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Sociobiology Helps - But not Enough[1]

Introduction

There has been a recent revival in attempts to give a biological explanation of some aspects of scientific inquiry. That man's cognitive capacity is an evolutionary product is relatively unproblematic but at the same time uninteresting, for accepting it does not give concrete guidelines to specific issues. Thus, when Popper argued that growth of knowledge resembles Darwin's natural selection he only gave a vague analogy: there is natural selection of hypotheses, for "our knowledge consists, at every moment, of those hypotheses which have shown their (comparative) fitness by surviving so far in their struggle for existence" (Popper, 1972:261). The main obstacle on the way of this proposal was the difficulty of turning the analogy into a literal insight, for natural selection requires heritable variation of fitness. Another protruding obstace was provided by the now fashionable historicist approach into the development of knowledge: given the fleeting and ephemeral changes in theories and even in the very standards of theory choice, it seemed difficult to see how science could possibly be biologically grounded, in any interesting sense. The upshot was a strengthening of the feeling of initial implausibility of the claim that biologically grounded mechanisms could suffice to illuminate the upper edifice of knowledge, scientific concept and theory formation.

However, there is a reformulation of the evolutionary epistemologists' proposal which tries to establish that natural selection has had, after all, a role to play in the history of science.[2] Michael Ruse (1985) has argued that evolutionary epistemology is more than an analogy, and that, with a little help from sociobiology, the substantial amount of literal truth in it is easy to appreciate. Sociobiological credentials may not extend to the explanation of particular scientific beliefs and theory choices, but a modified thesis still retains pausibility: the epistemological virtues that enter into scientific belief formation are biologically grounded.[3]

I shall start by outlining the general argument for an evolutionary account of epistemic virtues, and then ask if it is indeed plausible to think, as Ruse now suggests, that "sophisticated modern science is simply jungle-lore writ fine and small". I then turn to an example which makes even the modified sociobiologist case suspect by showing that refined intuitions of theoretical elegance militate against one pausible innate epistemic virtue. The note ends with some speculations on human and animal reason. The suggestion is in keeping with mainstream epistemology: once Evolution has produced theoretical reason and replaced God as an explanatory principle, it has turned into a Deist.

Sociobiology and the genesis of epistemic values

Ruse's view is, roughly, as follows. Although Homo Sapiens differs from other species in that it has language, customs, morality and religion, it has not thereby escaped its biology. Morality is simply a biological product of our need to get along with fellow members of our species. Similarly our cognitive capacity has evolved as an adaptive response to biological needs. What Ruse proposes is a strategy for deriving and explaining cognitive values that are constitutive of knowledge. Empirical evidence is needed to substantiate specific claims, but assuming that such evidence is forthcoming, and assuming that we have been able to identify certain features that scientists take seriously, a sociobiologist can raise questions about the nature of these features and the end products of inquiry.

What are the features that scientists take seriously? Philosophers of science have been wary of giving lists of the relevant epistemic virtues, and even more of attempts to give these notions a transcendental grounding. But there is relative unanimity, from such present or former realists as Richard Boyd and Hilary Putnam to antirealists like Kuhn, that correctness, accuracy, or truth is high in the list. The realists have argued that a being whose representation of the world is correct or accurate or truthful is likely to survive, and Kuhn has maintained that accurate fit with the deliverances of observation is the *sine qua non* of scientific inquiry. (See, e.g., Boyd, 1980; Putnam 1978; Kuhn, 1977; 1983).

Although having this feature no doubt contributes to survival, it is not the only relevant cognitive value. A being can play it safe on the cognitive side, and maximise its chances of being correct by accepting trivialities. But this strategy leads to paralysis and can be disastrous to overall safety. For this reason there must be what Isaac Levi has called informational values, information content, explanatory power, simplicity, and the like.[4]

Ruse accepts this bifurcation, noting that opinions "divide over the ultimate nature of science and reason of acceptance of one theory rather than another." Perhaps the most fundamental dispute is between those who argue that empirical evidence is decisive, and those who rather refer to simplicity and like desiderata as final arbiters. Ruse does not counsel abandoning the criterion of fitting the facts. Rather he advocates a middle line, proposed by Wilson, which, Ruse says, would explain why scientists behave the way they do. In this picture, fitting the facts is absolutely basic in science, but not enough: "For instance, thanks to natural selection, notions of simplicity are going to be crucial in science as elsewhere in life. One chooses the simplest curve to link the dots, not because it has some ontological claim on being right — or because it is absolutely determined by facts — but because it is the simplest. It is the curve that requires least human hassle, as we strive to manage and use the information of our senses. To put the matter bluntly, the cave man with a taste for the complex, tended still to be working out the mathematics, whilst his simple-minded cousin was getting right down to feasting and copulating." (Ruse, 1985:256).

As this quotation shows, Ruse's account owes much to Wilson's sociobiological strategy for linking biological and cultural evolution. According to Lumsden and Wilson there are culturgens, such as artefacts, pieces of behavior and mentifacts (e.g., concepts), which are relatively homogeneous and can be looked upon as the basic units of culture (Lumsden & Wilson, 1981:27). Scientific theories, ventures Ruse, are comprised of culturgenes, and the central problem now is how these culturgens were learnt, organized, and passed on from one generation to the next. Lumsden and Wilson explain this process through what they call epigenetic rules, innate dispositions which constrain the ways humans think and act. These rules are on two levels. First, there are epigenetic rules which organize the basic items of information, e.g., color sensations, smells and sounds. Second there are secondary rules which organize this basic information, enabling us to cope with the results of the first level rules.

Whatever these secondary rules look like, they must, according to Ruse, give rise to our basic mathematics and logic. Thus epigenetic rules would support the inference that 2+2=4. Secondary rules also bear on the methodology of science, directing our thinking towards laws, especially causal ones. Given a set of initial conditions we expect the consequences, and it is apt to think that evolution has taught us to respect causal thinking. Finally, along with the drive to causal laws there is a push to simplicity and unification: science aims at a maximally simple representation of nature.

But what is the connection with evolutionary epistemology? The mathematics, logic and methodology of science have emerged as products of secondary epigenetic rules, and these rules supposedly have emerged because they have given our ancestors an evolutionary advantage. The claim is not that biology is able to explain the contents of our scientific theories in all their details, because it is implausible to think that possession of a scientific theory as such confers reprductive value. However, although we should not expect that the notions or theories we have fit with our immediate adaptive needs — or that the biologically inculcated criteria always lead to the right result (Ruse 1986:174, 163). There is an indirect way in which evolution helps: our appreciation of the relevant desiderata involved in theory choice are biologically based. We have come to appreciate the right logic and mathematics, concern with cause, predictive fertility, and unifaction simply because "those proto-humans who took these concerns seriously tended to survive and outproduce those that did not" (Ruse, 1985:254). And Ruse writes: "The Australopithecine who (say) took note of changing leaf-color and who then predicted coming climatic changes (and took appropriate action) tended to get through the winter a lot better than his rival who blithely withered away the summer and fall". The same is true of consilience. Ruse says that the pre-human who refused to take coincidences at face-value and looked for unifying causes was able to add up the water-hole and traces of blood, and to infer to an unseen tiger.

Unity of method

According to Ruse (1985:255) we still have this form of inference with us. A physicist who fails to accept unseen entities may lose the Nobel Prize: "But cave-man and physicist are linked by the same epigenetic rules, ... Moreover, not only are there reasons why we accept one scientific theory rather than another — and why indeed we create and accept science at all — firmly rooted in our biology, but even now adaptive advantage is not totally divorced from the success of science."

Exactly how would the indirect explanatory strategy carry over to the choice of particular scientific theories? It is hardly conceivable that we have a separate epigenetic rule for the adding of any two numbers, for this would result in their infinite number. It is here that the interplay of the various types of epigenetic rules comes to play: the push towards simplicity and consilience favors beings who are able to devise theories, because theories give an economic way of generating facts. This is Ruse's solution. "But the Darwinian has a direct and unforced way out of this dilemma. The statements of logic and mathematics, particularly the complex ones, do not exist in splendid isolation. They are built up from simpler statements, by fixed rules. That is what axiomatic systems are all about. Thus, even though Pythagoras' theorem may not have an epigenetic rule to itself, the fairly basic ideas expressed in Euclid's axioms seem relatively close to nature. Grasping ideas about going in straight lines, and so forth, could certainly be of use in life's struggles. In other words, you can readily argue that more advanced mathematics is an epihenomenon on a biologically based set of simple statements and rules." (Ruse, 1986:169-170).

There are two important objections to the idea that the cave-man and the physicist are in the same methodological boat. First, one could argue that the emergence of alternative logics and non-Euclidean geometries shows that the logics and mathematics which actually manage to represent the world cannot possibly have their roots in biology. Ruse's strategy is to distinguish between issues that matter to everyday concerns and those that don't. To the extent alternative logics and mathematics is play with abstract possibilities, they cannot give cause to alarm. One can certainly deviate from a system grounded in epigenetic rules, just as one can play all sorts of fantastic games. The use of non-Euclidean geometry and unorthodox logics in physics, however, is not mere game playing. Yet, thinks Ruse, Darwinian epistemology survives, for "The epigenetic rules were developed to help with day-to-day living, not for mapping the intricacies of the universe in the pursuit of Nobel Prizes." (Ruse, 1986:171). Whatever the logic and mathematics of quantum and relativity theories are, to get across a room as quickly as possible we do not walk along a curved line.

Actually, argues Ruse, resort to alternative logics and mathematics rather speaks for, not against, epigenetic rules, because the former are needed to square theories with facts. The lesson to draw is that the rules are ordered: we rather jettison Euclidean geometry than, say, modus tollens. The ordered rules are guides to thought and action, but there is no reason to expect perfect fit. This flexibility in balancing goes towards showing that "the deeper you dig into modern science, the more Darwinian it becomes." (Ruse, 1986:171).

Apart from the fact that it is questionable whether this move is within the bounds of sociobiology, the answer is not enough: for collaterally it requires an assumption of methodolocial unity. Even if we were able to isolate, these outlandish cases, it remains to be shown that animal reason and human theoretical reason are essentially identical on issues in the remaining territory. Fortunately there is an independent argument, by Elliot Sober (1981), which appears to be ideal for this purpose. "If there really were separate inferential techniques — one specific to tasks whose outcome mattered to survival and reproduction, the other applicable to tasks which had no such practical consequences — it would be impossible to talk about selecting for the methodology of theoretical reason." But such a state of affairs is unlikely, because "it would be unfit from an informational point of view for an organism to encode separate inferential techniques when a single all-purposed one would serve" (Sober, 1981:108-109). Memory space is in limited supply, and the assumption that humans would be endowed with two sets of rules, one for day-to-day affairs and another for theoretical matters, is therefore a non-starter.

Simplicity of use and theoretical elegance

Let us see how far this strategy takes us towards establishing that methodological norms and epistemic virtues have stayed the same from cave men to modern physicists. Leaving truth and explanatory power aside for the moment, there are problems with simplicity and consilience. Perhaps the most important theoretically relevant type of simplicity or elegance is what Kristin Shrader-Frechette (1988), following J. J. C. Smart (1984), calls Ockham's Razor Simplicity (O-R simplicity, for short). Now it has been admittedly difficult to give a formally satisfactory notion of simplicity, but the following informal characterization of comparative simplicity suffices here: a theory T1 is O-R simpler than T2 if it has a smaller number of principles, laws, properties or entities.

Is it, then, plausible to think that O-R simplicity could have evolved as the notion favored by evolution? Is an O-R simple system one which "requires the least human hassle"? Hardly. My counterexample comes from the province of logic but can, I shall maintain, be extended beyond. Let us distinguish, following Susan Haack, between a narrow and broad way of defining logical systems (Haack, 1978:18—22). In the narrow sense L1 and L2 are two formulations of the same system when they have the same axioms, and/or rules of inference, once notational differences are discounted. In the broader sense L1 and L2 are formulations of the same system if they have the same theorems and valid inferences, given that allowance has been made for different notations and choices of primitive constants.

Now there can be, in the broad sense defined, alternative formulations of propositional logic which generate the same set of truths, but contain unequal numbers of axioms and rules of inference. If we eye the various systems it becomes quickly clear that the differences in formulation highlight different advantages, and manifest different notions of simplicity: some are simple to use or apply, whereas others excel in O-R parsimoniousness. Thus Mendelson's system, with its three schemata, is less OR- simple than the system designed by Meredith: the latter has only one axiom and two rules of inference. Now, Meredith's system has negation and material implication as primitive connectives, but Nicod showed in 1917 that one can simplify this too. He showed that one could take P/Q (Not both P and Q) as primitive and derive the entire propositional calculus from one single axiom and one rule of inference. These are:

[p|(q|r)]|([t|(t|t)]|{(s|q)|[(p|s)|(p|s]}) and

$$\frac{P \quad P|(Q|R)}{R}$$

Nicod's system may have a high degree of O-R simplicity, and as William and Martha Kneale (1984:526-527) write, Russel suggested in the second edition of *Principia Mathematica* that this formulation could be substituted for the one employed in the first. But it does have a drawback. As the Kneales put it, rather mildly, "it can scarcely be said that the reduction achieved by Nicod is a simplification which makes the theory easier to grasp". And they continue: "If what is wanted is perspicuity and naturalness in the presentation of arguments, the best set of axioms for use with the principles of substitution and detachment is that given by Hilbert and Bernays in 1934. It contains fifteen axioms, grouped together in sets of three."

But it is pretty clear that if we have perspicuity and naturalness as crucial desiderata, the real choice is not between the various axiomatic systems but between axiomatic systems and systems of natural deduction. A natural deduction system has no privileged set of formulas designated as axioms. Rather, it has a great number of rules of inference (and a rule of assumption which licenses a start without axioms). The advantage of all systems of natural deduction is that they are intuitively appealing and easy to apply — indeed, that is why we have Lemmon's *Beginning Logic*—yet sufficient to study properties of formal systems.

And recall how Gentzen, the founder of natural deduction systems, starts his classic "investigations into logical deduction". "My starting point was this: The formalization of logical deduction, especially as it has been developed by Frege, Russell, and Hilbert, is rather far removed from the forms of deduction used in practice in mathematical proofs. Considerable formal advantages are achieved in return. In contrast, I intend first to set up a formal system which comes as close as possible to actual reasoning. The result was a 'calculus of natural deduction ('NJ for intuitionist, 'NK for classical predicate logic"). (Gentzen, 1969:68).

Let me draw some interim conclusions. To start with, if the epigenetic rules are geared to licensing our basic inferences, all these systems are equally good. But once we start the climb up to an axiomatic or other way of systematizing logical truths, we face the problem that the various desiderata compete. The problem is not merely that the desiderata are distinct, but that they are ill at ease with one another. Moreover, Ruse's proposal that the axioms of Euclid seem so "close to nature" that they have an epigenetic rule of their own hardly carries to the most O-R simple axioms of logic or physics. And it is interesting to note that Gentzen was fully aware of the tension. Writes Szabo, editor of Gentzen's collected works: "In view of Gentzen's efforts to find more 'natural' methods in mathematical logic, it is not surprising that his first consistency proof for elementary number theory (§ 4) is formalized in terms of an N-type calculus (NK). [N-systems were just systems designed with naturalness in mind], where simplicity and elegance of procedure are sacrificed to the demands of 'naturalness'. In § 8, Gentzen reverses his methods and uses an L-type calculus (LK) in order to simplify his consistency proof, but, in doing so, jeopardizes some of the naturalness in procedure". (Szabo, 1969:4).

Animal reason and theoretical reason

Let us go back to our initial question: Could sociobiology help us explain epistemic values, and hence, indirectly, theory choice? The problem is that although humans might be innately disposed to value certain types of cognitive virtues, these dispositions are indeterminate with respect to more specific descriptions of these virtues. They appear be more akin to capacities to learn or develop a taste for some determinable virtue than inborn dispositions to prefer a determinate one. This indeed appears to be the view of new-wave sociobiologists such as Lumsden. Whereas classical sociobiologists focused on reactions allegedly hard-wired into the brain, Lumsden and Gushurst (1985:17-21) now advocate an alternative in which behavior is not reducible to genes and genetically determined responses. Rather, their gene-culture theories focus on "the epigenetic rules that make the culture-learners more likely to acquire certain systems of ethical knowledge rather than others" — and the same applies to secondary epigenetic rules involved in belief formation.

Although this proposal goes some way towards explaining cognitive virtues, it appears that it is too coarse-grained for the purposes of illuminating scientific belief formation. Some notion of truth, correctness, accuracy or empirical adequacy is a sine qua non in theoretical science — but the sociobiological strategy does not tell more exactly what this notion is. The counterexample to biologically based simplicity preferences must be seen in the same light: if indeed O-R simplicity and simplicity as ease of use are two distinct notions, two determinants of a generic determinable; if these notions are exemplified in systems in roughly inverse proportions; if O-R simplicity is what the theoretical physicist is after, while perspicuity and naturalness (ease in application) is what counted in the jungle (as is plausible to think), we have undermined the claim that science is the more Darwinian the deeper we dig. Although the determinable virtue may be biologically grounded, the determinate is not.

One objection that might be advanced against my example is that it centers on logic, not empirical science. But this is no drawback. On the contrary, when using this example to explore our intuitions, we need not pitch the various notions of simplicity against other virtues, such as scope or explanatory power. In choosing between scientific theories there is always the difficulty that we may not be judging rivals for their simplicity or intellectual economy, but rather for their undifferentiated degree of simplicity-cum-explanatory power-cum accuracy. Furthermore, such choices may also reflect expectations about the feasibility of a research program — after all, one is never in the position to judge between two fully developed theories, with all evidential data at hand. But in the logical example we do not have decision-making under risk but under certainty: what you see is what you get.

Two further related objections come easily to mind, and they lead us quickly to the crucial issues, for it could be maintained, secondly, that there is nothing wrong about the sociobiological strategy. Rather, what the example shows is that simplicity must be deleted from the list of features scientists take seriously. Simplicity may play a heuristic but not a justificatory role — after all, it easily leads to erroneous choices. And this in turn goes towards showing that as an epistemic virtue it is at best secondary, operating in the context of discovery and not justification (see Shrader-Frechette, 1988, for three arguments against simplicity). Third, it might be countered that the virtues as such are biologically inculcated, but specific weightings are not. As the examples of epigenetically wayward logics and mathematics shows, there is and must be leeway for judgment in balancing criteria.

Neither one of these objections saves evolutionary epistemology, in the modified yet demanding form proposed by Ruse. Take the second objection first. Simplicity as ease of use no doubt is an important and possibly innate desideratum, and OR-simplicity a serious theoretical substitute. But if sophisticated reason coounsels anything here, it is suspicion towards both. It could be surmised that simplicity as perspicuity and naturalness are supported by primary epigenetic rules which, as Lumsden and Gushurst write, "regulate the development of systems ranging from the peripheral sensory filters to perception", while the secondary rules "assemble the inner mental processes ..., including the procedures of consciously deliberated valuation and decision making" (Lumsden & Gushurst, 1985:7). Thus both would be innate, one governing jungle aesthetics, the other one theory aesthetics. But apart from being mildly speculative (at the moment, anyway), there is a more serious methodological objection to the proposal. To see this, consider Ruse's approving reference to Quine. Quine, a naturalist predecessor of Wilson's and Ruse's own Darwinian epistemology, has argued that natural selection explains why our innate quality spacings are so successful. But Ruse fails to mention that in the very next paragraph Quine documents the limits to animal reason: nature has given us not just inborn similarity (and simplicity) measures but also the possibility of rising above them (Quine, 1969:13). Although the possibility of rising above jungle aesthetics somehow must reside in man's biologically based cognitive capacity, theoretical reason essentially consists of deliberation in which first inclinations are kept in suspense. The point of the logical example should be seen in the same light: it is not important to establish the innate (though dispositional) supremacy of this or that ingredient of the generic notion of simplicity, but rather, to see that innateness is not what counts.

The third response is, I think, well-taken, but with it we lose whatever is left from the sociobiological strategy for saving evolutionary epistemology. Early evolutionary epistemology failed because it was implausible to think that the detailed contents of beliefs or scientific theories were biologically grounded. Ruse now proposes that evolutionary epistemologists change tack and argue, instead, for biologically grounded norms and epistemic values. But the difficulties remain. Traditional epistemologists can easily accept that reason is a product of evolution, but insist, with Quine, that this product does not come with detailed genetically wired instructions for use. Nor is it of substantial help for the sociobiologists of more recent persuasion to maintain that these instructions are not hard-wired or detailed, for it then becomes difficult to delineate this strategy from that of the traditional epistemologist: what force does the insistence on biological grounding have any more? The thought that emerges, then, is this: now that Evolution has taken the role of God as an explanatory principle, and produced in man a capacity for inference and deliberation, it has turned into a deist: it keeps a fatherly (or motherly) eye on its creations but has decided, in the interest of these very creatures, not to interfere with their daily affairs.

NOTES

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2. For the distinction between the weak evolutionary epistemologists' thesis that cognition is a biological phenomenon, and the more ambitious thesis that scientific change can be modelled on evolutionary-biological lines, see Callebaut and Pinxten (1987, especially p. 4).
3. Campbell (1987) maintains that evolutionary evolution is not committed to the strong thesis that specific beliefs can be explained through natural selection.
4. See Levi (1980), chapter 2.4. In an illuminating discussion of the emergence of rationality Elliot Sober (1981) explores the genetic wiring of our cognitive apparatus. According to him a technique for constructing beliefs is rational if it is reliable and fruitful. Reliability has to do with the frequency with which the technique produces true or approximately true beliefs. But, Sober writes, "since a policy which almost never constructs beliefs at all might be reliable, in this sense, we also require of a rational procedure that it yield beliefs that are general, nontrivial, explanatory and simple". This latter set of virtues Sober calls fruitfulness, and it appears to correspond to Isaac Levi's batch of informational values.

REFERENCES

Boyd, R.N.
1980 "Scientific Realism and Naturalistic Epistemology". Pp. 613-662 in P.D. Asquith and R.N. Giere, (eds.), PSA 1980, Vol. 2. East Lansing, Michigan: The Philosophy of Science Association.
1985 "Observation, Explanatory Power, and Simplicity." Pp. 47-94 in P. Achinstein and O. Hannaway (eds.), Observation, Experiment, and Hypothesis in Modern Physical Science. Cambridge, Massachusetts: The MIT Press.

Callebaut, W. and R. Pinxten
1987 "Evolutionary Epistemology Today: Converging Views from Philosophy, the Natural and the Social Sciences." Pp. 3-55 in W. Callebaut and R. Pinxten (eds.), Evolutionary Epistemology. A Multiparadigm Program. Dordrecht: D. Reidel.

Campbell, D.T.
1987 "Selection Theory and the Sociology of Scientific Validity". Pp. 139—158 in W. Callebaut and R. Pinxten (eds.) Evolutionary Epistemology. A Multiparadigm Program. Dordrecht: D. Reidel.

Gentzen, G.
1969 The Collected Papers of Gerhard Gentzen. Edited by M.E. Szabo. Amsterdam: North-Holland Publishing Company.

Haack, S.
1978 Philosophy of Logics. Cambridge: Cambridge University Press.

Kneale, W. and M. Kneale
1984 The development of Logic. Oxford: Clarendon Press.

Kuhn, Th.
1977 "Objectivity, Value Judgment, and Theory Choice." Pp. 320—339 in T. Kuhn, The Essential Tension. Chicago: University of Chicago Press.
1983 "Rationality and Theory Choice". The Journal of Philosophy LXXX, No. 10:563—570.

Levi, I.
1980 The Enterprise of Knowledge. An Essay on Knowledge, Credal Probability, and Chance. Cambridge, Massachusetts: The MIT Press.

Lumsden, C.J. and E.O. Wilson
1981 Genes, Mind, and Culture. The Coevolutionary Process. Cambridge: Harvard University Press.

Lumsden, C. J. and A.O. Gushurst
1985 "Gene-Culture CoevolutionÖ Humankind in the making". Pp. 3-28 in James E. Fetzer, ed., Sociobiology and Epistemology. Dordrecht: D. Reidel.

Popper, K.R.
1972 Objective Knowledge: An Evolutionary Approach. Oxford: Clarendon Press.

Putnam, H.
1978 Meaning and the Moral Sciences. Boston: Routledge & Kegan Paul.

Quine, W.
1969 "Natural Kinds". Pp. 1-23 in Nicholas Rescher (ed.), Essays in Honor of Carl G. Hempel. Dordrecht: D. Reidel.

Ruse, M.
1985 "Evolutionary Epistemology: Can Sociobiology Help?". Pp. 249-265 in James H. Fetzer (ed.), Sociobiology and Epistemology. Dordrecht: D. Reidel.
1986 Taking Darwin Seriously. A Naturalistic Approach to Philosophy. Oxford: Basil Blackwell.

Shrader-Frechette, K.
1988 "Three Arguments Against Simplicity". Forthcoming in N. Rescher (ed.), Aesthetic Properties in Natural Science. Lantham, maryland: University Press of America.

Sintonen, M.
1988 "Taming a Regulative Principle: From Kant to Shlick". Forthcoming in N. Rescher (ed.), Aesthetic Properties in Natural Science. Lantham, Maryland: University Press of America.

Smart, J.J.C.
1984 "Oskam's Razor". In J:H: Ferzer (ad.), Principles of Philosophical Reasoning. Totowa, New Jersey: Roman and Allenheld.

Soper, E.
1981 "The Evolution of Retionality". Synthese 46: 95—120.

Szabo, M.E.
1969 "Introduction". Pp. 1-28 in M.E. Szabo, The Collected Papers of Gerhard Gentzen. Amsterdam: Nort-Holland Publishing Company.

Wiison, E.O.
1975 Sosiology: The New Synthesis. Cambridge, Massachusetts: Harvard University Press.
1978 On Human Nature. Cambridge, Massachusetts: Harvard University Press.

Wuketits, F.M.
1984. "Evolutionary Epistemology – A Challenge to Science and Philosophy," Pp. 1-33 in F. M. Wuketits (ed.), Concepts and Approaches in Evolutionary Explanation. Towards an Evolutionary Theory of Knowledge. Dordrecht: D. Reidel.

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