PART XII

LAUDAN'S PROGRESS AND ITS PROBLEMS

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Towards a Richer Model of Man: A Critique of Laudan's *Progress and Its Problems*

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In setting forth a new theory of the growth of scientific knowledge, Larry Laudan shows that any account of scientific change has consequences for the relationship between the history, philosophy and sociology of science. It is a laudable feature of his work that he does not treat any of these disciplines as undifferentiated monoliths. In fact, one of his main goals is to show that his account of progress requires specific ways of doing and relating these three disciplines. As an historian invited to speak at the Philosophy of Science Association, it seemed appropriate for me to appraise the function of history of science in Laudan's program. Without going into unnecessary detail, let me begin by examining some of his main claims.

The focus of Laudan's argument is on science construed as a form of problem-solving activity and on progress and rationality as appraisals of problem-solving capabilities. Steering an ingenious course between the Scylla of Popper's World 3 and the Charybdis of Feyerabend's world of "anything goes", Laudan believes that the virtues of both can be obtained in his world without the deficits. The unit of analysis is "the problem" and the goal of science is "solution" rather than truth, corroboration or falsification. On Laudan's account, problems come and go, inflated by solution and resistance to solution, deflated by the narrowing of domains and the waning of theories which originally defined the problem. The scientist coming to a Laudanite for recommendation and consolation will get no stern, unambiguous Popperian-type rules of method because prescriptions for future action depend upon interpretations of the relative performance of past research traditions. Even methodologies of science, including Popper's but excluding Laudan's, are historically contingent. But contingency need not be grounds for pessimism. On what Laudan calls a "broadened notion of rationality" ([6], pp. 131-132), time- and context-bound criteria of (say) intelligibility, problem-weighting and experimental control are rational if contemporary actors considered them to be compatible with what they took to be science.

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It would be absurd, after all, to criticize past choices on the basis of evidence, criteria or technologies unavailable to contemporary actors; what is necessary is that the "best available" standards of the time be used ([6], p. 130). But here Laudan is no soft-hearted relativist. Although all choices between scientific research traditions contain time- and culture-specific components, Laudan believes that his model of scientific change provides a "transcultural" and "trans-temporal" component as well which permits one to assess the progressiveness -- and, therefore, the rationality -- of any research tradition anywhere at any time. That timeless appraisal capacity is none other than the net problem-solving effectiveness of one research tradition with respect to another.

It should be clear, thus far, that Laudan would like to make historical accountability a more significant prerequisite for the job of philosophers, sociologists and practising scientists than is now the case. As an historian accustomed to the ontological disdain with which many philosophers of science treat historical case material -- one need only recall Lakatos casting "social-psychological" history into the refuse heap of the footnotes ([5], p. 381) -- Laudan's proposed reform falls on ears ready to listen. If what he proposes is accepted, then demand for historians and well-crafted historical case studies will rise and with it, both supply and status. But reform usually involves trade-offs. While Laudan wishes to upgrade the importance of the history of science for the activity of judgment-making, he wants to legitimate a *certain way* of doing history of science and no other. I shall return to this issue shortly; but first I want to take up an important related issue: the problem of "early pursuit".

1. The Problem of Early Pursuit

To ask *how* one can evaluate the success of one research program over another is also to ask *when* the appraisal is made. Laudan refers to three different times (T) after the introduction of a new research program: (T_1) immediately, which presumably means as soon as at least one person knows about it; (T_2) a short time after the appearance of the new research program (for example, 5-10 years); and (T_3) in the long run, that is, well after the old research program is judged to be dead. Two basic modes of appraisal are possible at these different times: pursuit/non-pursuit or acceptance/rejection. T_1 and T_2 are clearly interesting and highly problematic cases. Feyerabend, for example, makes the early bird adopter and pursuer of a new, not-well-supported theory the ideal of good science. The early bird is the risk taker *par excellence* and while he may occasionally be eaten by a rival bird, his supply of worms is greater than that of anyone else [2]. Laudan's early bird is less aggressive but what he loses in shock value he makes up for in (apparent) sophistication. Laudan allows for the important possibility that a scientist might accept an old, time-tested research program because of its overall problem-solving adequacy while rationally pursuing a new, barely-tested research program on the grounds that it has a higher *rate* of problem solving success. This is an appealing thesis but further differentiation of *degrees of pursuit* would be desirable. In a weak, almost trivial,

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sense merely reading an account of a new theory is a form of pursuit, one which may end in total rejection. There is also a moderate sense of pursuit: one might selectively adopt parts of a new theory because they are entirely compatible with what one already accepts and yet still reject the "hard core" components. Finally, the strong case, exemplified by (say) the case of Galileo, states that one may adopt *and* work on a new, non-well-supported theory in the face of strong counter evidence. Laudan appears to want to use "pursuit" only in the strong sense where it is almost indistinguishable from adoption; but clearly there are other weaker senses of pursuit which occur in science. Put otherwise, a certain amount of low risk-taking always takes place in science but radical risk-taking is apparently rare. Laudan and Feyerabend, while often in disagreement, want to make *some form* of risk taking a virtue of rationality and a prime feature of science. Laudan's justification is that high rates of problem-solving support hunches about the future promise of a new research tradition or its more progressive elements ([6], pp. 111-112).

At this point, we may ask just how common it is for large numbers of scientists to behave according to Laudan's prescriptions. Is Laudan, in effect, singling out a feature of how scientific communities *actually* behave or is he proposing an ideal toward which they *ought* to strive? The case of Galileo, used to much effect by Feyerabend, hardly supports the position that the "scientific community" engaged in "radical pursuit" of the Copernican theory; the case of the reception of Dalton's atomic research program would seem to offer better corroboration of Laudan's model ([6], p. 113). This leaves us with a somewhat mixed verdict -- mixed, I say, because one wonders exactly what assumption Laudan is making about the openness of the scientific community to innovation and how and why the receptivity of a given community changes over time. Consider these possibilities: the thesis that scientific communities are generally conservative will make a rationality theory of "high risks" look radical; such a philosophy will always be urging reform. On the other hand, the thesis that scientific communities are generally pluralistic and open will make a rationality theory of "high risks" look historically reasonable; such a philosophy will provide scientists with grounds for justifying what they would have done in any case. In short, it is noticeable that Laudan barely attends to the *actual* mechanisms of decision making in scientific communities. By sharply demarcating the cognitive from the social and psychological, he attends only to cognitive reasons and, in particular, putatively good reasons allegedly separable from individual contexts of belief. One cannot escape the impression that, *pace* references to John Stuart Mill's so-called "incomplete explanation condition"([6], pp. 209-210), Laudan holds implicitly a view of man as a free, rational agent, virtually never constrained by the processes of his unconscious or economic and social condition in life.[1]

2. The Problem of the Social History of Science: A Case Study

In order to bring out some of these basic criticisms, I am going to

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resort to a specific case, one with which I am intimately familiar and which most people know well: the diffusion of the Copernican theory. Strangely, the rationality of adopting heliocentrism over either geocentrism or geoheliocentrism is not included in Laudan's list of what he calls "preferred pre-analytic intuitions", cases where our normative intuitions, our gut feelings at T_3 decide the rationality of adopting a new over an old research tradition ([6], p. 160). This is strange because the so-called "Copernican Revolution" is an old and classical battleground for theorists of rationality and not a few have used it as a warrant for their claims. Yet, from the viewpoint of their accountability to the historical record, there have been many difficulties. Kuhn's incommensurability thesis, for example, fails to explain why Ptolemaic-style astronomers flocked to *De revolutionibus* immediately and borrowed heavily from its equantless models and numerical parameters while still retaining hard core geostatic assumptions.[2] Popperian naive falsificationism makes Copernican heliocentrism rational with respect to Ptolemy's astronomy *after* Galileo's discovery of the phases of Venus but cannot decide the issue with respect to Tycho's system. Lakatos and Zahar make heliocentrism the rational choice in 1543 but fail to explain the small number of pursuers and adopters between 1543 and 1650 ([5], pp. 375-381). Feyerabend's account makes all anti-Ptolemaic posturing rational on the grounds that any attacks on the hard core assumptions of a well-entrenched theory are good because theoretical rivalry and combat selects out the strongest theories. Although Feyerabend has been criticized for allowing an infinite armory of weapons to the proponents of a new theory, his is the only account of scientific change thus far which (apparently) legitimates the ten Copernicans who flourished between 1543 and 1600.[3] But let us now see what a Laudanite would say.

Copernicus' theory gave new solutions to several conceptual problems that had been anomalies for Ptolemy for 1500 years. These are, of course, precisely the same problem solutions which Kuhn appraised as "aesthetically neater" and more coherent than the Ptolemaic ([4], p. 181f.) and which Lakatos and Zahar took to be novel, dramatic, non-ad hoc facts lending *immediate* support to Copernican heliocentrism ([5], p. 376). These solved problems include: planetary retrogradations, the existence of the annual component, the bounded elongations of the inferior planets and the relative ordering of the planets by their sidereal periods and distances. Lakatos and Zahar ignore the fact that this dramatic support is purchased at a considerable price, to wit, setting the earth in threefold motion. They handle the problem of the relative weighting of conceptual problem solving success vs. failure by making the discovery of novel Zaharian facts both necessary and sufficient conditions for rational choice.[4] Now Copernicus himself did not go as far as Lakatos and Zahar. He openly allowed that in order to achieve the coherence-producing consequences of his new cosmology, one must premise an *absurd* claim ([1], p. 5). Leaving aside whether or not the facts alleged by Lakatos and Zahar are really "novel" in the sense that they wish, we can see that Laudan improves on their account already by attending more subtly to the problem of relative weighting. But what prescriptive scenario would emerge? According to Laudan, if RP_1 is *momentarily* more adequate than RP_2 but RP_2 demonstrates a *higher rate* of problem solving success, then it is rational for the actor to *pursue* RP_2 but not to accept it as though it were true ([6], pp. 111-112). This seems to take care of Copernicus himself in the first flush of theory construction (ca. 1509-1513) but it makes Copernicus irrational to accept *and* pursue his own theory in 1543 since he could not claim that his theory was a terminally effective problem solver.[5] Copernicus probably recognized this as he lay dying with the first copy of the book in his hands, perishing as he published. Indeed, until one can no longer claim that RP_1 (Aristotelian mechanics) is "momentarily adequate", there will be a weighting problem and the Laudanite will recommend pursuit rather than adoption. Now, since all ten Copernicans in the sixteenth century, including such notables as Kepler, Galileo, Bruno, Maestlin and Digges, both adopted and pursued (variants of) Copernican heliocentrism, they would be judged irrational on Laudan's account -- that is, they would be charged with overenthusiasm for their loyalties but praised for their reasoning in deciding to pursue the new, the promising and the absurd.[6] However, the highest award should go to *Tycho Brahe*, for Brahe recognized the continuing adequacy of Aristotelian mechanics even as he improved on its anomalies (removing the solid spheres) *and* he recognized the high rate of progress exhibited by the Copernican research program which led him to adopt the new ordering of the inferior planets. By the 1590's, Christopher Clavius and some of the Jesuits at the Collegio Romano saw the merits of Tycho's position and paved the way for the strong Jesuit commitment to the Tychonic system in the seventeenth century. Good Laudanites, the Jesuits!

Let us return now to a point raised in our introductory remarks concerning the *kind* of historiography of science which Laudan advocates. In his chapter on the history of ideas, Laudan rejects intellectual history-as-mere-exegesis, a history concerned solely with the articulation of texts. Instead, he calls for a stronger intellectual history which takes as its primary aim the explanation of changes and modifications of belief ([6], p. 184ff.). In this respect, I am in full agreement with him; explanatory history provides a dynamic account of change. However, Laudan wishes to restrict the historian to the search for a certain kind of *ex planans*, namely, rational explanatory laws; and, in his case, rationality consists in choices among more and less progressive research traditions. The pursuit, let alone the adoption, of social or psychological explanatory laws is sharply rejected on two grounds. First, Laudan invokes John Stuart Mill's argument that we can never hope to give a complete enumeration of all the antecedents of some event or belief X; instead, an adequate explanation is one which restricts itself to those circumstances which "seem to be most crucial and relevant to the occurrence" of X ([6], p. 210). The decision to cease enumerating causes is, then, a methodological rule and its justification forms Laudan's second main objection to the invocation of non-rational explanations. Consistent with his own model of rationality, he claims that the past record of cognitive sociology is abysmal; cognitive sociology, says Laudan, is "exegetically bankrupt", its concepts -- social class, economic background, kinships systems, occupational roles and psychological types -- are too crude for sophisticated explanations and, finally, "the vast majority of scientific beliefs (though by no means all) seem to be of no social significance whatever" ([6], pp. 218-219). In summary, then, the historian's task is to seek

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rational explanations for the holding and changing of beliefs. The theory of rationality chosen should be the best one available and that, in turn, should be determined by an appraisal of the relative success records of different types of explanation. While acknowledging, rather briefly, what he calls the "continuous interpenetration of 'rational' and 'social' factors" ([6], p. 209), Laudan believes that sociology should strive to explain only what cannot be explained by a (rich) theory of rationality such as his. Although Laudan appears to hold forth some small hope for future progress in cognitive sociology, he apparently does not think that such future research should concern itself with what we might call the "sociology of rationality".

I share Laudan's conviction that not all beliefs are socially determined but I do not share his eagerness to cut off the search for social and psychological explanations so early in the game. What I want to see and what Laudan also wants is a model of man which takes account of man's autonomy, which allows him to transcend and act back on the environment in the service of his own interests. Yet although the *ideology* of rational, autonomous man is a persuasive and appealing one, a realistic and rich model of man must also allow for man's plasticity, his unconscious living out of, and striving to repair the conflicts and wounds of childhood and the determination of his life values and goals by the social conditions of his world. Returning now to the case of the Copernican Revolution, I would like to illustrate how such an alternative model of man might be a fruitful problem solver.

If my reconstruction of the Laudanite position is correct, then it was rational for Rheticus, Copernicus' first follower, to pursue the Copernican research program but *not* to adopt it. We can give a rational explanation for his pursuit of certain features of the theory, such as the precessional model, the replacement of the equant and the determination of the lunar model: each of these innovations of Copernicus could be reinterpreted from an earth-static reference frame and its solutions applied to anomalies in the Ptolemaic-Aristotelian research tradition. For most astronomers of this period, the pursuit of the Copernican research program ended here; it ended, that is, with no consideration of the other conceptual anomalies which Copernicus had solved in the Ptolemaic research tradition. Rheticus, however, both pursued and adopted the main claims of Copernicus' research program because (we might say) he rated the significance of Copernicus' solutions more highly than did any of his contemporaries. Now clearly Rheticus' decision to pursue the heliocentric theory as a research program was influenced by his decision to adopt it and vice versa. But that would mean that an irrational choice (adoption) was a condition for a rational one (pursuit) and a rational choice (pursuit) a condition for an irrational one (adoption). We might try to resolve this problem by pointing to contextual factors. For example, Rheticus, an exponent of a Neopythagorean-Neoplatonic world view greatly valued images of harmony and, in fact, these images are invoked by Rheticus as justifications for Copernicus' new ordering of the planets. However, Rheticus was not alone in subscribing to this world view: there were many others who were Neoplatonists and who were familiar with the Copernican theory (e.g., John Dee and Robert Fludd) and who rejected it. They did not recognize such arguments as sufficient to hold the positions which Rheticus did (see my [12]). How, then, can we explain Rheticus' moves under such conditions of intense theory rivalry? A few years ago, I suggested a candidate explanation in [11]. This explanation invoked several general assumptions about human beings: (1) that early childhood experiences and perceptions influence later behavior; (2) that unresolved conflicts will tend to reappear in later life; and (3) that specific events of traumatic severity in the life of a child can intensify a low-lying conflict or initiate a new one. I then argued that Rheticus' loss of his father at the age of fourteen was an instance of an early traumatic experience. His father was beheaded for sorcery. I then predicted, prior to studying the texts, that one would find strong ambivalence and extreme reverence toward authority in Rheticus' personality and the search for wholeness and unity in significant father figures. Copernicus, I argued, represented for Rheticus the restored, unified father, a man who had no son and whose theory restored the fragmentation of the Ptolemaic monster into a whole and complete organism ([11], pp. 186-193). Rheticus' special relationship with Copernicus, then, would explain why he weighted certain problem solutions more heavily than did his contemporaries.

In another paper, I have also tried to explain the conservatism of the vast number of early astronomers who knew and read *De revolutionibus* [13]. Without engaging in great detail, my argument briefly boils down to the following claims: (1) The astronomer in the universities was expected to concern himself with teaching elementary mathematics and elementary astronomy, with the construction of almanacs and the making of planetary and astrological predictions. (2) Although he held realist ideals (contrary to Duhem), the astronomer had no warrant to justify new physical conclusions on the basis of arguments drawn from geometry. (3) The absence of a warrant to seek new physical explanations of heavenly phenomena came about as a result of a division of social/intellectual roles within the universities between the professors of mathematics and philosophy. This division was reinforced by differentials of status and income. Hence, (4) the physical assumptions made by astronomers were based upon the premises and arguments of natural philosophers, premises which the astronomers had (over time) given up the right to challenge. It became part of the expectations and sanctions attached to their role *not* to challenge them. As a result, (5) pursuing or adopting the conceptual problems solved by Copernicus entailed a change in the astronomer's role norms. (6) That change came about, eventually, as individuals began to reject the universities and to move into new social environments (especially the royal courts) freed from pedagogical and disciplinary role constraints.

Put more generally, we need not hold a strong causal account of explanation of the kind which Laudan (properly) attacks, that is, "all believers in situation Z accept belief-type X." Rather, I think that we can have a more moderate explanatory account which states that "all actors in situation Z will be *sanctioned* if they accept belief X as true or pursue belief X; but, in belief situation Z', all actors are free to hold or pursue belief-type X, X' or even not-X." The *constraint* *assumption*, as we may call it, explains *both* why most sixteenth-century astronomers accepted and sought to extend those problem solutions in the Copernican theory which were compatible with Aristotelian mechanics *and* why they did not pursue the implications of Copernicus' highly progressive solutions to a variety of conceptual problems. In short, *they were operating in a social context which did not sanction risk taking across disciplinary boundaries.*[7]

3. Conclusion

One of the significant strengths of Laudan's model of scientific growth is its capacity to draw significant generalizations from the historiography of science over the past two decades. However, in negatively judging recent work in the social history of science, he concludes that it is so unprogressive as to warrant *non-pursuit* by historians. As a result, Laudan takes a rather conservative position with respect to a burgeoning historiographical research program. I have tried to suggest in this paper that his position is too extreme. By excluding social and psychological explanations where rival rational explanations are present, he lands himself with some implausible appraisals of historical actors: Tycho Brahe and the Jesuits emerge as exponents of the most progressive research program in the sixteenth century while Rheticus, Digges, Maestlin and the *early* Kepler and Galileo are judged irrational for adopting the Copernican program.[8]

In broadening his theory of rationality to include much that is often excluded by some historians and sociologists, Laudan shows that any model of man must include an adequate place for the rational, judgmental faculties; but, I would argue, a truly sufficient model must also allow a significant place for the role of experience, especially the experiences of childhood and the constraints of social context. To have the former without the latter would be to opt, I think, for an impoverished view of ourselves.

Notes

[1]Although Laudan ([7], p. 542) states that his aim was not to develop a model of man, it is clear that his account presupposes one that has yet to be fully articulated. He writes: "Contrary to his [Westman's] suggestion, I am *not* committed to the view that man is always or even usually a rational agent; indeed, *what is remarkable, when one considers all the forces conducing him to act irrationally*, is that he manages to make rational choices at all." (My italics.) Laudan assumes here that in the absence of (unspecified) forces, man tends to act rationally. But what forces does he have in mind? And do they miraculously cease to act or are they counterbalanced when the voice of reason speaks forth?

[2]I have criticized the historical adequacy of Kuhn's account in my [11].

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[3]I say "apparently" not to indicate full agreement with Feyerabend -- there are real problems with his putatively counterinductivist reconstruction of Galileo's reasons for adopting Copernicanism (cf., Machamer [8] and Feyerabend's reply in [2]) -- but to indicate that Feyerabend's account is at least consonant with the *historical fact* that very few adopted the heliostatic theory in the sixteenth century.

[4]Note that Lakatos and Zahar at least offer an attempt at a *rational* account of what counts as a *qualitatively superior* problem solution whereas Laudan does not.

[5]There is some disagreement among historians about the period when Copernicus worked out the heliostatic theory. Rosen ([9], p. 345) believes it to have been between 1509 and 1513 but Swerdlow ([10], p. 431) doubts that one can say how long before 1513-14 Copernicus developed his new theory.

[6]Laudan ([7], pp. 543-544) contends that his statement is a form of hero worship. A rationality theory is not flawed, he argues, simply because theories judged to be good in retrospect were not judged to be worthy of acceptance immediately after their promulgation. Hence, the early Copernicans exceeded the bounds of cognitive rationality. It is interesting to note that Laudan's appraisal of sixteenth century Copernicans puts him cognitively and politically on the side of conservative academicians both Protestant and Catholic. Would he have agreed (cognitively, of course!) with the condemnations of Bruno and Galileo? It could be argued, after all, that it was situationally and cognitively rational for the Church to rid itself of threats to its authority at a time when doctrinal deviations and defections could easily be turned into propaganda for the other side. Laudan says that the available arguments and evidence changed "dramatically" in the Copernican case. But *when* this happened he does not say. Recall that Galileo's telescopic discoveries simultaneously solved problems on both the Copernican and Tychonic systems. Perhaps Laudan believes that the "dramatic change" came with Newton in (say) 1686. In that case, all Copernicans between 1543 and 1686 were cognitively irrational to accept heliocentrism as true or even probable. Surely this is not what Laudan intends when he speaks of developing a model of rational scientific change that "comes to terms with the realities of actual science" [7].

[7]Laudan's introduction of the distinction between what he calls "situational" and "cognitive" rationality in [7] appears to represent a new and promising departure from his strategy in *Progress and Its Problems*. In the book, he has virtually nothing to say about the rationality of *non*cognitive goals. The notion that an actor could simultaneously seek to satisfy both cognitive and noncognitive goals is precisely what he wishes to avoid acknowledging because he wants to show that the domain of "rationally held beliefs" is autonomous from social and emotional contexts. He thereby avoids two interesting kinds of cases: (1) where an actor's beliefs are *both* situationally and cognitively rational, where the situational context is a necessary condition for the cognitively rational processes which occur; and (2) where cognition "rationalizes"

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an emotional or situational state or where an actor transforms a situationally or emotionally rational goal into a cognitive one. His historical example of the young geneticist in Stalinist Russia who wants to keep his job but *believes* that Lysenko's theories are cognitively irrational (on Laudanite grounds) is an extreme example usually called "persecution for one's well supported beliefs." Extreme examples, however, do not usually generalize into rules although they often assist in unveiling fuzzy cases. The example which I offered in my paper, however, is not an instance of the type presented by Laudan. Consider Osiander: his *situational* goal was to defend Copernicus against the charge of (disciplinary) border violations by contending that astronomers cannot know physical causes and, hence, to imply that astronomers do not have the right to cross over into the domain of the natural philosopher with claims to such knowledge. Two observations are in order. First, Osiander's political/situational goals are rationalized by his epistemological/methodological position. Secondly, Laudan, whose own philosophy is not dissimilar in certain respects to Duhem's and Osiander's, would probably judge Osiander to be acting on cognitively rational grounds -- *independently, of course, of all situational factors*. Laudan would thereby miss something very interesting about Osiander's methodology, namely, that one reason for Osiander holding his (putatively) rational methodology was his (situational) goal of defending Copernicus against disciplinary strife. Similarly, Laudan's approach misses the "situational component" in a feature of the Lysenko Affair which he omits from his own example. Consider the case of one P.P. Lobanov, a Lysenkoite in the 1930's who became the Minister of State Farms and who, since 1965, as president of the Lenin Academy of Agricultural Sciences, has been responsible for the systematic destruction of Lysenko's agrobiology. Was he a sincere, cognitive rationalist who merely suppressed his "true beliefs" for thirty years? According to David Joravsky, the premier historian of the Lysenko Affair: "Better to disregard the problem of sincerity and genuine thinking when dealing with such a man. He typifies the bureaucratic intellectual: his position influences his thought much more than his thought influences his position. One might even say that his position in the bureaucratic hierarchy determines his position in thought. A Soviet novelist [Leonid Leonov] -- who shares this quality himself -- called such people not *ortodoksy* but *vertodoksy*, not orthodox but weathercocks" ([3], p. 178). How many cognitively rational thinkers on Laudan's account are weathercocks?

[8]Laudan rightly challenges me to provide a reason why the sixteenth century Copernicans were behaving rationally. I shall not here feign a reply in terms of a general theory of cognitive rationality (footnotes have their limits!) but I do promise at least a situationally rational account in the near future.

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