The Strong Program and Asymmetrical Explanation of the History of Science: A Reply to Collin

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In the article "A Tension in the Strong Program: The Relation between the Rational and the Social," I stated that David Bloor, citing the principle of symmetry, expresses that rational and irrational beliefs must be explained in the same way, that is, by causes of the same kind. On this wise, he rejects the methodology of traditional philosophers and historians of science as asymmetrical; since they explain evidence-based beliefs with epistemic reasons and unreasonable beliefs—e.g. beliefs based on indoctrination, propaganda, ideology, and superstition—citing social factors. On the other hand, Bloor argues that the rational is made entirely of the social and, therefore, is itself a kind of social factor. But by admitting this maximum conception of social constructivism, Bloor can no longer consider the rational and the social as two separate identities and accuse the traditional view of asymmetry. However, Professor Finn Collin says that Bloor's critique of the traditional model never commits him to the duality of the rational and the social.

What is at Stake?

The Strong Program in the sociology of scientific knowledge, as Bloor initiates it, is 'symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs' (Bloor 1991, 7). This means that Bloor does not admit the common way of science historians; since they take an evaluative perspective based on modern science. They explain beliefs that are considered true or have convincing evidence differently from beliefs that are false or unreasonable and accepted due to merely professional interests or social instincts. Bloor calls this method of the historiography of science "the teleological model" and finds it indefensible; since it violates the symmetry principle which is 'taken for granted in other scientific disciplines'. In contrast, he proposes new historiography based on the symmetry tenet (13).

In terms of the three divisions of beliefs—true/false, rational/irrational, and successful/unsuccessful—Larry Laudan considers the symmetry principle to consist of three types of symmetry, respectively: epistemic symmetry, rational symmetry, and pragmatic symmetry. In his view, since the truth of scientific theories can never be proved, only epistemic symmetry can be defended; while the other two symmetries were rejected. However, he declares that rational symmetry would be defensible if Bloor articulated a sociological model for reasons (Laudan 1981, 184-196). In later years, Bloor proposed such a model, illustrating that the relation of evidence to beliefs is not an autonomous connection that exists independently of humans; rather, it is a relation established by human societies in terms of their interests. Bloor, therefore, claimed that reasons, whether empirical evidence or logical implications, are entirely made up of social factors.

But this path, although it seems very reasonable, has a hidden conflict. Bloor's two allegations are inconsistent:

> 1) Explaining rational beliefs by epistemic reasons while explaining irrational beliefs by social causes is asymmetrical, i.e. based on typically different causes.

But at the same time,

> 2) Epistemic reasons are nothing but social factors and, therefore, are of the same type.

The conflict is that on the one hand he sees epistemic reasons as opposed to social factors, and consequently of a different kind, and on the other hand he classifies these two as of the same type.

Nevertheless, believing that there is no conflict here, Collin has accused me of misreading the Strong Program. He understands Bloor's protest against his opponents in a way that does not commit him to rational-social duality. Collin suggests a new interpretation of this objection, according to which Bloor did not claim that the explanation of rational beliefs by reasons and irrational beliefs by social factors is asymmetrical. For Collin, Bloor's criticism of traditional historians is that: a) they mistakenly thought that rational and social are two different kinds of explanations; and b) they also mistakenly assume that they can provide a rational explanation for significant episodes in the history of science and leave sociological explanations to anomalies. Therefore, he profoundly disagrees with the conclusion I drew in the article that 'Bloor's criticism is not why they regard rational and social explanations as two different kinds; rather, it is why, by their assumptions, they use *two different types* of cause, i.e., rational type and social type, to explain scientific theories' (Shahryari 2022, 196).

In addition, Collin rightly argues that Bloor's use of the word "rational explanation" does not commit him to two kinds of explanation.[1]

The Duality of Rational Explanation and Social Explanation

The controversy between Collin's interpretation and mine, then, is whether, when criticizing his opponents, Bloor himself comprehends rational explanation and sociological explanation in opposition to each other, or he attributes this duality only to them and is not committed to it. In other words, we must see whether Bloor's criticism of his opponents is why they offer an indefensible asymmetrical explanation of the history of science, or, instead, he complains about why they have mistakenly considered rational and social explanations as opposed to each other and therefore asymmetrical.

I think the evidence in favor of my interpretation is so great that it is difficult to have another interpretation of this objection. Considering Bloor's objections to the proponents of the teleological model, we can clearly see that Bloor accuses them of a verily discriminatory

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[1] Much of Collin's note is devoted to comparing Wittgenstein's and Bloor's views on rule-following and the distinction between the two. Collin believes that for Wittgenstein, the rule-following argument is supposed to give normative social determination to social action. Bloor, on the other hand, has Wittgenstein as saying that his argument causally determines social agents' practice. Thus, Collin continues, Bloor must either demonstrate how he came to this conclusion from Wittgenstein's argument, or it must be said that he has confused the two. This argument, if I understand correctly, is the same point that Collin made in his book (Collin 2014, 66-80). This, although interesting, has nothing to do with my argument in the article, and I cannot go into it here.

view of the history of science based on their preconceptions; not of failing to realize the rational and the social unity. See for example, when he complains that 'If explanation is allowed to hinge on prior evaluations, then the causal processes that are thought to operate in the world will come to reflect the pattern of these evaluations (1991, 13).' Similarly, if Bloor had merely criticized the ontology behind the teleological model, rather than rejecting its value-based explanation, he would no longer have accused the model of betraying science by saying that it imposed the prejudices of scientists on reality; while in contrast, 'the strong programme possesses a certain kind of moral neutrality' (13).

These quotations make it clear that Bloor's critique is not of the ontology of the teleological explanation of science, but of their discriminatory and prejudiced nature. I firmly hold that these leave no room for Collin's interpretation. But there is more evidence. A similar critique can be found in Barry Barnes' *Scientific Knowledge and Sociological Theory*. Barnes is talking about "common sense theories of the incidence of beliefs" according to it, many thinkers divide beliefs about nature into two true and false categories, treating the first category as unproblematic, explaining it with rationality and philosophy, but considering the second as a result of bias and deviation and explaining it with psychology and sociology (Barnes 1974, 2-3). Barnes states that *it is not possible* in sociology either 'to treat "truth", or "naturally reasonable inductions", as unproblematic baselines for explanations, and all other beliefs about nature as distortions in need of causal explanation' (11). Barnes, then, is sharply critical of this dualistic view of the explanation of beliefs, not objecting to why these thinkers thought that philosophical and sociological explanations are of two kinds.

Following Bloor's lead, Martin Kusch aims to establish the sociology of philosophical knowledge. He treats the methodological symmetry considered by Bloor to mean that 'the acceptance of beliefs, theories or points of view should never be explained in terms of what it is rational, true or progressive to believe. Instead, the sociologist of scientific or philosophical knowledge must seek social explanations both for views that, as *we* see it, are rational, true and progressive, and for those that are not' (Kusch 1995, 23). Therefore, Kusch also underscores that reliance on rational considerations to explain beliefs is a transgression of the symmetry tenet.

Other commentators on the Strong Program have understood the principle of symmetry and Bloor's criticism of the teleological modal as my interpretation, not Collin's. These include the long list,[2] including Larry Laudan (1981, 187-8), Martin Hollis (1982, 75-8), Newton-Smith (1981 253-7), John Worral (1990, 316-7), Paul Boghossian (2006, 113 and 117-8) and most interesting of all, Collin himself (2011, 31).[3] All of them have taken Bloor's criticism in such a way that evaluation should not be effective in the way of explanation and have argued

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[2] The list I have given here contains only commentators who have dealt with rational symmetry, not epistemic or pragmatic symmetry. But critics of these two principles have also argued against the Strong Program's symmetrical explanations, stating that evaluating beliefs in terms of their truth or success can affect how they are explained (See Newton-Smith 1981, 252; Anthony Flew 1982: 367; Bruno Latour 1999, 117-118; and Tim Lewens 2005: 572, among others).

[3] To be fair, I should say that Collin's statement is ambiguous between epistemic symmetry and rational symmetry.

against this very idea. None of them intended to defend the duality of rational and sociological explanations in the face of Bloor's criticism.

Intriguingly, Bloor responded to some of his early critics, who sought to defend the teleological model. In these responses, he did not accuse them of misunderstanding his criticisms of the teleological model, nor did he say that he merely rejected the contrast between rational explanation and sociological explanation. Rather, by emphasizing the presence of social factors in all science, Bloor states that causal and sociological explanations must be sought. See, for example, Bloor's response to Laudan (Bloor 1981), or to Newton-Smith and Worral (Bloor 1991, 177-9).

Moreover, conforming to Collin's commentary, the teleological model too, offers symmetrical explanations; since explanations in terms of social causes and epistemic reasons offer the same type of explanation. Thus, another piece of evidence against Collin's account includes Bloor's words in which he explicitly denies this claim and calls the traditional method asymmetrical. 'The teleological model,' he says, 'violates the requirements of symmetry and impartiality' (1991, 12). Here, too, Bloor explicitly accuses proponents of the teleological model of presenting an asymmetrical explanation: 'Reject that model and all its associated distinctions, evaluations and asymmetries go with it' (12). And he says most explicitly that the teleological model 'represents an extreme form of asymmetry and so stands as the most radical alternative to the strong programme with its insistence on symmetrical styles of explanation' (13).

These indicate that Bloor himself regards the teleological model as asymmetrical, consisting of heterogeneous types of explanations. His complaint, therefore, is not why proponents of this model mistakenly assume that their model is asymmetrical. Nor is it a question of Bloor committing himself to two kinds of explanation simply since he uses the term "rational explanation." I think the sum of this evidence leaves no room for doubt that Collin stands against the orthodox interpretation of the Strong Program.

There are still more problems lurking in Collin's interpretation. Bloor launches the symmetry tenet as a methodological principle in explaining science. Therefore, one of the goals of the Strong Program is to contribute a symmetrical explanation of the history of science. In *The Enigma of the Aerofoil*, for example, Bloor attempts to provide a symmetrical picture of the history of the aircraft wing and hopes that 'seeing the symmetry principle in operation will help convey its meaning more effectively than merely trying to capture it in verbal formulas or justify it by abstract argument' (2011, 6). In describing this method, Bloor says that 'explanation of the German behavior is thus of the same kind as my explanation of the British. The same variables are involved, but the variables have different values' (6).

But, according to Collin, there is no fundamental distinction between the symmetrical work of sociologists of scientific knowledge and the work of traditional historians of science. Since, in his eyes, rational explanation and sociological explanation are one and the same; they both offer symmetrical explanations of science. Put another way, according to Collin, the Strong Program's only problem is with historians' misconceptions that rational

explanation and sociological explanation are of two different kinds; but what they have presented as the history of science is really symmetrical. This is what I have mentioned as one of the negative consequences of uniting the rational and the social (Shahryari 2022, 199); while Collin accepts it wholeheartedly.

Thus, relying on Collin's picture, no change in the historiography of science is required, and the Strong Program has never pursued this goal. This program, as Collin relates the story, had only philosophical and theoretical ideas about social construction of epistemic reasons, not methodological guidelines for symmetrical historiography of science. Collin presents his interpretation of the symmetry principle as its correct understanding. He very rarely refers to the Strong Program's texts; nevertheless, the textual evidence I presented earlier refutes his reading. Even if we consider his claim not as the Strong Program's interpretation but as a correction in it, the result is still not promising. For, as I have just shown, this proposal addresses the tension at a very high cost—that is unlikely to be covered by the Strong Program's proponents.

Epilogue

The social constructivism of rationality, if limited to the sense that social factors play a central role in the formation of all reasons, can justify the principle of rational symmetry. Since it demonstrates that in explaining all rational beliefs social factors have a significant role, as well as beliefs influenced by indoctrination and propaganda and … (to wit, irrationally held beliefs). But if it goes beyond that, realized in the extreme sense that reason is nothing but social factors (Bloor 1984, 297) and, therefore, rational and social are essentially of the same kind (Bloor 2011, 406), it will conflict with the presuppositions on which the rational symmetry tenet is based—assuming the evidence I have gathered for my interpretation is convincing.

Note that my discussion pertains to the relationship between the construction of rationality and rational symmetry and does not imply anything about epistemic or pragmatic symmetry. Put differently, the principles of symmetry do not require each other, and this gives a new twist to the discussion. This, first, may make a case for Collin's interpretation; since it is conceivable, albeit improbably, that the asymmetry of the teleological model in Bloor's view simply means it transcends epistemic and pragmatic symmetry, not rational symmetry. In the same way, it can no longer be criticized that the unity of the rational and the social makes the symmetrical historiography of science something like classical historiography. Because these two styles of historiography are similar only in rational symmetry and not in other types of symmetry. Nevertheless, it should be borne in mind that textual evidence, like empirical evidence, cannot conclusively determine the truth or falsity of a theory—i.e. the author's idea. Therefore, here too we must go to the explanatory virtues and recognize which interpretation best explains the textual evidence. Accordingly, I think the quotes I provided, along with the other evidence cited, broadly support my interpretation and challenge Collin's exegesis.

Further, relevant to the first point, the distinction between different principles of symmetry raises new and serious issues: Bloor argues that the truth and success of theories are as much a social construct as their rationality. We must appreciate whether these arguments are such as to justify epistemic and pragmatic symmetries; or, conversely, the tension I mentioned between rational symmetry and the social construction of rationality is true of epistemic symmetry and the social construction of truth, as well as pragmatic symmetry and the social construction of success. Answering this question requires further investigation; it should be noted, however, that by calling the teleological model "asymmetrical", Bloor has committed himself to two kinds of explanation and two kinds of cause. This has made it difficult for him to defend maximal social constructivism while retaining his criticism of the teleological model.

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