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Practice as Smokescreen:

On the Failure of Anti-Realism in Philosophy of Historiography

Abstract

The special issue of the Journal of the Philosophy of History (Volume 19, Issue 3) on "Current Issues in Analytical Philosophy of History" claims to advance a "practical turn" in the philosophy of historiography (i.e., study of history). In this paper, I argue that this appeal to practice functions as a smokescreen concealing deep philosophical problems and hijacking realism-debate. I point out how the contributors in the special issue systematically define “realism” in ways that create easy targets, then proceed to argue against their own redefinitions with arguments that do not hold. I contrast the approach taken by papers in the special issue with genuinely practice-based philosophy, using Siska De Baerdemaeker's analysis of dark matter experiments as an example of what practice-oriented philosophy means. I conclude that the anti-realist arguments fail to illuminate historiography and instead increase the number of conceptual confusions and distance from practices that philosophy should dispel.

Keywords: Historiographical Realism; Anti-Realism; Practice Turn; Constructivism; Methodology; Analytic Philosophy of History

1. Introduction

The special issue of the *Journal of the Philosophy of History* on "Current Issues in Analytical Philosophy of History" arrives with a serious promise. Guest editors Ewa Domańska and Krzysztof Brzechczyn announce how there is a transformation in analytical philosophy of history: a shift "from abstract, formal analyses of concepts and explanatory models toward reflections on historians' research practices."[1] They suggest that the "revival of the analytical philosophy of history thus reinforced by the practical turn and the social turn in historical research as well as in analytic philosophy itself"[2] that will make the field more relevant to working historians and to pressing social issues.

These are admirable intentions. Philosophy of historiography has, indeed, suffered for a long time from disconnection with actual historical practice. If the special issue delivers what it promises, it will represent a significant advance, almost a breakthrough in philosophy of history.

Unfortunately, it does not. What the special issue offers instead is a series of sophisticated-sounding arguments that, when analyzed further, systematically (i) redefine key philosophical terms to create strawmen, and (ii) invoke "practice" without any genuine engagement with historiographical work – the try to hijack "analytical" and "practice-oriented" philosophy of history and use these categories to continue mere conceptual play.

In this article, I discuss the argumentative strategies used in the special issue's theoretical papers (included, oddly, on special issue on practice-oriented analytic philosophy

[1] Ewa Domańska and Krzysztof Brzechczyn, "Expanding the Boundaries of the Analytical Philosophy of History," *Journal of the Philosophy of History* 19, no. 3 (2025): 263-280, at 265.

[2] Ibid.

of history). I argue that these papers share a common structure: They first claim to reject, through arguments, "realism," but, when looked more closely, the realism they reject is a *caricature*. Meanwhile, the constructivists approaches are justified through unwarranted references to “practice”.

To illustrate what genuine practice-based philosophy looks like, I draw on Siska De Baerdemaeker's relatively recent work on dark matter experiments.[3] Discussing De Baerdemaeker's analysis provides a striking illustration of how philosophy can illuminate actual research practice, identify novel logical structures in reasoning, and draw substantive conclusions with real implications. The contrast between De Baerdemaeker's work and the articles in the special issue of *Journal of the Philosophy of History* 19(3) is stark and telling about what “focus on practice” means in philosophy of historiography.

2. The Semantic Shuffle

The first and most deepest problem in the articles of special issue is what I call the "semantic shuffle". This is the systematic redefinition of "realism" into something that no serious realist (and I admit being one) would accept, followed by triumphant refutation of this strawman.

Consider, for example, Zeleňák's article. Zeleňák argues that "realists [--] offer misguided interpretations of constructivism."[4] His diagnosis is that realists incorrectly attribute to constructivists views they do not actually hold. As he puts it: "I focus on several claims that they incorrectly attribute to constructivists, which, at the same time, they use to

[3]Siska De Baerdemaeker, "Method-Driven Experiments and the Search for Dark Matter," *Philosophy of Science* 88, no. 1 (2021): 124-144.

[4] Eugen Zeleňák, "On the Realist-Constructivist Controversy in Contemporary Philosophy of History," Journal of the Philosophy of History 19, no. 3 (2025): 281-301.

criticize constructivism."[5] Zeleňák identifies three misattributed claims: that constructivists deny the existence of the past, that they believe there are only descriptions (not events), and that they embrace epistemological skepticism. Zeleňák argues against these attributes: "recent constructivists argue that historians (and their works) provide knowledge and understanding; therefore, in fact, they reject epistemological skepticism attributed to them by realists."[6] This is a fair observation. But notice what Zeleňák offers as diagnosis: “the whole debate may become more fruitful if it turns to what is, in fact, the bone of contention: defense v. rejection of the correspondence approach to historical knowledge."[7]

Here the semantic shuffle begins. "Realism" is defined as commitment to a naive correspondence theory according to which historical works are “trying to represent preformed past counterparts.”[8] Sure, Mitrovic, in his writings uses the term “correspondence.”[9] But the naïve idea about representation and truth implied here are not what defines realism. Moreover, when Zeleňák argues that an epistemic pillar of realism is “knowledge of the past in terms of correspondence”[10] one can only wonder what correspondence theory of truth (which Zeleňák reads in naïve terms) has to do with theories of knowledge.

To clarify, a historiographical realist holds that our best historiographical accounts are *approximately* true descriptions of the past – not that they perfectly correspond to some "pre-

[5] Ibid. 291.

[6] Ibid. 295.

[7] Ibid. 283.

[8] Ibid 288

[9] Branko Mitrović, *Materialist Philosophy of History: A Realist Antidote to Postmodernism* (Lanham: Lexington, 2020)

[10] Zeleňák, "Realist-Constructivist Controversy”, 284.

formed"[11] reality (whatever this means – common-sense interpretation being that past was *causally* formed before the present, which is obviously true). The realist can fully acknowledge that historians construct narratives, make interpretive choices, and use conceptual frameworks. The question is whether these narratives, choices, and frameworks are constrained by, and responsive to, evidence about what actually happened in a way that allows us to infer that the past – yes, *the past* – was approximately like narratives and other descriptions say it was.

Hubálek and Kowalewski Jahromi push this further in their writing about "Rothian steps" that take us “beyond the beaten paths of historical realism.”[12] They argue against “realist intuitions and common-sense ontologies revolving around the ideas of Universal History and The Past”[13] where “Universal History” is “mirroring The Past”.[14]

Again, no sophisticated realist holds this view. To be honest, I have never heard about the connection between realism and Universal History. The argument is innovative but equally defective. One can perfectly well deny Universal History while hold that particular claims about the past are either approximately true, depending on how the past was like, and that we can have good reasons to accept some such claims – this is realism. The realist who believes that Caesar crossed the Rubicon in 49 BCE is does not have to commit to some murky notion of *Universal History* being available, even in principle. The realist is

[11] Ibid. 288.

[12] Michal Hubálek and Piotr Kowalewski Jahromi, "Naturalizing the Past(s): Three Rothian Steps Toward a Future Philosophy of History," *Journal of the Philosophy of History* 19, no. 3 (2025): 319-344,

[13] Ibid. 342.

[14] Ibid. 324.

committed only to this particular claim being approximately true of a mind-independent past.[15]

The pattern repeats throughout the special issue. Terms are shuffled to create easy targets: "Realism" becomes naive correspondence theory; "The Past" becomes Universal History; "Knowledge" becomes certain, infallible “correspondence” with reality; "Historical description according to realist" becomes perspective-free mirroring. I do not even take stance here what these items are supposed to mean, I hope those against realism enlighten me in the future through their “practice-oriented” philosophy of history (see below my doubts on this).

With these definitions in place, refuting "realism" is child's play. But the refutation is empty because the target never existed, as pointed out. The options are not exhausted by "naive correspondence" and "anti-realism."

The semantic shuffle creates an illusion of philosophical progress while leaving central questions untouched. If realism meant what the special issue claims it means, no one would be a realist. But realism does not mean that. The modest realist holds that the past occurred independently of our knowledge and that our best accounts are probably approximately true of the parts of the past they are about. Nothing in the special issue focuses

[15] One could write here about Danto’s Ideal Chronicler like Roth does in the special issue but this would only lead to yet another repetition of ideas that have been questioned (See Veli Virmajoki, "Defeating the Ideal Chronicler," *Journal of the Philosophy of History* (2025) but no answers given even in the special issue, while the Ideal Chronicler argument is repeated by Roth (see Paul A. Roth, “Curbing Narrative Anxiety,” *Journal of the Philosophy of History* 19, no. 3 (2025): 301–318). See also: Arthur C. Danto, "Narrative Sentences," *History and Theory* 2, no. 2 (1962): 146-179.

on refuting this modest position. Realist are modest, believe it or not, and no strawmen refutes, therefore, realism.

3. Practice as Smokescreen

The special issue's second major failing is its idea that making understandable the "practice" of historiography as a criterion of adequacy of philosophy while the papers provide very little engagement with actual historiographical practice. References to “practice”, it seems, are simply a method to hijack realism-debate and to silence realist.

The gap between the rhetoric of “practice” and the actuality of abstract theorizing in the articles of the special issue is vast. When one examines what the papers actually contain, one finds philosophical argument piled upon philosophical argument, with historians and their work appearing – if at all – as brief examples briefly mentioned rather than carefully analyzed.

One might think that this disconnection between promise and delivery is not accidental. One might argue that the abstract arguments in the special issue *require* distance from actual historiography. If one analyzed in detail how historians actually reason, justify claims, and evaluate competing accounts, the neat anti-realist conclusions would become difficult to sustain. The appeal to "practice" functions, it seems, precisely to avoid the hard work of examining practice.

Consider what genuine practice-based philosophy looks like. In her paper "Method-Driven Experiments and the Search for Dark Matter," De Baerdemaeker examines how particle physicists justify experiments designed to detect dark matter particles.[16] This is a genuinely puzzling case: dark matter, by definition, does not interact with ordinary matter in

[16]De Baerdemaeker, "Method-Driven Experiments," 124.

ways we can easily detect. How do physicists argue that their experimental methods will be effective in detecting dark matter?

De Baerdemaeker identifies a novel logical and methodological structure of scientific practice that she calls "method-driven" reasoning, which contrasts with the more familiar "target-driven" reasoning. In target-driven reasoning, scientists select methods based on known features of the target system. But dark matter's definition is remarkably thin: we know it exists (from gravitational effects), that it is nonbaryonic, and that its coupling to standard model particles is very limited. This is not enough to justify method selection with more standard reasoning.[17]

Instead, physicists employ method-driven reasoning: they ask what features the target *would need to have* for various established methods to be effective, then construct plausibility arguments for those features. De Baerdemaeker shows how this logic operates in actual physics. She examines production experiments at the Large Hadron Collider, where physicists search for dark matter by looking for missing energy signatures. De Baerdemaeker discusses in detail how these experiments assume that dark matter couples to standard model particles through some mediator – an assumption not independently confirmed but plausible given various theoretical frameworks. De Baerdemaeker discusses direct detection experiments like LUX and CDMS, which look for nuclear recoils from dark matter scattering, and explains how these experiments assume that dark matter particles are weakly interacting, have masses around 100 GeV, and exist stably in the galactic halo.[18]

Crucially, De Baerdemaeker traces the historical development of these approaches, drawing on a 1988 review paper by Primack, Seckel, and Sadoulet that explicitly articulates

[17] De Baerdemaeker, "Method-Driven Experiments".

[18] Ibid.

why neutrino-detection methods might work for dark matter.[19] De Baerdemaeker shows how these physicists constructed plausibility arguments for the required assumptions, appealing to cosmological scenarios that would generate the observed dark matter abundance if dark matter had certain coupling constants. The paper by De Baerdemaeker is a detailed analysis of how actual scientists reasoned about method choice in a difficult epistemic situation.

This is what real practice-based philosophy looks like. It involves detailed examination of specific cases, identification of logical and methodological structures that can be found by analyzing an actual practice, real implications for how we interpret results, and illumination of practice in that one understands better what is going on in research after reading philosophy.

Now compare this to the special issue. Where are the detailed examinations of historiographical works? Where are the analyses of how particular historians have reasoned, what methods they have employed, how they have justified their conclusions?

Kuukkanen's paper offers two "case studies": One about Finnish Civil War historiography – mostly referring to his other article elsewhere, and doing so on a very general level –, and other about David Irving's Holocaust work.[20] But these are not genuine case studies in De Baerdemaeker's sense. They are brief gestures, not detailed analyses of actual historiographical reasoning. Kuukkanen argues that

[19] See J. R. Primack, D. Seckel, and B. Sadoulet, "Detection of Cosmic Dark Matter," *Annual Review of Nuclear and Particle Science* 28 (1988): 751-807.

[20] Jouni-Matti Kuukkanen, "Methodologism as a Philosophy of Knowing," *Journal of the Philosophy of History* 19, no. 3 (2025): 345-365.

> "Claims in historiography are variously warranted through the following kinds of sources: archival material, historiographical literature and discourse, and shared beliefs. They may also be warranted because they seem to flow from the text without an explicit stipulation and because of textual coherence that supports what is claimed. Finally, the authority of the historian may also be a source of warrant. In this situation, the historian is trusted to tell some account as sincerely and accurately as possible, and her judgement is taken as reliable."[21]

There is nothing wrong in itself here, but how does this really dig deep in how historiographic logic and methodology work in detail?

The case study on Irving is curious. It consists of listing Irving's violations of good practice (such as Irving falsified and manipulated evidence, selectively misused and mistranslated sources, suppressed contradictory facts, and relied on unreliable or fabricated material to advance his ideological agenda) without analyzing how proper historiographical reasoning would proceed. We learn what Irving did wrong, but not what doing it right looks like in positive terms. The case reveals rules by showing violations, but this negative approach does not tell us how historians actually construct knowledge. It is like trying to understand how bridges are built by examining only collapsed ones. Sure, one can learn a lot once a bridge collapses *if one knows its structure beforehand*. But when we are not told how, exactly, historiography produced knowledge from the ground up, collapsed works do not provide understanding.

As said, in the special issue, there is a lot of noise about focusing on practice, but relatively little practice studied. The special issue's contributors tell us that historiography

[21] Ibid. 361

involves practices, methods, and rules. But they do not show us what these practices, methods, and rules are by examining actual historiographical work in detail.

The contrast with De Baerdemaeker is telling. She can tell us *specifically* how Primack, Seckel, and Sadoulet argued for the effectiveness of direct detection experiments. She can identify *exactly* what assumptions LHC physicists make about dark matter coupling and why these assumptions are considered plausible. The special issue's contributors do not tell to any such detail while particular historian, or group of historians, has reasoned about any particular historical question.

One might object that dark matter physics is somehow easier to analyze philosophically than historiography. But this gets things exactly backwards. Dark matter is an extraordinarily difficult topic, epistemically speaking. The target is defined almost entirely negatively: we know what dark matter is *not* (not baryonic, not strongly interacting, not electromagnetically coupled) far better than what it *is*. Physicists cannot directly observe dark matter, cannot manipulate it in controlled experiments, and cannot even be certain it exists as particles rather than as a modification of gravitational theory. If any domain poses epistemic challenges comparable to historiography's, it is this one. Despite this, De Baerdemaeker shows that rigorous, practice-based philosophical analysis is possible even such tricky cases. She identifies the logical structure of reasoning, traces how actual physicists have deployed that reasoning, and draws substantive conclusions about the interpretation of results.

Historiography faces analogous challenges. Consider the study of preliterate societies, where documentary evidence is absent and historians must rely on archaeological remains, later oral traditions, and comparative analysis. Or consider the study of marginalized groups whose voices were systematically excluded from official records. Or consider events where key documents were destroyed, witnesses died without testifying, or the historical actors

themselves had limited understanding of what they were doing. These are genuinely difficult cases where the "target" is poorly defined and the methods for accessing it are uncertain.

But these difficult cases are precisely where philosophical analysis could be most valuable. How *do* historians reason when evidence is sparse? What assumptions do they make about the reliability of different source types? How do they construct plausibility arguments for interpretations that cannot be directly confirmed? These questions parallel De Baerdemaeker's questions about dark matter: What assumptions must be made for a method to work? How are those assumptions justified? What follows for the interpretation of results?

A serious practice-based philosophy of historiography would study difficult historical cases with the same care De Baerdemaeker analyzes the case related to dark matter. It would identify the logical structures historians employ when reasoning under uncertainty. It would trace how particular historians have justified method choices when the target is poorly understood. Such analysis would be genuinely illuminating – far more so than abstract arguments about whether "the past" is "constituted."

The special issue does nothing of this sort. Its contributors make a lot of noise about "practice" while avoiding the hard work of examining how historians actually could face logically and methodologically difficult topics.

4. Imagined Practice Is Not Practice

Perhaps the most telling example of the gap between rhetoric and reality in the special issue comes from Kuukkanen's attempt to ground his "methodologism" in practical considerations. However, we are provided thought-experiments and intuitions, not practice:

> "Consider a historian who argues for a specific point. Maybe she claims to know Jack the Ripper's identity or something about the origins of World War I. Let us suppose that she is well educated on the topic. She has studied all the relevant

> literature, consulted all the archival material, and employed the best historiographical and investigative methods. Then, she writes a book about the subject, one that exhibits clear and sound reasoning. The book is organised so that all its material and sentences attest to the final conclusion. A reader can understand the grounds for the claims and examine references and footnotes. In brief, the historian has done everything correctly in terms of her discipline's methodology and the rules of reasoning. Next, she is faced with discussions and criticisms from her colleagues and the broader public. Because her work is done well, she manages to hold her ground in these debates."[22]

Kuukkanen then asks whether we can say this historian "knows" what she claims.

But notice what has happened here. This is not engagement with practice – it is thought-experiment about imagined practice. We are given an idealized figure who has done basically everything correctly and asked to pump our intuitions about whether she "knows." This is precisely the kind of abstract philosophizing the special issue claims to move away from.

Compare this to De Baerdemaeker's approach. She does not ask us to imagine a physicist who has done everything correctly. She examines what Primack, Seckel, and Sadoulet actually wrote in 1988 – their specific arguments for why neutrino-detection methods might work for dark matter, their appeals to cosmological scenarios, their reasoning about cross-sections and coupling constants. From this detailed examination, the method-driven/target-driven distinction emerges as a genuine insight about scientific reasoning.

This pattern – using "practice" as rhetoric platform, then immediately retreating to abstract argument – characterizes the special issue as a whole. The word "practice" appears frequently; engagement with actual historiographical practice does not.

[22] Kuukkanen, "Methodologism," 346–347.

5. The Striking Absence

A remarkable feature of philosophy of historiography, compared to other fields, is the absence of arguments from track record.

Consider how debates proceed elsewhere. When philosophers question whether a discipline achieves knowledge, they typically examine the discipline's history. Has knowledge accumulated? Have earlier conclusions been vindicated or overturned? Do practitioners converge on stable results, or do fashions shift without cumulative progress? These are natural questions to ask, and answering them requires historical investigation – looking at what a discipline has actually achieved over time.

In debates about the cognitive status of science, for instance, the "pessimistic induction" plays a central role. The argument notes that many past successful theories turned out to be false – the optical ether, phlogiston, caloric theory – and concludes that we should expect our current theories to suffer the same fate. Defenders of scientific realism respond by examining the history more carefully: they show that the theoretical components responsible for past success were often preserved through theory change, even when other components were abandoned. The debate is conducted through detailed historical analysis.[23]

Where is the analogous argument in philosophy of historiography? If historiography fails to capture mind-independent reality, one would expect the history of historiography to exhibit recurring failures. Historiographical accounts accepted in one generation would be overturned in the next, not because new evidence emerged but because the earlier accounts were systematically mistaken about what happened. The history of historiography would be a graveyard of abandoned frameworks, with each generation constructing its past anew, unconstrained by what actually occurred.

[23] See also Veli Virmajoki, "A Deceiving Resemblance," in *Realism and the History of Historiography*, eds. Mitrovic and Forland: *The Poverty of Anti-realism*. Lexington Books.

But this is not obviously what we find. Consider the Eddington case. The 1919 eclipse observation has been studied by historians for decades. Different historians have emphasized different aspects – some focus on the physics, others on the social dynamics, others on the data handling. But there is no fundamental disagreement about who the relevant people were, what the most important events were, or how people reasoned.[24] Historiographical understanding of the episode has grown more nuanced, but the basic picture remains stable: Eddington led an expedition to observe a solar eclipse, measurements were taken, the data were analyzed, and the results were announced as confirming general relativity. Later historians have added complexity to this picture – showing that Eddington's handling of discordant data was more creative than sometimes acknowledged – but they have not overturned it.[25]

This stability is what we would expect if historiography succeeds in capturing aspects of mind-independent reality. It is not what we would expect if events exist only under descriptions and those descriptions are subject to constant revisions, at least in some principled way. If the anti-realists were right, we would expect historiographical knowledge to be radically unstable – each generation imposing new conceptual frameworks that constitute entirely new "pasts." Or at least it would great to hear how they explain this is not the case for most parts of the history – about the past.

The special issue's contributors do not address this challenge. They do not examine the history of historiography to see whether it supports their anti-realist conclusions. They do

[24] See Harry Collins and Trevor Pinch, *The Golem: What Everyone Should Know About Science* (Cambridge: Cambridge University Press, 1993).

[25] See, for example, Matthew Stanley, *Einstein's War: How Relativity Triumphed Amid the Vicious Nationalism of World War I* (New York: Dutton, 2019).

not explain why historiographical knowledge appears stable. The historical argument that would support constructivist and would be natural to make is entirely absent.

Perhaps there is a good explanation for this absence. Perhaps the anti-realists have considered historiographical track record and found it supports their view. But if so, they have not shared their analysis. The striking absence suggests that the arguments for anti-realism are developed in abstraction from actual historiographical practice – exactly the opposite of what the special issue's rhetoric about "practice" would suggest.

6. What Realism Actually Claims

Let me be clear about what historiographical realism claims – and what it does not claim.

Historiographical realism, properly understood, holds: (1) The past occurred independently of our current knowledge of it. Events happened, people acted, states of affairs obtained – whether or not anyone now knows about them. (2) Historiographical claims are either true or false in virtue of what happened. (3) We can have good reasons to accept some historiographical claims as *approximately* true. Evidence – documents, artifacts, testimony, physical traces, and other *real* things from the *past* – constrains quite strongly what we should believe about the past. (4) Our best historiographical accounts, arrived at through careful method and critical scrutiny, are likely approximately true about the past – but never perfect, partly because of evidence, partly because restricting conceptual frames, and so on.

This is a modest position. It does not claim that we can achieve perfect correspondence between our accounts and "the past as it really was". It does not claim that there is a unique correct, god-given description of every historical event. It does not claim that historical knowledge is certain or infallible. It has no relation to Universal History, whatever that means. It does not claim that historians are passive mirrors rather than active interpreters.

The special issue's contributors argue against realism, but the realism they argue against is not this modest position. They argue against naive correspondence theory, against Universal History, against the idea that historians passively record a preformed past. Having defeated these strawmen through the semantic shuffle, they claim victory over "realism." And even have the nerve to do so under the term “practice”, which they are least concerned about, it seems.

7. What Philosophy of Historiography Should Do

The special issue discussed in this paper promises a shift in direction in philosophy of historiography: a shift toward practice, toward social relevance, toward engagement with actual historiographical work. It delivers something quite different: abstract arguments against strawman positions, and references to "practice" without genuine engagement.

It seems that "practice" remains a smokescreen concealing the absence of genuine engagement and hijacking realism-debate. The semantic shuffle continues to create the illusion of progress while the substantive questions remain unaddressed. And the realism debate in philosophy of historiography remains, as it has been for too long, a dispute conducted through redefinition rather than argument, through gesture rather than analysis.