. The standard account asks why Darwin was forced to take Jenkin's criticism

[70] See, e.g. Vorzimmer, *Charles Darwin*.
[71] E. Mayr, *Growth*, 514, see also ibid., 681–697.

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seriously, rather than having theory of inheritance that would not have made his theory to be susceptible to it. As a result of this difference in the contrast classes the course of external history of the Jenkin–Darwin episode (as well as the further questions it raises) will also be very different.

6. Conclusion

I argued that internal history plays an indispensable explanatory role in writing external history and external history also plays an indispensable explanatory role in writing internal history. So the metaphysical asymmetry between internal and external history is not present in the explanatory domain. We should write internal history on the basis of external history and we should write external history on the basis of internal history.

Historians use internal and external history in a way that is not entirely dissimilar to the way scientists use data and theory: they construct theories on the basis of the data and then interpret the data on the basis of the theory and also test the theory against new data, giving rise to a cyclical reassessment of both the theory and the data. Similarly, historians construct internal history on the basis of external history and then interpret external history on the basis of internal history and also test internal history against external history, which gives rise to a cyclical reassessment of both internal and external history.[72]

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