

TOWARDS A RATIONAL
PHILOSOPHICAL ANTHROPOLOGY

THE VAN LEER JERUSALEM FOUNDATION SERIES

INTELLECTUALS AND TRADITION

Editors: S. N. Eisenstadt and S. R. Graubard

SCIENCE AND VALUES: Patterns of Tradition and Change

Editors: A. Thackray and E. Mendelsohn

THE INTERACTION BETWEEN SCIENCE AND PHILOSOPHY

Editor: Y. Elkana

SOCIETY AND POLITICAL STRUCTURE IN THE ARAB WORLD

Editor: M. Milson

SOCIALISM AND TRADITION

Editors: S. N. Eisenstadt and Yael Azmon

DYNAMICS OF A CONFLICT

A Re-examination of the Arab-Israeli Conflict

Editor: G. Sheffer

TOWARDS A RATIONAL PHILOSOPHICAL ANTHROPOLOGY

by J. Agassi

FRAGMENTATION AND WHOLENESS

by D. Bohm

VARIETIES OF MARXISM

Editor: S. Avineri

THEORY AND PRACTICE

by N. Rotenstreich

EDITORIAL BOARD OF THE VAN LEER JERUSALEM FOUNDATION

S. N. Eisenstadt, The Hebrew University of Jerusalem

Y. Elkana, The Hebrew University of Jerusalem

G. Holton, Harvard University

R. K. Merton, Columbia University

M. Milson, The Hebrew University of Jerusalem

N. Rotenstreich, The Hebrew University of Jerusalem

Esther Shashar, Executive Editor, The Van Leer Jerusalem Foundation

TOWARDS A RATIONAL PHILOSOPHICAL ANTHROPOLOGY

by

JOSEPH AGASSI



MARTINUS NIJHOFF / THE HAGUE / 1977

© 1977 by *Martinus Nijhoff, The Hague, The Netherlands*
Softcover reprint of the hardcover 1st edition 1977
All rights reserved, including the right to translate or to
reproduce this book or parts thereof in any form

ISBN-13: 978-94-010-1097-9 e-ISBN-13: 978-94-010-1095-5
DOI: 10.1007/978-94-010-1095-5

SET IN ISRAEL BY ISRATYPESET, JERUSALEM

Preface

The thesis of the present volume is critical and dual. (1) Present day philosophy of man and sciences of man suffer from the Greek mistaken polarization of everything human into nature and convention which is (allegedly) good and evil, which is (allegedly) truth and falsity, which is (allegedly) rationality and irrationality, to wit, the polarization of all fields of inquiry, the natural and social sciences, as well as ethics and all technology, whether natural or social, into the totally positive and the totally negative. (2) Almost all philosophy and sciences of man share the erroneous work ethic which is the myth of man's evil nature – the myth of the beast in man, the doctrine of original sin. To mediate or to compromise between the first view of human nature as good with the second view of it as evil, sociologists have devised a modified utilitarianism with deferred gratification so-called, and the theory of the evil of artificial competition (capitalist and socialist alike) and of keeping up with the Joneses. Now, the mediation is not necessary. For, the polarization makes for abstract errors which are simplistic views of rationality, such as reductionism and positivism of all sorts, as well as for concrete errors, such as the disposition to condemn repeatedly those human weaknesses which are inevitable, namely man's inability to be perfectly rational, avoid all error, etc., thus setting man against himself as all too wicked. Be it as it may, I propose that the Greek dichotomy between nature and convention was powerful stuff that offered a challenging extremist framework for the philosophy and the sciences of man, yet one which has outlived its usefulness; in the distant past it was challenging but in the recent past it became increasingly a hindrance: it helped suppress or

confuse efforts to develop an alternative to it or challenging scientific theories that do not fall well within its confines. Strangely, it is now possible to argue that a more moderate framework than the Greek polarization of everything into nature and convention may be by far the more challenging one. A more moderate framework will enable us to develop both a better philosophy and a better science of man – to develop a better image of man. Only if we dispose of the Greek dichotomy will the road be cleared for a more moderate yet more rational picture of man as only partly rational, hopefully more at peace with himself, his partial rationality, and his partial democracy; as partly rational, but progressing – both in his education and in his ability to resolve conflicts. In brief, the chief aim of the present study is to attack the no longer so very important yet still too prevalent all-or-nothing attitude in philosophy and to offer some rudiments of an outline of a substitute to it in the theory that takes certain unpolarized human qualities as central, such as the limited (but) human dignity, suffering, understanding, and struggle.

The following five chapters evolved out of three lectures delivered (in Hebrew) at the Van Leer Jerusalem Foundation in May 1971, and one lecture delivered at the Boston Colloquium for the Philosophy of Science, Boston University, in October 1972. The first chapter is an expansion of the first lecture delivered in Jerusalem; the second chapter is an expansion of the Boston lecture and more so of points raised in the ensuing discussion; the third chapter expands the second Jerusalem lecture; the fourth and fifth chapters, the last Jerusalem lecture. I do not know why this has happened, or how the neat and orderly ascent exhibited in the table of contents – much too orderly, to my taste – has come into being. Perhaps I just repressed the obvious until it hit me in the face.

My gratitude is, first and foremost, to Dr. Yehuda Elkana of the Hebrew University of Jerusalem, director of the Van Leer Jerusalem Foundation; he invited me to deliver the lectures, took generous care of all the technicalities involved, and invited me to submit the final version plus notes (of which more in the introduction) for publication. I am deeply indebted, also, to admirable Miriam Balaban, editor of the same Foundation for her help in day to day matters, as well as for her patience and constant encouragement. And I am indebted to the Van Leer Jerusalem Foundation and to Esther Shashar, now executive editor of the Foundation for the technical assistance which went into the production of this work. Also, my gratitude to Professor Robert S. Cohen of Boston University, Chairman of the Boston Colloquium for the Philosophy of Science, who advised me on the final

composition of this work. My gratitude, also, to Professors Edward Davenport and John Wettersten who read the penultimate version and made valuable comments. The indices were prepared by Charles M. Sawyer, Eva Alkon Katz and myself. I much appreciate their help.

Of all the people from whose conversations and exchange I have profited, I hope, in the writing of this work, I can only mention the few with whom I had lengthy exchanges which I feel have left a broad and deep impression on me. These are Judith Buber Agassi, Robert S. Cohen, Shmuel Ettinger, Ernest Gellner, Edward Goodman, Daniel Greenberg, Berek Gross, Ian C. Jarvie, Kurt Klappholz, and J.O. Wisdom. I cannot adequately acknowledge my debt to them.

Any debt to the giants of our tradition cannot, of course, be expressed except in an intellectual history. I have tried to do some justice in the present work to our intellectual history, but I am all too aware of my deficiency in this respect. I hope, however, that the pervasive influence of Spinoza, Marx, Freud, and Russell will be noted by the connoisseurs; but the most immediate and direct influence on me is, of course, that of my teacher, Sir Karl Popper, whose methodology as well as social philosophy I take as points of departure.

Herzliah, Israel, Summer 1974

Sudbury, Mass., U.S.A., Winter 1975

PREFACE	v
TABLE OF CONTENTS	ix
INTRODUCTION: AGAINST THE ELITISM OF EXCESSIVE SCHOLARSHIP	1
Notes	8
CHAPTER ONE: MAN AS MACHINE	21
Notes	22
I. Positivism is to be rejected out of hand	24
Notes	28
II. Reductionism is an attractive metaphysics	35
Notes	39
III. Explanation is not elimination	46
Notes	52
IV. In praise of methodological pluralism	67
V. In praise of idle speculation	79
Notes	86
CHAPTER TWO: MAN AS ANIMAL	92
Notes	94
VI. Man-as-animal is not the animal-in-man	100
Notes	106
VII. The philosophical weakness of neo-Darwinism	119
Notes	129
VIII. The subtlety of behaviorism is sham	152
Notes	158
IX. Behaviorism as a stern moralizing	166
Notes	170
X. Anti-intellectualism explained	178
Notes	183
CHAPTER THREE: MAN AS RATIONAL	189
Notes	191

XI.	Greek metaphysics today	193
	Notes	197
XII.	Science and pseudo-science are entangled	209
	Notes	215
XIII.	Science is traditionally based on a myth	222
	Notes	225
XIV.	The myth that science is utterly rational	232
	Notes	236
XV.	Social science without the myth of science	243
	Notes	247
CHAPTER FOUR:	MAN AS SOCIAL	256
	Notes	257
XVI.	The rationality of science is partial	261
	Notes	264
XVII.	Assuming too much rationality is silly	265
	Notes	268
XVIII.	Equality is hard to define	275
	Notes	278
XIX.	Psychologism and collectivism explain away each other	290
	Notes	294
XX.	A non-reductionist demarcation between psychology and sociology	304
	Notes	309
CHAPTER FIVE:	MAN IN THE IMAGE OF GOD	318
	Notes	319
XXI.	Utopias of psychologism and of collectivism are identical	319
	Notes	323
XXII.	Skepticism rehabilitated	336
	Notes	339
XXIII.	Culture is no burden	345
	Notes	348
XXIV.	An image of the democratic man	357
	Notes	359
XXV.	Towards a rational philosophical anthropology	363
	Notes	367
INDEX OF NAMES		371
INDEX OF SUBJECTS		385

Introduction: Against the Elitism of Excessive Scholarship

Many studies, especially those of ambitious nature, suffer from excessive annotation and documentation. They exhibit too much scholarship. The social function of such exhibition is to intimidate and thus discourage the uninitiate from publishing, especially ambitious works.¹ Here I wish to discuss this function, explain my displeasure with it, and help the ambitious reader to beat the system by offering a quick and easy method of simulating the scholarly stance.

The annotation and documentation of this volume seem to me quite redundant, and certainly suffering from excess. I have undertaken it at the insistence of Dr. Elkana, who has invited me to give the lectures on which this work is based and to submit the work for publication. I therefore feel that, first of all, I owe it to the reader to explain my conduct, since I do not wish to place the responsibility for my actions anywhere else than with me.

This is not the first time that I publish a heavily documented and annotated text, and, as before, I do it with mixed feelings. The purpose of documentation and annotation is to aid the specialist reader who wishes to pursue a point further than the intended reader. For the latter, parsimony may be essential: a reader flooded with references will not easily know which to pursue. Sometimes, indeed, a crucial debt is expressed in a reference hidden in a mass of scholarly ones. The concealed purpose of heavy documentation and annotation is not so much, in my opinion, to help or confuse the judicious reader (specialists do not generally need help and they see through the confusions when they want to) as to browbeat the less judicious one. This observation is a bit unkind, perhaps, since it is the “lay” reader who usually asks for documentation. That is to say, because readers are less

independently minded than they might profitably be, they wish to be challenged as little as possible, and they can achieve that by putting a premium on challenge — such as high level of scholarship. Quite a few papers are rejected, let me observe, though admittedly stimulating, on the ground that they are “not sufficiently well documented”. All I have gained from having published extensive documentations and annotations is that my calling these grapes of scholarship sour is ascribed by popular gossip not to any inability to attain them but to some regrettable anarchistic strain in my character.

I acknowledge that heavy documentation and annotation has some limited merit: it imposes discipline on its author, it affords him the opportunity to spend enjoyable time in a library, perhaps it makes him check and eliminate some of his worst errors, perhaps also express his personality in the peculiar medium of references and notes (which few have thus far exploited). But the wood gets all too often lost for the trees, the average reader gets bullied, the average student and scholar get intimidated. In such cases, though not always, in my deep sympathy for their unnecessary suffering, I consider excessive foot-noting an evil.

The evil, to repeat, is that of intimidation. This may be located either in the act of the intimidator or in the weakness of the intimidated. It seems more reasonable — though it is not — rather to block the act of intimidation than to eliminate the weakness, the disposition to be intimidated. For, it is claimed, eliminating the weakness would require an arduous process of reeducation and self-education. And so, in the interests of the intimidated, intimidators are allegedly blocked by the existing elaborate machinery of refereeing and reviewing all scholarly and scientific publications which is supposed to eliminate all sorts of undesirable developments. This blocking acts as a sort of censorship. Censorship is usually very counterproductive. Thus, as I observe, censors in fact encourage rather than discourage intimidation: censorship does not prevent authors from intimidating their readers, but rather it enables editors to intimidate authors and even force authors to intimidate their readers. But my opposition to censorship is more of a matter of principle: it is wrong for anyone other than the author, journal editor, or publisher, to take their responsibility for their part in the publication. It is an excellent rule that the only persons who should be responsible for publication of anything whatsoever are those specifiable by the law of the land (I restrict myself here to the laws of democracies²) to be responsible for what they actually do. If anyone, in particular, feels like publishing an intimidating essay or tract, he should surely be free to do so. And we should be free to

torpedo the attempt at intimidation – by exposing his techniques as sham.

It is all too easy to appear much more of a scholar than one is, provided one is not awed by scholarship. A drop of scholarship about the history of scholarship is the best remedy against the malady of excessive respect for scholarship. From time immemorial, the scholar was one who primarily read. If he had to write, he was often gratified by expressing himself in a brief marginal note in the book he read. Since the malady of excessive awe to scholars was prevalent amongst scribes, marginal notes often got interpolated. This led scholars to make their notes obviously scholarly, their notes often consisting of documentation or of a mere scholarly hint, such as, “compare this text with . . .” The commentaries – Hellenistic and medieval – which started as one man’s documentations and annotations of another man’s book, soon developed independently, especially when more than one author was so treated, and notes appeared as strings of scholarly pearls. Naturally, first came the pearls, then the technique of stringing them, especially when the printing press came and made publication a major decision. Stringing was the hardest, for it all too often betrayed inevitable lacunae in scholarship; authors delayed publication – they still do – until they could consult a few more authors on this or that subtopic, or until they died and their colleagues, executors, or heirs made for them the decision to publish or not to publish. This is what I.C. Jarvie has christened as thoroughness mentality. This mentality suited well times of veneration of the written word, especially when it came in fits and starts, when commentaries were meant to restore the original word and its sanctity. Even today there are still writers, good, bad, and indifferent, who dare not express themselves, or do not wish to, except by presenting such strings of scholarship, and who hesitate to publish the strings unless they appear with no gap and bear testimonies to their authors’ omniscience. Other writers prefer to write their own smooth text and add a string of pearls to support them as marginal notes, much in the style emulated here. Texts without scholarship, I am repeatedly told, and emphatically so, are dismissed out of hand. All counterexamples to this are declared special cases – and with much justice: scholarship or no scholarship, any volume not dismissed out of hand is, *eo ipso*, a special case.

There is one simple method of deception, among many, which I have found regularly practiced by some of the best scholars around; I do not know why. When they say something that they cannot document they surround it by things they can heavily document. I usually

find out what they wish to say by finding the most heavily documented pages of their books and reading the undocumented sentences in these very pages. I do not know why this method of deception is practiced, except that the author who uses it may feel that he is doing something wrong which he should conceal. The something wrong here would be an unsupported claim; the code by which it is wrong is inductivism, the code which perpetuates the archaic thoroughness mentality.

Inductivism, more precisely, is two conflicting codes. One says, to be any good a theory must emerge from facts through a long process of hard study. The other says, a good theory may come first but it should be supported by facts all the same, as if it emerged from them. These two ways of seeing inductivism are what causes the trouble. One who gives up the first and endorses the second can easily comply by the inductive standard. The father of modern inductivism, Sir Francis Bacon, already noted this and fumed against it: he said that those who first speculate and then look for support usually distort the facts to fit their views. I am unimpressed: we all distort facts to some extent, and those of us who are sufficiently flexible often prefer to alter their views, contrary to Bacon's observation that people never do. He was clever but too suspicious; partly because he was no scholar and had no experience of the joy of liberation from mistakes.³

For my part, I suggest, if ever you decide to write, that you write first and read afterwards; you will find it much easier and much more enjoyable. Abandon entirely the old mode of the commentator, and when you write, make your writing your central activity, not your reading. Let your imagination run wild and serve you as a source of information. Read after you write, in order to check what you wrote. Reading may then show you in error. You can then correct your manuscript either by erasing the error and replacing it with its better alternative, or keep the error there as a step in the development of the views that you present to your reader. When it is of interest, I recommend retaining the old error (as an error, of course) and adding the correction. One way or another this will give you ample opportunity to document and annotate too, if you wish. And do not worry if not every page is documented, or else annotate without any documentation. For my part, I recommend giving the notes something of an artistic coherence, a peculiar style and a leitmotif or two. But this goes above and beyond the standard requirement. (I still recommend it, though: it is much fun.)

I have aired these suggestions with various scholars. Some were shocked, like government lawyers who were shown a new and huge

loophole in the income-tax laws.⁴ Others said: you can do it, do it! — meaning, not many are as clever as you in finding and utilizing the loophole, thank God. They are thus still concerned with limiting the number of writers. Let me, therefore, discuss why.

There is, no doubt, a horror in face of the flood of publications. Philosophers like Polanyi⁵ and Popper⁶ agree on one point: something ought to be done here. They fear, of course, trite publications; but the fear of significant ones is much more widespread. Coming to think of it, the two fears amount to the same. In the wake of Michael Polanyi, indeed, Thomas S. Kuhn⁷ has propagated the vulgar doctrine of the paradigm which says that all members of the scientific clan must emulate one paradigm, or else chaos will reign. (That is to say, even a significant work is of no avail in the state of chaos.)⁸ This claim is both untrue and pernicious, and I wish to expose the fears underlying the fear of much significant work being published. One fear is, obviously, that diverse significant writers will disagree, thus forcing the little man to make up his own mind as to whom to follow. The other fear is that he will not even have the time to read all of them. So the fear is that of being left out in the cold. It is, in brief, a herd philosophy. For, no one says, I wish all eligible bachelors except 100 or so were somehow ineligible so I could properly choose a spouse; but they do say, as Kuhn recommends, I wish there were very few competing leaders. And this is the true cause of much intimidation that goes on under the guise of the demand for excessive scholarship.⁹

The restrictive character which extensive documentation and annotation and empirical checking often have, is not only a matter of restrictive intentions. The lack of consideration for the reader alone may make the annotation messy and expressive of an author's unpleasant self-centeredness. The author may then quote an authority in order to show his independence of it, in a sort of priority claim. Claims for priority go well with intense scholarship for many reasons which are so transparent that one might wonder how scholars with so much learning and insight miss the point; trying to impress the reader favorably, they achieve the opposite.

I recommend, in opposition to this, that authors consciously develop an atmosphere which they find congenial and which emanates from their notes and compensates the reader for his effort, by pleasing him. In particular I recommend that the references be made not only to a narrow compass of scholarly work. Accordingly, I have referred to science fiction. Social scientists have now begun using literature as a quarry for scholarly exercises; but they use only fossilized material, which has otherwise ceased to be living literature.

Let me explain the ideology behind science fiction and how I intend to use it.

A convenient starting point may well be Lord Snow's celebrated *The Two Cultures*. It may be remembered that he noticed in the very beginning of his lecture that social scientists may be in between the two cultures, between the artists and the scientists. But he felt they rather split into the two groups. In that lecture, and more so in its follow-up (they are published together), the two cultures are characterized by two qualities: tools and politics. The tools are divided into software and hardware; the politics into reactionary and progressive. The software are books and pictures and fantasy; they belong, says Snow, to the reactionaries; the hardware are experiments and field-work and mathematics and such; they belong, says Snow, to the progressives. All this is so pitifully Baconian – inductivist, and old-fashioned at that – that all one needs to do is draw the readers' attention to it.¹⁰

Snow himself notes in his second lecture, that his thesis really boils down to the claim that scientists are temperamentally progressive and that artists are not. He notes that Dostoevsky was a reactionary, though a humane and wise one: he really does not wish to put down the artists when he says that they are temperamentally reactionary. Now Snow also notices the existence of some progressive writers, whether novelists or poets; he does not in the least feel that this refutes his thesis – on the contrary, it only shows that artists can fight their temperament quite successfully, or perhaps even change it.

Snow's position puzzles me. It may indeed be the case that artists, especially novelists and poets, are reactionaries; perhaps the existence of an odd progressive writer is indeed neither here nor there. But by Snow's own light, the progressivist literature about science, as well as science fiction literature, should spell both progressivism and the hope for a better future.¹¹ It is the irony of history that the most reactionary and rearguard among the leading science fiction writers, Robert Heinlein, has endorsed Snow's view in order to conclude that science fiction is the only progressive and the only avant-garde literature today.¹² This indicates that we should try to state what is progressive in science fiction literature (it is almost never avant-garde, of course).

The most widespread aspiration (utopian or not)¹³ of progressive science fiction writers is to get rid of the common man: he is banished, discouraged, killed, or he improves.¹⁴ The common man today is forced to play robot, as many a stag or a cock is forced to play eunuch; and we do not need to robotize humans any longer. Science fiction, be it art, popular art, or mere trash, is moved, to a large

extent, by a dream, a dream of a free man who follows no leader and needs no servant.¹⁵ Such a man does not fear excessive creativity, and he does not feel obligated to take notice of every worthwhile creation either. Progressive science fiction rests on the maxim, live and let live; or rather, expand and let others expand: expansion is joy, learning is exploration, is adventure.

The need for drudgery is gone. The work ethic is still with us.¹⁶ It makes us feel guilty if we are not on top of things, up-to-date, *au courant*. And to keep on top of things we have to work so hard that we resent those who add to the stock of things to be on top of. Work ethic is understandably misanthropic and illiberal: it opposes the idea of letting others expand at their own pace, at their own risk.

The incredulous reader may ask, how the work ethic causes preventing others from expanding. The answer is involved. First, if you expand I have more work to learn what you have achieved, and so you better expand slowly or I will be unable to catch up with the explosion of knowledge. Second, work ethic leads to the inductivist theory that learning is the process of hard work and slow expansion: your exploration, then, is not a knowledge-expansion, since it is adventurous; it is fun, but also it is the neglect of your duty to spend long tiring hours on the bench, at the microscope and in the library.

Thus inductivism and work ethic cooperate,¹⁷ and their corollary is that you should write tedious notes rather than an adventure of ideas. I find this corollary objectionable and harmful. Let those who wish to write tedious notes, or not so tedious notes, do as they please; and let the adventurous minds do as they please too. For my part, I like intellectual adventure more than scholarly annotation. And I wish to point out that adventure is tolerated by free spirits and is opposed to in the name of scholarship by the timid. It is not enough to live in a world where human robots are redundant: we have also to combat the imposition of the thoroughness mentality which perpetuates the ethos of the human robot.¹⁸

The reader may, then, consider it paradoxical that the notes in this volume are a concession to the ethos which they combat. Be it so. The first two chapters include an attempt to expose the work ethic as the outcome of the inductivist fear of error, a fear that sounds like the basis of true science, and that, true to itself, presents man as a robot operated on by threats. In the third chapter I expose this fear as rooted in mythology, the mythology which polarizes truth and error, reality and appearance, nature and culture. In the last two chapters I expound the idea of a free adjustable man who freely interacts with his society. I thus expose the conflict between the individual and

society as a mythological polarization: not that the conflict does not exist, but that it cannot be the cornerstone of the philosophy of the social sciences as it hitherto was. Man is by nature, many would agree as a matter of course, a self-liberating social animal, a problem-solver and a conflict-resolver. Yet the self-same people who accept this as a matter of course also contradict it as a matter of course, at times by declaring that naturally man is born free, and at times by declaring that naturally man is born into society and so is irrevocably limited by it. This should be better noted: the theories that man is born free, or born into society, are parts of one myth, and one that conflicts with the view that man is, and should be, self-liberating.¹⁸

My deepest concern, then, is in self-liberation. And I wish to make the following the most central pragmatic corollary of my study. Regrettably, people who could succeed in this process but fail due to failure of their nerve, have the urge to discourage others – perhaps in order to prove the generality of the failure of nerve. People with any aspiration whatever, need to hear others' responses; they should, however, take care not to consult discouraging pedants. Moreover, we can alter institutionally the response pattern of such people and make them seek redemption by encouraging others. The world will be a much better place to live in if today's systematic discouragement for its own sake – in the world of learning or in the artistic world or anywhere else – will be curbed.¹⁹

Notes

1. The study of the function of excessive scholarship in the form of exegesis and pedantry can be found in the first chapter, on decidophobia, of Walter Kaufmann's delightful *Without Guilt and Justice, From Decidophobia to Autonomy*, Wyden, New York, 1973, where the annotation and references are presented in a particularly elegant and greatly commendable method.

2. It is no doubt the law of land that certain publications need documentation. Quite generally, and without entering any legal technicalities, in some countries the publication of information harmful to others – whether individuals or parties – must be accompanied by evidence of a certain sort. The law does not demand the evidence to be conclusive, but the testing of it to be of certain kinds and the results of the test sufficiently supportive by certain well-set criteria.

When I discuss the alleged need for support, I do not discuss matters covered by such laws. Rather, I discuss the fact that scientists and scholars can always publish information that they cannot support as mere conjectures. Yet the inductive code which many scientists endorse forbids the publishing of conjectures, especially on informative matters. And I am arguing against them.

3. Bacon's view that publicly committed intellectuals cannot change their views was confirmed by the bitter struggle the phlogistonists put against the anti-phlogistonists and refuted by the acceptance of the refutation of Newtonian optics in 1818. Even Laplace gracefully gave way though Davy reports it cost him dearly and changed his character.

The idea that the revolution in optics refuted Bacon belongs to William Whewell who responded to Sir John Herschel's dogmatic adherence to Baconianism. Yet a Baconian view of the revolution in optics was soon modified in a convincing manner, and the modified version was soon made popular, and vulgarized, I think, by S.P. Langley, Florian Cajori, Dampier-Whetham, Max Planck, and Thomas S. Kuhn. The neo-Baconian view is this: the publicly committed intellectuals do not change their views; they simply die out and leave the field to younger, less prejudiced minds. To this Martin Buber answered (*Israel and the World*, "Prejudices of Youth", p. 42) that young people's opinions are less well-founded than those of older people and so are more likely to be prejudices – in Bacon's own sense (though Buber does not discuss the nature of prejudice; which is a pity).

All this is discussed at some length in my *Towards An Historiography of Science, History and Theory*, Beiheft 2, 1963, facsimile reprint Wesleyan University Press, Middletown, Conn., 1967, sections 1 and 12 in particular, and my "Revolutions in Science, Occasional or Permanent?" *Organon*, Vol. 3, 1966, pp. 47–61. See also my "Science in Flux: Footnotes to Popper", in R.S. Cohen and M.W. Wartofsky, eds., *Boston Studies in the Philosophy of Science*, Reidel and Humanities, Dordrecht and New York, 1968, pp. 293–323, reprinted in my *Science in Flux*, Reidel, Dordrecht, 1975; my "Continuity and Discontinuity in the History of Science", *J. Hist. Ideas*, Vol. 34, 1973, pp. 609–626, especially section 1, The Radicalist View of Science and Its History; and my "Sir John Herschel's Philosophy of Success", in Russel McCormmach, ed., *Historical Studies in the Physical Sciences*, Vol. 1, University of Pennsylvania Press, 1969, pp. 1–36.

4. There are further, and very specialized, functions of a detailed annotation. The most delicate operation for the annotation of an avant-garde text is that of dispensing with the kind of pig-headed objections to the ideas an author develops which only high-powered and erudite scholars can have and usually do have. Such notes enable their author to display erudition and competence equal to theirs, so as to show they need not be ashamed to learn something from him in case he has something new to say. Thus, he can dispel the difficulties which only learned readers can have by discussing highly critically and in an erudite fashion some of their sacred cows – but this is effective only if the author can slaughter the cow in one single sure hit or blow or slash; he better check this with pig-headed scholars, and not lose his patience while doing so (easier said than done). Finally, in his notes an author can correlate earlier and later parts of his study, thereby not only offering further connections, but also an anticipation and thus a quick entry to his world of ideas and its ways, particularly by the impatient pig-headed scholar.

All this is considering the possibility of winning the pig-headed high-powered reader. Now this is not easily describable by any given rules since the pig-headed

high-powered reader is at times the doorman of the scientific elite club who has his own ideas about whom to approve of and why. Yet, just because he is the doorman, winning him may open many doors. I am speaking from personal experience, and all I can add is that these doormen can be real bastards who would just as easily close a door in your face because they compete, or think they compete, with you in some quite extra-intellectual domain. Also, these pig-headed high-powered bastards often read only notes and then blame the author for what they think he says in his text – especially when they have their own reasons, as mentioned. Now whenever a work is ambitious, especially ambitious in its very scholarship, it may well invite hostility for the very extra-intellectual quality of being too ambitious. – Note, here, that excessive scholarship is treated by the scholarly doorman as extra-scholarly; and quite rightly. – And when someone picks on you concerning a very subtle scholarly point, there will be no more than a handful of people the world over who will be willing and able to pronounce on such a point when it does not involve them in a quarrel, and perhaps none when it does. Hence annotation may, indeed, be a risky business. The best is to outlive these bastards (easier said than done) and see their private empires crumble or taken over by who knows whom.

5. Michael Polanyi, *Logic of Liberty*, London, 1951, recommends academic freedom, quite unqualified, including, p. 33, the freedom “to teach one’s subject in the light of one’s own opinion”. This freedom he denies quite explicitly in his *Knowing and Being*, University of Chicago Press, Chicago and London, 1969, p. 94, and recommends in addition censorship in the learned press, p. 93. He himself feels that all this can be misused, pp. 94–5. See also note 8 to this introduction.

A similar argument is used by Leszek Kolakowsky in defence of censorship at large, in his *Essays on Catholic Philosophy*, Warsaw, 1955, which I have been unable to see. Leopold Labedz quotes the salient passage on p. 73 of his “Leszek Kolakowsky: or, Ethics and Communism”, *Soviet Survey*, No. 23, January–March 1958, pp. 71–8:

“Just as every American gangster can practice his profession *ad lib.*, if only he has enough money to pay the judges and police, so every intellectual gangster can bamboozle his readers if only he has enough money to publish his products . . . In the socialist countries the scientist and philosopher must be responsible to the reader, and there is no freedom to cheat the people with the output of the morbid minds of any street charlatan . . .”

The sad fact is that in his *Logic of Liberty* and other early works Polanyi indefatigably argued against planned science, saying science must be free and for this it needs a free society. Now, it turns out, he thinks science should be planned, though by its own leadership rather than the leadership of the community at large. And for this, indeed, society at large needs to be democratic indeed. Kolakowsky does not ask, what happens when the leadership is in error, whom is it responsible to? Polanyi says, in his *Personal Knowledge* and elsewhere, that the acceptance of the authority of the leadership of science is voluntary. This is

seemingly liberal, except that, by definition, it makes the dissenters outsiders until and unless they win, and so it is liberal, if at all, only in a free society.

6. Sir Karl Popper goes a step further than Polanyi. He requires that the censorship be not only voluntary but also self-imposed. He wants the restrictions internalized, and by means of the Hippocratic oath, which he recommends that every scientist take (this recommendation was made by a few authors after Hiroshima). And so he writes on p. 52 of his “The Moral Responsibility of the Scientist”, *Encounter*, Vol. 32, No. 3, March 1967, pp. 52–7, that “the establishment of high standards to judge our work by, and the duty to constantly raise these standards by hard work, are indispensable”. Popper opposes censorship but requires that we accept the maxim *sagesse oblige* akin to *noblesse oblige*. Now what *noblesse* should *oblige* may be controversial, but less than what *sagesse* should. In particular, whether *sagesse*, just as much as whether *noblesse*, is an excuse for snobbism, standoffishness, elitism, or not, is surely debatable. I, for one, find the scientific elitism the worst kind, and find Popper’s exhortation a force pushing in that direction, no less objectionable than Polanyi’s. See also Popper’s “Autobiography”, sections 10 and 12, in P.A. Schilpp, *The Philosophy of Karl Popper*, La Salle, Ill., 1974.

7. Thomas S. Kuhn, “The Function of Dogma in Scientific Research”, in A.C. Crombie, ed., *Scientific Change*, London, 1963, pp. 347–369, and 386–395.

See in particular, on pp. 392–3 his use of Polanyi’s idea of tacit knowledge to justify his avoidance of terms like “intellectual framework”, “conceptual model”, “basic assumptions” and “methodological rules”; he identifies his “finger-exercise” or “practice-problem-solving” or even “paradigm” with Polanyi’s “tacit knowledge”. See also next note.

8. The view that diversity leads to anarchy is the most gripping of conservative ideas, especially in politics, especially in Communist countries. See my “Methodological Individualism”, *British J. Soc.*, Vol. 11, 1960, pp. 244–270, reprinted in John O’Neill, *Modes of Individualism and Collectivism*, Heinemann, London, 1973. See also my “Institutional Individualism”, in the same journal, Vol. 26, 1975, pp. 144–155.

Polanyi explicitly warns against diversity: “The continued pursuit of science would break down” he pronounces, *Knowing and Being*, University of Chicago Press, Chicago and London, 1969, p. 80, “if scientists came widely to disagree about the nature of things.”

The same holds for Thomas S. Kuhn, of course, who expresses the need for dogmatism in the essay cited in the previous note and in his contributions to I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge*, Cambridge, 1971. See also my review of that volume, “Tristram Shandy, Pierre Menard and all that”, in *Inquiry*, Vol. 14, 1971, pp. 152–164, where I discuss in detail the censorship within science. I should add here that the popularity of the views of Kuhn reflects the herd-like character of the world of learning, much in