

Review

Values, Objectivity, and Explanation in Historiography by Tor Egil Følrand (Professor of History, University of Oslo). London and New York: Routledge. 2019. ISBN: 9781138203730.

Følrand's new book *Values, Objectivity, and Explanation in Historiography* is a most welcome contribution to the debate on the philosophy of historiography. Følrand is Professor of History at the University of Oslo, and, as he explicitly states in the Introduction, he deals with some of the main issues in that field from the perspective of a practicing historian. The goal is bridge building, in the belief that "philosophy of history would benefit from increased knowledge of what historians actually do" (p. 2).

Now, the growing attention to the concrete practices of research is one of the distinguishing features of the (relatively) new practical turn in philosophy of science. Følrand's book fits perfectly within that approach: by backing his theoretical ingenuity with practical, down-to-earth wisdom, he succeeds not only in providing relevant and thought-provoking case-studies that foster historiographical reflections. He also succeeds in bringing to the fore of historiographical analysis issues and problems that were too hastily deemed as substantially irrelevant to a narrativist-inspired philosophy of history. We are thus presented with a series of classical questions concerning the nature of historical objectivity, the structure of historical explanation, the role and function of values in historiographical research. Though many narrativists believe that those issues should no longer hold pride of place in the philosophy of historiography, Følrand argues that they are nonetheless inescapable in the practice of doing history.

The eight chapters that make up the book – some of them published here for the first time – attempt to shed light on these constitutive aspects of the historiographical work. The book is divided into two main sections. The first part, entitled *Objectivity, Values, and Theory Choice*, deals with the entanglement of values and objectivity: the primary goal of the four chapters composing this part is to stress – *repetita iuvant!* – that though objectivity is not to be conceived of as neutrality, and, therefore, that adopting a moral (evaluative) does not entail that an objective assessment of the past cannot be given, we should maintain a central role for the notion of historical objectivity in explaining what good history is. The second part, entitled *Explanation and Causality*, is devoted to the analysis of those two notions: Følrand's goal is to show that historical research is committed to the search for patterns of explanation and that some of these patterns necessarily involve causal assumptions.

The first two chapters, entitled *Participants and Fellow Travelers* and *Court Historian* respectively, reconstruct and critically discuss two historical episodes that disclose the complexity – both internal (in relation to scientific standards) and external (in relation to politics and society at large) – of the notion of historical objectivity. In the first chapter, Følrand reconstructs and analyzes what he calls the fall of objectivism in Danish and Norwegian historiography. There is something deeply paradoxical, he remarks, in the way in which Danish and Norwegian historians and philosophers of historiography have dealt with the notion of historical objectivity. In the 1970s, the rejection of objectivity as neutrality was the strong point of leftist historians: contrary to positivists, who believed that historians should abstain from taking a stance on their subject-matter, the anti-positivists argued that the viewpoint of the historian could not be bracketed or eliminated, since events can only be understood and regarded under a particular description.

Clearly – and this is a point that Følrand carefully and repeatedly stresses – the anti-positivist emphasis on the constitutive function of the historians' viewpoint is grounded on the possibility of somehow reaching a substantial agreement on the moral and political values among the members of the community of researchers. Referring to the Norwegian scenario in the 1980s and 1990s, Følrand writes that "[h]istorians not only could but *should* be part of the Left, or at worst of the left-leaning faction of the center-right Liberal Party" (p. 25).

However, when the moral and political bonds that bind together the members of the community got loose, the side effects of that approach come to the fore. The culture war in Danish Historiography is a good instance of this kind. By that formula, Førland means to refer to the challenge that Bent Jensen, Professor Emeritus at the University of Southern Denmark in Odense, raised against “the Danish guild of diplomatic historians” (p. 28). According to Førland’s account, Jensen is a militant historian who explicitly purports to take a moral and political stance on the events that he studies. Not only does Jensen deny any contradiction or conflict between morality and historiography, he also believes that contested topics such as Bolshevism, Nazism, the Second World War, and the Cold War cannot be studied and properly understood without adopting a moral perspective on them (p. 34). Consequently, Jensen shares with the anti-positivists the rejection of the naïve idea of objectivity as moral neutrality; the relevant point is that, unlike them, he is on the Right. In particular, he is extremely critical of how the Cold War has been depicted by the majority of Danish historians, and he openly questions the truth of the standard reconstructions of the events that mainstream Danish historians formulated and endorsed.

The paradoxical trait of that *affair* is that to defend themselves from the charges raised by Jensen, many Danish historians had recourse to a positivist language, thanks to which they could draw once again a clear-cut distinction between historical facts and moral or political values. When the agreement among the historians is lost, and new and competing values start to be held, the old-fashioned concept of historiographical objectivity regains its rationale. The problem is that such foothold cannot be gained anymore.

The consequences of the want of agreement among historians could be quite easily kept at bay if it were not for the fact that historiographical disagreements can be used to foster political conflict. This subject is what the second chapter is about: Førland here discusses and comments on the trial that opposed Jensen to Danish journalist Jørgen Dragsdahl, who was accused by Jensen of having been an agent (of influence) of the KGB in Denmark back in the 1980s. What is interesting to note is the imbroglio of political and scientific elements that made that the Dragsdahl-Jensen case so thorny, so much so that it was difficult even to establish the criteria by which to judge Jensen’s claims. Should they be considered as scientific statements or rather as claims aiming to inform Danish citizens about the ambiguous conduct of a public figure? The fact that three different courts reached three different verdicts shows that it was by no means an easy task. And yet, Førland argues, we all share the intuition that historiography is not – and should not be – the continuation of politics by other means. We should resist the post-modernist temptation to get rid of historiographical objectivity: it is not only a matter of meeting the demands of scientific accuracy; most importantly, it amounts to realizing that scientific objectivity is necessary to a fruitful and satisfactory civic life. I like the way Førland puts this point: if political, moral, or religious values are allowed to influence and determine historiographical interpretations, then it is highly plausible that the readers of the historiographical works will be led to reject all those accounts written by historians who held values incompatible with their ideals. “The effect”, Førland states, “is a breakdown in communication, and possibly, a fragmentation of society into segments with separate value systems and their own historians” (p. 3); “without scientific historiography, society is bereft of a critical means to create common and consensual ground among groupings that have different political preferences, different ideologies, and different religious convictions” (p. 103). I do not buy Førland’s entire argument: I think that it can be proved that some moral and political values are not necessarily as disruptive as he believes. In any case, he has the merit of reminding us that seemingly technical and abstruse issues have broader and more direct implications, which a self-conscious philosophy of science should not overlook.

The remaining two chapters of the first part, entitled *Witches Cannot Fly* (Chapter 3) and *In Defense of Objectivity* (Chapter 4), are devoted to articulating a theoretical account of historiographical objectivity. In Chapter 3, Førland criticizes the idea of situated truths as formulated by Norwegian historian Erling Sandmo. According to many post-modernists – of whom Sandmo is the most influential representative, at least in

the Norwegian context – different communities live in different realities: post-modernists argue that cultural phenomena are so complex that they cannot be grasped independently of the cultural framework within which they operate. In this sense, while drought is today conceived of as a natural phenomenon, in the XV century it was treated by French peasants as a supernatural event that had to be solved recourse to magic. What is at stake here is the very criteria of identity for events: in which sense is it possible to say that the two events are of the same kind? Such ontological incommensurability goes hand in hand with semantic incommensurability. Since we do not understand drought as XV century French peasants did, we are left with two options: either we endorse the radical whig view that we are right and they are wrong, or we end up admitting that we are both right, and, consequently that different communities not merely hold different beliefs to be true – which is trivial – but, more radically, grasp different and incompatible truths.

The notion of situated truths is intended to convey the idea that truths are dependent on the justificatory practices of the various communities. Now, as Førlund correctly points out, such a notion is extremely controversial. His argument can be approximatively reconstructed as follows: according to the standard conception, while truth is a semantic concept, justification is an epistemic concept. Consequently, any effort to make truth an epistemic concept is openly prescriptive: we should, therefore, have very good reasons for revising our conventional understanding of it. Førlund holds that we have none – and I think he is entirely right on this point.

In reality, post-modernists give various arguments in support of their view. At least four of them are worth mentioning here. Their first argument is metaphysical: since post-modernists believe that we do not have any access to reality apart and independently of the descriptions that we can give from our perspectives, they conclude that alethic relativism follows. The missing premise is that truth is the correspondence between description and reality. Another argument they put forward is methodological: if we historians aim to provide a satisfactory explanation of the practices of the people that we are studying, we should take their beliefs seriously; otherwise, it is highly unlikely that we will succeed in understanding why they acted as they did. Yet another argument is conceptual: we now know that the mechanisms of truth ascription vary from time to time, and they are strongly dependent on the social and institutional contexts in which beliefs are held, and descriptions are made. So, if the mechanisms of truth ascription are contingent and relative to specific cultures, we should be open to admitting that truth, as well, is contingent and relative to the context. Finally, I see post-modernists voicing a quite widespread ethical concern: as historians, we should respect the cultures of the past that we study. Assuming that we know *better* than them is *ethically* unfair.

Now, those arguments should be kept separate; indeed, they are by no means convergent. In particular, the metaphysical argument – which is the one that does almost all the job of securing the post-modernist conclusion that truths are situated – is controversial. And I must confess here that I do not entirely understand why Førlund deals with that issue in the way he does. In my view, he concedes too much to his post-modernist opponent when he writes that “[w]ith ‘congruence’ I do not mean correspondence in the sense that reality is as we describe it; we can never know how reality is in itself, only how it appears to each of us individually” (69). My expressivist leanings make me extremely wary of the very language used to state the argument, and I feel tempted to discard it altogether. In any case, I am aware that I may be biased here, so I will not push this line of reasoning any further. To make my point, it is enough to remark that the alethic relativism that post-modernists advocate is not a local issue that only concerns history, but represents, instead, a global issue affecting every field of inquiry.

That said, once the metaphysical argument is out of the game, we can easily reformulate the contents of the other arguments in a way that does not take recourse to the contested notion of situated truths. And this is what Førlund does: he rightly stresses that we can say that people in the past held different *beliefs*; that they were justified in holding those beliefs in light of the evidence available; that systems of beliefs depend on the institutional contexts in which the believers live; that our aim as historians is not to judge

them for their beliefs but rather to understand why they ended up endorsing them. As is evident, we can speak of belief rather than truth, and no practical difference would ensue. Førland is explicit on this: “Sandmo and I disagree on the concept of truth in cultural history”, and, more precisely “we disagree on whether his concept of truth – one where what our objects of study consider to be true is to be true, even though both Sandmo and I know better – leads to better cultural history than research based on conventional concept of truth” (p. 79).

As a philosopher, I am not entitled to take a position on this issue. *You shall know them by their fruits*, and such an evaluation pertains to historians only. As a philosopher, however, I feel entitled to express some reservations about the idea that Førland defends, that we *know* better than the people in the past that are the object of historiographical research. This is a relevant point in the economy of Førland’s argument, which I find problematic. Let me be clear on this: I do believe that, in a certain sense, we know better about the drought than XV century French peasants did. Taken the word “know” at its commonsense value, this is a claim that everybody understands and, I guess, almost everybody would endorse. However, if we hold that our explanation of drought is better than the one devised by XV century French peasants because it is true;¹ and we also embrace, as Førland seems ready to do, a fallibilist account of scientific knowledge – leaving open the possibility that what we believe will be rejected by future generations (p. 72) – some difficulties are likely to arise.

Indeed, to avoid inconsistencies, we should rely on a satisfactory account of truth-approximation: that would enable us to explain how we can be wrong about drought, and yet be closer to the truth than XV century French peasants. Unfortunately, we lack such an account. Accordingly, Førland’s argument misses a premise.

I do not think that this is an impossible problem to overcome. Quite the contrary, I believe that it can be rather easily dealt with, but it asks to shift attention from the notion of knowledge to that of the expressive power of a language – more on that in a moment. For now, I would like to stress that though such a conceptual move is, at least in principle, available to Førland, it seems to enter into conflict with some features of his account of objectivity and explanation. That is the main point of disagreement between Førland and myself: Førland is a pluralist about historical explanation – and I think he is entirely right on this – but he is also willing to maintain a strong conception of objectivity – actually, as strong as possible, provided that “strict objectivity is epistemologically unattainable for historians” (p. 87).

Before formulating in some detail the reasons for my dissatisfaction with Førland’s proposal, let us then turn attention to his analysis of the notion of historical objectivity, to which Chapter 4 is dedicated. As hinted at above, Førland’s starting point is the idea that *global objectivity* – i.e., that kind of objectivity “where the perspective is irrelevant” – cannot be achieved by historians: at best, they can achieve a form of objectivity strong enough to ground the distinction between historiography and politics, between historiography and propaganda (see Chapter 1). That is the function that historiographical objectivity is asked to perform.

The arguments that are supposed to rule out the possibility of strict objectivity in historiography are mainly four: 1) the fact, pointed out by Hayden White, that historical narratives are underdetermined by historical evidence; 2) Danto’s analysis of narrative sentences, according to which “the description of events in history is always provisional, always deferred, never stable” (p. 89); 3) Davidson’s idea of interpretative

¹ For the sake of fairness, I have to point out that Førland’s actual position on this issue is more nuanced than how I have portrayed it here. Førland’s view is that “that since our explanation of drought is so good and so much superior to and contradictory to the one devised by 15th century French peasants, we are warranted in believing that our explanation is true (and theirs, hence, false), and therefore in denoting our explanation ‘true’ — which by implication warrants us denoting their explanation ‘false,’ even though (this is perhaps debatable) they might have been warranted in denoting it ‘true’” (personal communication).

indeterminacy, which starts from the assumption that alternative descriptions of an action or event are always possible, and concludes that actions or events are objective “within the realm of the specific description under which the action is described” (p. 90) (incidentally, I do not agree with Følrand on his reading of Davidson); 4) Paul Roth’s theory of events as constructs: the description of the events is determined by their narrator.

Now, Følrand remarks, it is true that “[t]here is a basic epistemological instability to historical descriptions”, as those four arguments show (p. 92). However, such an instability does not imply that there is no *rational* choice among different historiographical accounts. True, we cannot rely on the Rankean idea of value-free historiography; nonetheless, we can identify “a disciplinary code and a set of cognitive values” that give us all the objectivity that we need in order to make historiography a truth-tracking science” (p. 92). The list of cognitive values varies from author to author. Følrand provides an excellent overview of the different options on the market, adding that “[t]here is no agreement on the exact specification of cognitive values for theory choice” (p. 99). Some values (internal consistency, simplicity, accuracy, explanatory power) are almost unanimously held, some others are more contested (predictive accuracy); some fields of research are peaceful, some others (think, for instance, of late 20th-century history) are more conflictual because they are closer to people’s concerns and political, moral and religious values. In any case, it is also clear that such values should be empirically discovered by analyzing the concrete practices in which historians are engaged. They cannot be established by armchair methods.

It is here, I think, that the link between truth, knowledge, and historiographical objectivity is most clearly formulated. Følrand’s thesis is that we need some principle of demarcation to help distinguish history, conceived of as a truth-tracking science, from other activities aiming at producing and fostering political consensus or directed to informing citizens. Such a principle of demarcation is historiographical objectivity. The same idea is at work in Følrand’s theory of historiographical explanation, which is formulated in the second part of the book and, in particular, in Chapter 5, entitled *The Ideal Explanatory Text in History*.

Here the issue at stake is to have explanatory pluralism without also taking a relativistic stance. As is well known, philosophers of science have convincingly shown that explanation has a strongly pragmatic dimension: for a statement or an argument to count as an explanation, it has to be grasped and adequately understood by the person to whom it is addressed. Følrand explicitly adopts the pragmatic account: referring to the idea of explanations as answers to *why-questions*, he quotes Railton’s work on explanation and says that “to explain is to provide *explanatory information* about a subject”. That implies that “[a]ll information that reduces the questioner’s uncertainty about what the explanandum is due to is explanatory” (p. 114).

Railton’s account of explanation opens the door to a plurality of possible explanations. Indeed, different kinds of explanation answer to different kinds of *why-questions*. So, we have causal explanations – which are the ones most readily associated with historiographical research, and to the analysis of which Chapter 8, entitled *Problems of Causation in Historiography*, is devoted – functional explanations and, finally, structural explanations. What is important here to note is that, contrary to the traditional Hempelian model, the possibility of explanation does not depend on the existence of general laws. According to Hempel’s standard account, the nomological deductive model of explanation (NDM) is composed of an *explanans* and an *explanandum*. The *explanandum* is the singular event that we are trying to understand; the *explanans* is a set of premises, at least one of which must be a law. Since there are no laws of history that can bear explanatory weight – contrary to what Hempel stated in his famous article *The Function of General Laws in History* – the NDM rules out the possibility of historiographical explanation.

In light of the pragmatic account, we may have a sound explanation even in those cases in which general laws have not yet been discovered, or we have good reasons to believe they do not exist at all. As is evident, this is a most welcome conclusion. The other side of the coin, however, is that by liberalizing what

may count as a good explanans, explanatory pluralism is likely to turn into a malicious form of explanatory relativism.

Indeed, if an explanation is defined as everything that “reduces the questioner’s uncertainty about what the explanandum is due to”, then we should admit as a legitimate historiographical explanation every narrative or account that is found satisfactory by one questioner. This assumption has potentially harmful consequences because it seems to entail that it is up to the questioner to decide whether or not a piece of information counts as a good explanation. So, if we assume that the questioner is biased by political prejudices, it may well be possible that she will accept as an explanation just those pieces of information that fit into her existing belief.

To avoid such an unfortunate relativistic outcome, we need to find some set of objective constraints that enables historians to adopt a normative stance on what ought to be a good historiographical explanation. Følrand relies on Railton’s notion of *ideal explanatory text*. Følrand writes in this regard:

to classify Railton’s account as pragmatic would be to ignore that to be explanatory information must pass not only a context-dependent salience test but also an objective relevance test: it must be part of what Railton calls the *ideal explanatory text*. As its name indicates, this text is never produced in reality but is a theoretical concept that includes *all accurate information about every due-to relation relevant for the explanandum* (p. 115).

By referring to the ideal explanatory text, Følrand argues, we can identify some criteria through which historians are allowed to distinguish between good and bad (or illegitimate) historiographical explanation. Let us see how that works.

Chapter 6 and Chapter 7 present two highly contested “kinds” of historiographical explanation, namely, explanations that make use of the notion of social mentality (Chapter 6), and explanations that make room for some supernatural agent (Chapter 7). In the former case, Følrand states that there are good reasons to believe that “there is room in the ideal explanatory text for practical syllogisms with plural subjects that have beliefs and pro-attitudes” (p. 141). Those good reasons are drawn from actual historiographical practice. For instance, it is a fact that historians often refer to general features of a collectivity or a social structure. In many cases, indeed, we have easier access to information about the beliefs of a group (think, for instance, of a political party meeting) than to those of the individuals that compose that group. Similarly, it is a fact that historians usually choose “a supra-individual level of description of (parts of) society” (p. 141). The point that Følrand rightly stresses is that the adoption of a supra-individual level of description does not commit historians to adopting a redundant ontology, in which social groups have the same reality as individuals. The pragmatic aspect of explanation makes it possible to sever explanatory validity from metaphysical reality.

In the case of supernatural explanations, Følrand reaches a negative conclusion. As he explicitly notices, “my own reply to the question of whether the ideal explanatory text can accommodate God is in the negative, since God, like other supernatural entities, is scientifically inaccessible” (p. 164). If the idea of God’s acts is used to account for a historical event, that explanation fails to meet the standards of scientific knowledge. The notion of ideal explanatory text allows us to dismiss all the assumptions that are justified in terms of what Philip Kitcher has called *chimeric epistemologies*. In this sense, it accomplishes the task for which it was designed.

Følrand’s account of explanation is undoubtedly appealing: it is simple, strong, elegant, and internally consistent. Nonetheless, I believe that the same task that he accomplishes through the idea of an ideal explanatory text can be accomplished in a slightly different way, which does not make use of ideal notions. I must confess that I am highly suspicious of the notion of ideal notions. My reservations come down to the

fact that they are not accessible: we do not know what the ideal explanatory text will look like; maybe we are much farther from truth than we believe we are, and we have no way to ascertain it.

The same holds, in my view, for Førland's idea that we *know* better than people in the past. I see a common thread connecting Førland's account of historiographical knowledge to his analysis of historiographical explanation. In both cases, we seek some normative criteria to warrant the scientific status of historiography. And in both cases, I fear, we run the risk of being misled by the desire to achieve a God's eye point of view where there is none – and, most importantly, where we would not need one. Here is my argument. Consider Førland's idea that we *know* better than people in the past. We saw above that since we are fallibilist about every possible knowledge – at least, *empirical* knowledge – we cannot rule out the possibility that we are wrong about the nature of, say, drought. Though it is extremely unlikely, it might be that XV century French peasants are closer to the truth about drought than we are. Maybe our whole knowledge of nature is entirely mistaken. I do not want to push this skeptical line too far since I do not want to blur the distinction between fallibilism and skepticism. However, it is clear that if we assume that knowledge must be true, then if our scientific account of drought is at least partially mistaken or incomplete, we cannot be said to know drought better than XV century French peasants do. As said, a satisfactory account of the notion of truth approximation would get us out of trouble. Unfortunately, we do not have one. Accordingly, we seem to be in a predicament.

My suggestion is that we should not follow this road, and we should stop trying to ground objectivity on truth. Let me be clear on this: I do not want to deny that history is a truth-tracking science. My point is, instead, that we should not put too much burden on the notion of truth since we can never be *certain* to know it. Truth is what we aim for; consequently, it cannot be what grounds objectivity. Indeed, if it were so, we would be bereft of any possible epistemic constraints.

Nevertheless, we have some reasonable constraints. So, how can we justify the intuitive idea that we know better about drought than XV century French peasants do? The main difference between them and us is that we now have the conceptual resources for *explaining* their belief that drought had to be dealt with by using magic: our theoretical framework has more expressive power than theirs. It is not because we are closer to the truth than them that a) we know better than them, and b) our scientific explanation of drought is more objective than their magical explanation. Rather the contrary, it is because our (scientific) language has more expressive power than theirs that we can say that a) we know more and better, and b) our explanation has higher degree objectivity. As should be evident, this is a Kuhn-inspired solution: progress is to be conceived of not as progress towards truth, but as progress from a less developed stage.

From this perspective, I am in disagreement with Førland: while I give pride of place to the notion of the expressive power of language, he is looking for a more solid ground for objectivity. However, I do not want to let a disagreement – no matter how remarkable – overshadow the many points of contact between our two approaches. More importantly, I do not want to let a disagreement convey a wrong impression about the quality and seriousness of Førland's work. His book sets the stage for fruitful cooperation between philosophers of science and philosophers of historiography. It is a book definitely worth reading.

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