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Industrialization and Image of the Sciences: The Interaction of Economic and Pure Theory

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Industrialization and Image of the Sciences:
The Interaction of Economic and Pure Theory.

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of
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A. Thomas Ferguson, Jr.

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The Political Science of Science

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The problem is to recognize which interest groups are exerting preponderant influence and for what purposes...Our theme, therefore, is the need to subject the gothic mysteries of science and technology to ordinary political analysis, commonsense political judgment, and plain English.
--H. S. Nieburg, In The Name of Science

Chapter I

Introduction:

The Political Science of Science

I. Plan of Work. Role of the Introduction.

The bulk of this paper is devoted to a lengthy explication, analysis, and harshly negative critique of the course of the notorious debate, not yet closed, on the role of falsification in the growth of scientific knowledge. While it is primarily addressed to those conversant with recent research in the area, the paper is written with a view toward intelligibility and lucidity, and is as self contained as possible. It could therefore plausibly be read as a separate document.

My aim in this introduction is to construct the framework -- and to do no more than that -- for the analysis of the historical success of the scientific enterprise from the vantage point of political theory. That is, I

propose to describe briefly, and by describing, expose, the unholy alliance struck between the dominant (and, incidentally, Liberal¹) theory of the nature of science and the received (Liberal) view of the nature of the proper relation between science and government.

I wish to suggest that these theories exist in a symbiotic relationship, that the epistemology is cognate with the politics, and vice versa, and that the falsification of the first compels the abandonment of the second. It would be giving away my conclusion, were I to remark that of course the main part of the paper is intended as an effort in this direction.

II. The Latent Politics of Contemporary Philosophy of Science.

The standard theory of science, in the diverse forms conferred upon it by Popper and by Kuhn, is systematically described in the paper, and its ideological functions are dealt with there. I don't want to anticipate that section, and so will confine myself here to a few global remarks.

Generally one can say that the two forms of the theory, however sensationally they may differ in other contexts, share the autonomy thesis, a view according to which the only legitimate criterion for the evaluation of a scientific conclusion is an internal one, that is, one emerging from the scientific process itself. This thesis has been

formulated by Popper as follows:

...there are pure scientific values and dis-values (Unwerte) and extra-scientific values and disvalues. And although it is impossible to wholly preserve scientific research from extra-scientific values and applications, still it is the task of scientific criticism and discussion to oppose the mixing of the two spheres, and especially to rigorously exclude extra-scientific values from influencing the discussion of the truth of an assertion (Wahrheitsfragen)²

and is presupposed in an extreme form in Kuhn's confident gloss that

My argument...goes even further, for it emphasizes that, unlike most disciplines, the responsibility for applying shared scientific values, must be left to the specialists group. It may not even be extended to all scientists, much less to all educated laymen, much less the mob.³

(Taking this statement of Kuhn's at its face value, philosophy of science, conceived as a specialists' scientific group, must be immune to any political criticism not accepted by the group itself. Assuming normal professional narrow-mindedness and disciplinary xenophobia, none will be made, and in due course the absence of any criticism will be cited as additional support for the now evident truth of the autonomy thesis.)

In both cases the autonomy postulate is targeted to deny the theoretical (as opposed to the merely accidental, or "historical") significance for science of extra-scientific "ideologies" (such as Marxism, Pragmatism, or for that matter, natural theology), economic change, or the

influence of politics generally. Moreover, in neither version is the theory at all interested in the unique individual actually doing science, although Kuhn exhibits an interest in groups. In this way both Popper and Kuhn can preserve the traditional, Liberal, insistence on the unitary character of science as a method(ology), and maintain a strict separation of scientific fact from practical (in the sense of the German "praktisch"), although obviously, in Kuhn's case, not from professional, values.

There are, however, real differences between Popper and Kuhn, which it would be exceedingly unwise to neglect. Popper, as will be seen shortly, employs a very old image of science, which is rather grossly incognate with the present industrial reality. Kuhn, who has taken the phenomenon of "puzzles" -- small problems the form of whose solutions are specified in advance -- far more seriously than anyone else, stands poised on the brink of the abyss which separates Liberal epistemology from a vastly more radical (and in present context, subversive) account of the nature of knowledge. Indeed his (alleged) subsequent retrenching from the position he outlined in the first, 1962 edition of his Structure of Scientific Revolutions may be viewed as the consequence of his realization of just where his theories were taking him.⁴

But what of Liberal political theory? What is the liberal view of the relation between science and government?

It is this topic to which I propose to devote the rest of this introduction, in the conviction that the political prescriptions of liberalism can best be elucidated in the light of their unarticulated epistemological presuppositions, and that the discussion of epistemology -- of philosophy of science, on the Liberal account -- can be advanced by a consideration of the topic's hidden political dimension.

III. The Latent Philosophy of Science of Contemporary Politics.

Price⁵ and Galbraith⁶ start from the present position of science -- taken to include both the natural and social sciences -- in American life. Their joint concern is to develop an account "not only of the practical relation of scientific institutions to the economy and the government, but also of the theoretical relation of science to political values..."⁷; they are, in other words, political theorists concerned with the relationship of science to the government.

Now there are some minor differences of emphasis and vocabulary between Price and Galbraith. Price concerns himself exclusively with the "scientific estate," while Galbraith elongates his unit to include other components of the knowledge industry, in particular, members of university faculties not belonging in the sciences. Moreover, Price's work on close examination discloses evidence of its

belated emergence from the anti-Communist chrysalis from which so much recent American political "thought" has emerged, and he exhibits throughout a far more naive and simple-minded perception of politics in an advanced industrial society than does Galbraith. However, Galbraith himself has conceded that

I have appropriated and somewhat altered the usage of my friend Professor Don K. Price...⁸
and I am much indebted to his valuable book...

and the similarities in assumptions, conclusions, and argument are so strong that I think one is justified in treating them as representatives of a single viewpoint, which is Liberal.

Their argument may be paraphrased as follows:

Power in advanced industrial society depends on control of the means of production, and science is increasingly the most significant component of the production process. Consequently, it is to the producers of scientific knowledge that political power now accrues. This means that the mantle of the classical, profit-maximizing capitalist has now fallen on the shoulders of the members of the educational and scientific estate, who are ensconced chiefly in universities and research institutes. These latter, being devoted to science, do not share the ruthlessness and prejudices of the antagon-

ists with whom they must still contend for political power, the personnel of the 500 largest corporations. This educational and scientific estate, fired with the eros of truth, and its free pursuit, have it within their compass to buffer the acid results of politics, insuring the hegemony of truth, good taste, and welfare capitalism.

Even a cursory examination will show that the view of science implied in this account includes the autonomy thesis as an essential element. This is not surprising, for earlier the autonomy principle had been extensively identified with Liberalism. Consider, in cameo, the following.

First, as Price incautiously lets slip,⁹ the argument presupposes a (simple-minded!) dichotomy of truth vs. power. Government is conceived as a realm of action and will, which may or may not bear any access to truth, either of morality or of science. The scientific realm, on the other hand, is imagined to be sealed off by a Chinese Wall of integrity from contamination by politics. Where politics is admitted to have stolen its way into the scientific sector, its intrusion is always viewed as pathological. On this account Lysenko is the perverse eponym for a whole universe of malificent possibilities.

So it comes to be imagined that research priorities, problem selection, problem solving strategies, and most of

all the transactions at conferences or in journals of the "invisible college" of active research workers somehow escape the effects of the societal medium in which they are enveloped -- a medium characterized above all by scarcity, compulsive competition, and the omnipresence of politics.

Secondly, the character of scientific fact is unreflectively held to be uniquely free of public value.

Price, for instance, remarks that

....scientific precision is purchased by an abstraction, and an exclusion of concern for purpose and value, that make it impossible to deal simultaneously with all the aspects of any concrete problem.¹⁰

and Galbraith often criticizes the received opinion of his discipline, not for embracing its nominal canons of neutrality, but for its failure to live up to them. Speaking of the mass refusal by academic economists to acknowledge the existence of industrial concentration and consequent market power, he remarks that

In denying scientific recognition or even legitimacy to this trend, economic theory was not being politically and socially neutral.....It was playing an active -- an actively conservative -- role in the political process.¹¹

And elsewhere he comments that

Economics has not been a science but a conservatively useful belief defending that belief as a science.¹²

The manifest assumption here is that a scientific theory of the proper credentials would not share these failings, but actually be neutral.

Finally, and related to the foregoing, neither Price nor Galbraith finds very much problematic about the entire scientific enterprise, however much individual speculations are excoriated. For them, the status of natural (and authentic social) science, in weal or woe, is invariably quo. The suggestion that one might want to examine scientific conclusions from the standpoint of one or another metaphysic, or suggest that historical counter-examples be enlisted to scrutinize the most fashionable contemporary theories instead of the other way around (eg., witches or religious mystics cited as evidence against some version of a reductionist psychology, rather than the employment of the psychology to eliminate the historicity of witches) -- all of this to Price and Galbraith, would appear preposterous. A fortiori, the suggestion that a social science research program should constitute itself around a political position.

Such are the implications of their position, a view which over the centuries has acquired a quality of obviousness not much inferior to the rotundity of the earth. But however persuasive it may appear on the first complacent glance, it cannot be stressed too often or too strongly that the entire argument hangs suspended by a single, slender thread -- one of a highly theoretical character. For it is the autonomy thesis alone that ties together the politics and the philosophy of modern science.

IV. Consequence of Malfeasance.

Although Price and Galbraith are read mainly by social scientists, an audience which overlaps but slightly with that of Popper and Kuhn, their accord with the explicit conclusions of contemporary philosophy of science has not been without malignity. The amiable, albeit unconscious agreement contributes to the specious aura of inevitability which the autonomy thesis currently possesses. Had either school developed a different viewpoint, some word of the disagreement would inevitably have leaked back to the other, perhaps providing a stimulus for review and self-criticism. Examples of such cross-fertilization are rare, but they have happened. Perhaps then the political significance of science in the modern world would not have been so long occluded, we would have been spared the mendacity and self-deception of twenty years of Research and Development rhetoric about the "challenge to national greatness," "because it is there," or the "pull of the unknown," etc., and we would sooner have come to suspect that, as is argued in the remainder of this paper, in capitalist society, truth is power's last and most insidious mask.

Chapter II:

Industrialization and Falsification

I. The Argument Summarized. Preliminary Remarks.

The most striking characteristic of academic thought is its innocence. What follows is an argument that this innocence grossly distorts current discussions of the role of falsification in the growth of scientific knowledge: Specifically, that the real causes of the irresolution and aimlessness of the dispute are the diverse images of the societal functions of science, comprising both descriptive and prescriptive components, held firmly, although not fully consciously, by Popper,¹³ Kuhn,¹⁴ and Feyerabend. These images are of varying ages; each was once functional, but only Feyerabend's comports the present situation in the political economy of knowledge. This fact is obscured by the disputants' habit of pulling their examples indiscriminately from Thales down to Niels Bohr and beyond. Moreover, once the ideological cataracts are removed, one can observe right under the unwitting noses of philosophers of science the first, faint signs of the withering away of natural science, a phenomenon to which Feyerabend's work contributes.

Throughout the paper I elide the distinction between science and technology. This is less from indolence than from philosophic conviction, which can here only be stated

and not justified. In my opinion, a distinction between the two might have made sense in only one historical epoch, viz., the one that just ended.¹⁵ That no statesman's rodomontade is bereft of a reference, however fleeting, to the sanguinary state of the nation's "science and technology" is the tribute moribund usage pays to relentless reality. Moreover, as is currently fashionable, I treat science as coterminous with knowledge. This is manifestly inadequate, and would be a good subject for a paper -- another one.

II. Contemporary Science Integrated Into the National Political Economy. A Sketch for Background.

Once the sciences played only an indirect role in the maintenance and expansion of the political economy of capitalism. The New Science of Physics was the natural scion of a New Science of Politics, and the old modes of the legitimation of political authority first shuddered, then crashed into ruins. The way was open for the bourgeois revolutions of the 17th, 18th, and 19th centuries.¹⁶ But in the New Industrial State,¹⁷ (or, less elegantly, the hegemony of Monopoly Capital¹⁸), science has another, grander role.

This is its elevation to what Eisner recognizes as the "critical factor of production,"¹⁹ on a par with the hoary triad of land, labor, and capital. This industrialization of science transforms both science²⁰ and industrialism²¹ out of recognition. The mutations of industrial-

ism have never lacked for interested commentators, but what are the consequences for science? This is Ravetz's question, and his answer, even when discounted for its preliminary formulation, is devastating.

First, the extravagantly honored image of science as the disinterested search for truth goes the way of the economists' perfect competition under monopoly capital. This image will not vanish into thin air -- it is much too useful to some interested parties for that -- but only philosophers of science will be fooled. Science is now a commodity; and is another -- merely another -- aspect of commerce. Like any other commodity, it is a victim of the craving for money, for profits, and for power; the object of pork-barrel politics, and skullduggery, that characterize "political capitalism."²²

No Karl Marx is needed to predict that such portentous changes in the forces of production will re-order the relations of production. And the loss of scientific independence is a second aspect of the new political economy of knowledge:

...This change is as radical as that which occurred in the productive economy when independent artisan producers were displaced by capital-intensive factory production employing hired labor... With his loss of independence, the scientist falls into one of three roles: either an employee, working under the control of a superior; or an individual outworker for investing agencies, existing on a succession of small grants; or he may be a contractor, managing a unit or an establishment which

produces research on a large scale by contract with agencies.²³

Thirdly, science becomes bureaucratized. This has effects on the personnel. Organization men people the organization, and Minerva's owl flies from 9 - 5. Far more malign, however, is the shift in the seat of quality control, for notwithstanding an occasional scandal, the National Science Foundation does not yet give money away:

The dispersal of large sums of money, and even more the decisions between competing demands, are matters which require proper procedures of information and control. A completely informal consensus of a large community is not sufficiently precise or reliable to be the basis for such work; and the investing agencies must work from the evaluations and judgements of a group of advisors. With this concentration of powers of decision and control, the free market place of scientific results, whose value is established after they are offered and by an informal consensus, is replaced by an oligopoly of investing agencies, whose prior decisions determine what will eventually come on to the market.²⁴

New forms of property spring up. Once "scientific property" was a paper in a recognized journal. Now a good personal contact to the source of the magic spigot will do, or alternatively, a high score of publication points will be equally salutary.

With the clamor for publication the literature becomes redefined and vulgarized. Journals proliferate, to the general satisfaction of the prestigious editor, satisfied author, and affluent publisher. Then the informal

channels dilate:

Through mailing-list distributions, scientists will circulate not only reprints, but also preprints, duplicated preliminary research reports, conference abstracts, and informal 'newsletters' of people, events, and results. These other types of publication are not merely supplements to the official channel of communication, making results available more quickly than can the printed journals. Rather, they are complementary to that channel; and their function is to provide publication of a sort which is not subject to the hazards, as well as the delays, of the scrutiny of referees. For the same sort of function is performed by raw collections of conference papers, published in hard covers and sold on the market, but appropriately called 'non-books'.²⁵

It would be a grotesque blunder to categorize all this commotion under "knowledge dissemination" or some such rubric. This is to miss its point, which is not to be read, but to have been written. Ravetz calls the phenomenon "shoddy science."

The path is clear to more refined, and lucrative, forms of corruption. One is "entrepreneurial science":

It is in this borderland...that we find some significant pathological phenomena. The first occurs when a contractor (individual or institutional) develops a really big enterprise, which is most likely to be on some mission-oriented research in a field where money is plentiful and not too many questions are asked. There then develops a research business, making its profit by the production of results in the fulfillment of contracts. The director of such an establishment is then truly an entrepreneur, who juggles with a portfolio of contracts, prospective, existing, extendable, renewable or convertible, from various offices in one or several agencies. The business is precarious, of course, for his only capital is in his

friendly contacts with those who decide on the allocation of funds. In such a research factory, conditions are not usually conducive to the slow, painstaking, and self-critical work which is necessary for the production of really good scientific results. Hence, much, most, or even all the work can be shoddy; but the entrepreneur does not operate in the traditional market of independent artisan producers who evaluate work by consensus. So long as he can keep his contacts happy, or at least believing that they personally have more to lose by exposing themselves through the cancellation or non-renewal of contracts than by allowing them to continue, his business will flourish.²⁶

Still another is christened by Ravetz "reckless science":

...a particular innovation may be recognized as risky from the technical point of view, dubious from the commercial point of view, of very slight use to anyone at all, even the State, and a potentially serious nuisance to the public and source of legal and political difficulties, and yet still receive enormous sums from the State... In calculating cost and benefit it ignores all those costs of a project for which it cannot legally be called to account: in particular, the degradation of the natural and human environment. Since the combined effect of the present and future technological developments is likely to be catastrophic, this rush onwards can truly be considered out of control.²⁷

And of course there is the terrifying realm of "dirty science", of nuclear bombs, gassed sheep, "unthinkable" thoughts, super-efficient napalm compounds, and ICBM's. This last type comprises "research projects whose intended application lies beyond the pale of civilized practice and morality."²⁸

Such is the reality of industrialized science. While it may partake of the humorlessness, and share the suffocating Weltanschauung characteristic of the central Post-Newtonian tradition in philosophy of science,²⁹ in its totality it presents a unique problem of comprehension. Throughout, it is conditioned by one massively novel relation: absorption into the circumambient political economy.

III. These Facts Not Yet Assimilated by Philosophy of Science.

Contemporary philosophy of science plays with these facts like a cat with a ball of yarn -- does everything except digest them. This I propose to demonstrate by presenting stylized sketches -- all that is necessary -- of the views on falsification in science held by Popper, Kuhn, and Feyerabend. These sketches are crafted with a view toward highlighting the problematic that I am interested in, and of course a much more extensive treatment would be necessary for a discussion with pretensions to exhaustiveness. This qualification holds throughout, and I will not repeat it in the paper. Now to the oldest image of science still in circulation, dating from a period of relatively undeveloped productive forces and a primitive division of labor, when philosophy and natural science were done by the same persons, that of Popper.

For this paper there is exactly one point that is absolutely crucial to understanding his philosophy of science.

It is that

...all criticism consists in attempted refutations.³⁰

For Popper, science -- whether physical or social -- starts with problems. Not data, sensations, 'hard' facts, or any species of observation. Theories are advanced, in the form of deductive systems, as possible solutions to these problems. They are then tested by attempts to find a refuting instance: all planets in the solar system are conjectured to possess orbits tilting no more than seven degrees from the plane of the earth's orbit; we look, not for the eight which do, but for the one that doesn't. Although we are naturally pleased should our theory be corroborated, Nature is not asked by the scientist to say "yes", only "no". Theories with a refuting instance are junked. If the theory resists falsification, it may be accepted pro tempore, and subjected to still more tests. It is overwhelmingly important to understand that science progresses only by falsification: there is no "positive", accumulated evidence which counts when the epistemological chips go down. More emphatically, science does not produce "probable" theories, nor are facts irrefragably "established". At any moment, the most highly regarded theory may be overthrown by a single refutation, touching off a "scientific revolution". Such is the Popperian Logic of Scientific Discovery, the "scientia negativa" (Agassi).

This Popperian theory, however, suffers from one ex-

cruciating drawback: it is incognate with reality. Inspection of the available scientific laboratories failed to turn up significant evidence of any refutations in progress. The scientists, after all, were not consulted in advance, and perhaps could not be expected to live up to the extrinsic norms prescribed for them. There was an empirical chink in the theory's logical armor, and it was here that the opposition aimed their blows:

...I should like to raise before them [the Popperians] the spectre of the history of spectroscopy between 1870 and 1900. I think it fair to describe this period as one of mapping, in which the spectra of the elements were described with ever increasing precision. There is precious little 'refutation' going on here, yet it would be hard to deny Angstrom the title of scientist.³¹

That there is normal science...is the outstanding, the crashingly obvious fact which confronts and hits any philosophers of science who set out, in a practical or technological manner, to do any actual scientific research. ...real science (basic research, applied, technological, are all alike here)...is normally a habit-governed, puzzle-solving activity, not a fundamentally upheaving or falsifying activity...³²

With Kuhn's arrival, the doubts snowballed, and the opposition grew even more strident. He wrote, referring to the alleged falsification experiences, "Indeed, I doubt that the latter exist."³³ For him, science consists, not of the single-minded pursuit of refutations, but of a succession of periods of "normal science," interrupted by revolutions. During normal science, research proceeds inside the circum-

vallatory boundaries of a well defined community of investigators, with a concrete solution to some past problem embraced as a "paradigm" for the solution of the remaining difficulties. This paradigm-bound activity, what most scientists do, most of the time, is "puzzle solving," and is not at all directed toward falsification, but instead presupposes the truth of the underlying communal view of nature. Eventually some problems resist solution in this vein; the anomalies become major embarrassments. There is much distress in the profession -- even respectable men are failing -- and a crisis usually results. At this point a solution to the refractory problem(s) is brought forward which is not modelled on the old paradigm. Arguments that "cannot be cast in a form that fully resembles logical or mathematical proof"³⁴ ensue. In the end the community as a whole comes round to the new solution, the revolution triumphs, and a new paradigm reigns, whence another cycle begins, like the endless palace revolts of a banana republic.

The question inevitably surfaces as to how Popper ever mired himself so intractably in this quicksand of hortatory assertion. Here two questions must be distinguished. The first is how he got into it. The second is why he didn't get out of it.

The initial error probably had its roots in the special characteristics of Viennese culture during and after

World War I. There language (or rather, Language) had just replaced the recently deceased and plangently elegized deity as the authoritative source for last judgments. It has been written that "For Karl Kraus the word has personal life. Language, passion and thought are one and the same for him. Language is the name of the activity of his passionate thinking; his passion and thought are identical with their articulation,"³⁵ but he was only the first magnitude star of a large and glittering galaxy, whose lesser luminaries included Hofmannsthal and Wittgenstein himself. Popper kept a low profile for the duration of this linguistic Verklärte Nacht, but did not entirely escape its influences.³⁶ Confronted by the diverse claims to scientific status of Marxism, Relativity, and the psychologies of Adler and Freud, he hoped to find the guiding thread out of this labyrinth supplied by language, even as he realized that language only marked out a path made by other agents. Initially posing his problem not as a choice between different kinds of activities, or types of commitments, but as the "problem of drawing a line (as well as this can be done) between the statements, or systems of statements, of the empirical sciences, and all other statements -- whether they are of a religious or of a metaphysical character, or simply pseudo-scientific,"³⁷ Popper proposed his falsification criterion as the solution to this "problem of demarcation," as he still refers to it.³⁸

In this view, statements possessing scientific character were those deemed "capable of conflicting with possible, or conceivable, observations."³⁹ All others, while they might well be meaningful, belonged to some other realm, and did not pertain to science, save as a source of conjectures. And although Popper strenuously opposed the subsequent search by the Wiener Kreis for a criterion of a statement's meaning, and insisted that the question of linguistic meaning was of exiguous import, still the first fatal step off into the clouds had been taken. For much is meaningful -- as Popper knew well -- that is not scientific. A fortiori, however, there is much that can be the subject of some abstractly possible test which is of no scientific interest whatsoever; questions of language have an incomparably vaster scope than those of science. The concrete, active character of the political economy of knowledge got lost in a philosophical dispute over demarcation. This reluctance to recognize the diverse interests of philosophy and actual science colors all of Popper's subsequent oeuvre, indeed, as Masterman remarks:

At the earliest opportunity...Popper leaves discussion of scientific theories altogether to turn to philosophic theories, in order to analyze, brilliantly, whether these are not also, in a more direct way, refutable. He then, bar a hairsbreadth, equates these with scientific theories; and one suspects that ...it is these, and not science as it really is, which he has had at the back of his mind all the time.⁴⁰

The result was that Popper, the philosopher of Kantian filiation, who was interested in the condition of all possible experience, but who also averred that "I... have no personal interest in matters of commerce, or in commercially minded people,"⁴¹ ended up claiming in effect to find his theory confirmed in the relentless pursuit by entrepreneurial science of the conditions of all possible profitable experience.

Now Popper's theory of science was not conceived in a vacuum, and this is the key to its indurate success. For Popper, philosophical problems always came in from the cold, i.e., originated outside of philosophy.⁴² Some were suggested by the natural sciences, art spun off others, and the social sciences were considered to provide still more. Not the least of these wellsprings of philosophy was politics, and Popper could scarcely have been less veiled about the political implications of his theories. Relishing his starring role in the weighty drama whose plot -- the 2000 year old struggle to entrench the "Open Society" -- he outlined in The Poverty of Historicism and The Open Society And Its Enemies,⁴³ Popper promoted his generalized philosophy of science, of "Critical Rationalism," as the exclusively suitable ethos of that improbably porous community, with which current, Western, more-or-less (often, notably, less) Liberal states were extensively identified:

...my optimism lies entirely in my inter-

made the other look good, and together they were mutually conducive to the enlistment of unified belief. Popperians routinely identified epistemological dissent with incipient totalitarianism,⁴⁹ and the good Critical Rationalist, like Thomas Mann's liberal artist, was a bohemian only in the realm of ideas; for revolution in thought, evolution -- "piecemeal" evolution -- in politics.⁵⁰ One cannot, however, lightly abandon positions whose wreckage implies, not merely the defeat of a school, but the Decline of the West. Rather, what in other circumstance would be regarded as arrant pertinacity presents itself as of transcendent obligation. As a consequence, the theory had precious little room for maneuvering, when its fundamental thesis -- that science progresses by falsification -- came into question.

Kuhn, for his part, has professed that "Whatever scientific progress may be, we must account for it by examining the nature of the scientific group, discovering what it values, what it tolerates, and what it disdains."⁵¹ But he is no more willing than Popper to admit that among the constitutive community values might be those of power and money; that for instance, American physicists have for years forsworn theoretical research in favor of more remunerative experimental investigations, even though the theoretical results, when there are any, possess demonstrably greater value.⁵² His critique updates the image of what scientists actually do, to accord more with the institutional reality,

but never leaves the established realm of liberal legitimacy. In a word, his image is of a later vintage than Popper's, but still elides the brutal realities of the political economy of knowledge. This is why he is shocked, like Cardinal Bellarmine confronted by Galileo, at the debilitating thrust of Feyerabend's criticism:

...to describe the argument of Feyerabend's as a defense of irrationality in science seems to me not only absurd, but vaguely obscene.⁵³

And why he must try, in a classic statement of one-dimensional rationality, to render reason virtually coextensive with reflection on science:

To suppose...that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land.⁵⁴

So there Kuhn stands, dismally, able to do no other.

IV. Feyerabend Abandons The Puzzles.

But Reason is cunning. It was a quondam Popperian who brought to consciousness the suppressed interest in emancipation latent in Popper's philosophy of science. It was Feyerabend who, in the Republic of Science, assumed the role of Lenin to Popper's Kerensky.

Feyerabend had earlier showed in a classic series of papers, directed mainly at Nagel, that the presence of competing theories was a precondition for falsification, and consequently for progress in science. He thus redirected the

interest in research away from the Popperian stress on the use of logic as the "Organon of Criticism,"⁵⁵ and back to the actual development of science. He developed a view of perception which, while it had unmistakable Popperian antecedents, was novel in the stress placed on the ineliminable ideational elements of all observations; that they were always "theory-laden." Extending this insight through a highly original analysis of the case of Galileo,⁵⁶ he showed that what counts as "observational" has an "historical character;" that it depends quite often on old and senile theories, scarcely consciously held at all (assumptions about the medium in which observations are made, expectations of what is to be observed, etc.). Moreover, one can criticize these theories only on the basis of other observations, themselves equally theoretical. A correct general theory may thus be rejected, not because it is itself deficient, but solely because the wrong "observational" theories are providing the evidence which is used against it; that in other words, "observational" theories can develop out of phase with more general ones.

In such a situation, where the "evidence" is overwhelmingly against a new theory, the recreant are likely to despair and kick away their revolution. The alternative, Feyerabend argued, is to admit any and all theories, and let their proponents concoct the "observational" facts their theories need to acquire plausibility. Normal science flies

in the face of such a reversal of the customary relation of theory and practice, and so draws Feyerabend's ire.

First he points to the monolithic character of normal science, its disquieting likeness to an intellectual Brave New World:

The recipe, according to these people, is to restrict criticism, to reduce the number of theories to one... Students must be prevented from speculating along different lines and more restless colleagues must be made to conform and 'to do serious work'. Is this what Kuhn wants to achieve?⁵⁷

Then he shows that puzzle-solving as a criterion for normal science's existence comports organized crime, or even Oxford philosophy, just as well. This is because, he charges, Kuhn "has failed to discuss the aim of science."⁵⁸

Now the aim of normal science, even for Kuhn, is to create the conditions for scientific revolution. But Feyerabend considers Kuhn to err in prescribing a puzzle-solving effort, "the most boring and most pedestrian part of the scientific enterprise,"⁵⁹ as the optimal strategy. Instead proliferation of theories must precede a revolution. And such progress as there is in science is due to the transformations in the quotidian, "less humanitarian"⁶⁰ normal science 'component', forced by prior theoretical proliferation in the often invisible, but omnipresent 'philosophical component.'

With the cat this far out of the bag, Feyerabend plunges forward, con brio. Most scientists, he asserts,

resist changes in the philosophical component; they are often not even aware there is one.⁶¹ Contemporary scientific education

has the purpose of carrying out a rationalistic simplification of the process 'science' by simplifying its participants. One proceeds as follows. First, a domain of research is defined. Next, the domain is separated from the remainder of history (physics, for example, is separated from metaphysics and from theology) and receives a 'logic' of its own. A thorough training in such a logic then conditions those working in the domain so that they may not unwittingly disturb the purity (read: the sterility) that has already been achieved. An essential part of the training is the inhibitions of intuitions that might lead to a blurring of boundaries. A person's religion, for example, or his metaphysics, or his sense of humor must not have the slightest connection with his scientific activity. His imagination is restrained and even his language will cease to be his own.⁶²

So why would anyone want to be "truly scientific (dreaded words!)"⁶³ at all? Why indeed? Unless the answer is "brainwashing"⁶⁴ or the machinations of "Stalinists, in politics as well as in the philosophy of science."⁶⁵ interested in preserving "their basic myth."⁶⁶

Feyerabend sees but one hope for amelioration: we must abandon the "normal" practice of science for proliferation -- "revival of astrology, witchcraft, magic, alchemy, elaboration of Leibniz's Monadology, and so on,"⁶⁷ always remembering that the "sciences, after all, are our own creation, including all the severe standards they seem to impose on us,"⁶⁸ and that "the choice of a basic cosmology may become a matter of taste."⁶⁹ •

Kuhn bristles at this suggestion that normal science be abandoned. He objects that depriving science of its normal science puzzles will destroy the progressive character of science:

By their nature revolutions cannot be the whole of science; something different must necessarily go on in between.⁷⁰

In the developed sciences, unlike philosophy, it is technical puzzles that provide the usual occasion and often the concrete materials for revolution.⁷¹

Only after it [the transition to normal science] occurs does progress become an obvious characteristic of a field.⁷²

Feyerabend answers by reeling off an example of progress without puzzles:

This objection is refuted by the Presocratics who progressed (their theories did not just change, they were also improved) without paying the slightest attention to puzzles. Of course, they did not produce the pattern: normal science -- revolution -- normal science -- revolution, etc., in which professional stupidity is periodically replaced by philosophical outbursts only to return again at a 'higher level'.⁷³

There is, however, something most curious about this proposed model. In no way is it analogous to industrial science, and it does not appear at all compatible with any of the enterprises sketched in section two of this paper. Those conversant with the theories of Marx or the story of Midas, will have no difficulties imagining why; for the rest, even a nominal perspicacity should suffice for the detection of the heavy, if invisible, hand of the political economy of

knowledge at work. For puzzle-solving is supported, problem-solving is not. I. B. M. now smugly advertises that it has philosophers on its payroll, but the effectiveness of the pitch depends on its shock value. There can be knowledge, scholarship, academic rank and tenure, perhaps even progress of a sort in efforts such as that of the Pre-Socratics, but nothing at all resembling contemporary science. Why doesn't Feyerabend see this?

Here an unsettling question insinuates itself. Perhaps Feyerabend does grasp the facts of this integration. Possible, he doesn't care. Slowly the question begins to come into focus: Are Kuhn and Feyerabend interested in the same sorts of things?

I am certain that the answer is no, that Feyerabend's explicit raising of the normative issue portends something far more drastic than another theory of the nature of science. The easiest way to demonstrate this is to inquire into what Kuhn is interested in. The answer is: modern science. Now, what is modern science interested in? The answer to this question is adumbrated by Habermas:

The modern sciences produce...knowledge whose form (and not subjective intention) is that of technically applicable knowledge, although the application usually comes only much later.⁷⁴

Working in a different tradition, Habermas fills out the scheme implied in the above formulation in his essay "Erkenntnis und Interesse":

Three categories of investigation-processes allow the ascertainment of specific connections of logico-methodological rules and knowledge-constitutive interests...In the formulation of the empirical-analytic sciences a technical interest is incorporated.⁷⁵

It would be hopeless, and, felicitously, unnecessary to explicate the whole of Habermas's position in this paper.

Nevertheless, since his discussion of science is the only one current which manifests explicit awareness of the political economy of knowledge, a few key passages may usefully be inserted, for they throw Feyerabend's arguments into sharp relief:

In the empirical-analytic sciences, the system of reference, which prejudices the meaning of possible statements, specifies rules for both the construction and the testing of theories. Theories are hypothetic-deductive systems of statements, which allow the derivation of empirically meaningful law-hypotheses. These are interpreted as statements of covariance between observable events, and allow predictions according to postulated initial conditions. Empirical-analytic knowledge therefore is possible predictive knowledge. The meaning of such predictions, i.e., their technical applicability, only results from the rules according to which we apply the theory to reality...We can say that facts and the relations between them can be grasped "descriptively", but this mode of speech must not be allowed to conceal the fact that the relevant scientific facts constitute themselves only through a preceding organization of our experience in the functional system of instrumental action...empirical scientific theories disclose reality under the guiding interest of possible consolidation and expansion of information-controlled action.⁷⁶

Habermas himself pointed to the differences between Popper and himself, exhibiting a proper -- and wholly justifi-

fied -- confidence that where his theory diverged from Critical Rationalism, it did so in the direction of greater verisimilitude to the actual conduct of scientific research; he could account for the puzzles. But the potential critical dimension in Popperianism eluded him. Feyerabend found it, hidden in the emphasis on falsification. Falsification is not a technically oriented activity at all, not by itself. Discovering what a thing is not affords no automatic amplification of existing manipulative skills, although it is trivially true that any positive assertion can be expressed as a double negation. This is why the National Science Foundation finances only historical research on the Pre-Socratics, and it is the gravamen of Feyerabend's charge to Kuhn. Expressed in Habermas's terminology, Feyerabend is proposing a reversal of the "knowledge-constitutive interest" characteristic of science since the Renaissance. Only on this level does the argument finally cohere.

It is thus the intent of Feyerabend's argument to deprive puzzles of their function. Without the puzzles, however, industrialized science grinds to a halt. From the viewpoint of industrialized science, this might be considered malefic. Such is the charge brought against Feyerabend.

In a brief note published a few years ago, Feyerabend suggested that two paradoxes of induction, the celebrated Paradox of the Ravens, and the equally notorious Goodman conundrum, the so-called "new Riddle of Induction," could be

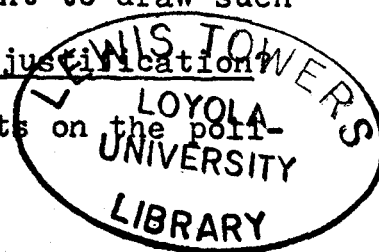
evaded by considering only negative, i.e., falsifying, instances to count in theory evaluation. Since these problems originate in justificational (i.e., problems of positive evidence) perplexities, they would both be emulsified in the scientia negativa. Subsequently he has suggested, not altogether implausibly, that excessive interest in such questions is pathological.⁷⁷ He concluded by imploring philosophers of science

to terminate the discussion of such red herrings as these and to concentrate upon the more rewarding task of advancing knowledge by the criticism of old hypotheses and the suggestion of new ones.⁷⁸

T. W. Settle, in a finely written reply, objected that

Feyerabend is right in his discussion of scientific method, but not in his discussion of induction. Contrary to most writers in the philosophy of science I do not think that knowledge grows by induction. But contrary to the usual opponents of inductivism I do not think induction can be dismissed, simply because it lacks this role in science.⁷⁹

Settle is here walling off science from the rest of human experience. He postulates that the procedures involved in science are of a radically different kind from any other variety of human knowledge. These other fields grow positively, by some sort of mysterious inductive process which eludes the Popperian reduction of induction to hopeful conjecture, which is then taken as a basis for action on a wing and a prayer.⁸⁰ But why does he want to draw such a distinction, why is he so obsessed with justification? The answer is no surprise, when one reflects on the poli-



tical economy of knowledge. Settle sharply distinguishes the quasi-philosophical "science" from technology, protesting that

...the role which confirmation plays in technology is ignored by Feyerabend...⁸¹

and then boils down his objection to the charge that acceptance of Feyerabend's proposal would be bad for business:

confirmation is indispensable in such more or less intellectual human pursuits as technology, banking, child-rearing, jurisprudence, insurance, politics, and industry.⁸²

Shoddy science, entrepreneurial science, reckless science, and dirty science could ask for nothing more, right down to the invocation of Mother ("child rearing"). The critical dimension drops from sight, and Hegel's Weltgeist strides across the pages of the British Journal for the Philosophy of Science -- "der Gang Gottes in der Welt" -- as Settle proposes to make institutional practices (e.g., entrepreneurial science) self-justifying:

Solutions to the problems of induction are likely to be provided by institutional, even legal, standards of what is to be counted as sufficient positive evidence...⁸³

V. The Withering Away of Natural Science.

So the battle lines are drawn. Industrialized science -- the "alienation of reason"⁸⁴ -- or "revival of astrology, witchcraft, magic, alchemy, elaboration of Leibniz's Monadology, and so on."⁸⁵ What can be said about the probable outcome?

In the short run, the implications are clear. The admixture of a little vulgar Marxism is wonderfully conducive to reliable vaticination. What the Gesellschaft wants, the Gesellschaft will get. The New Industrial State is not imaginable, save with its characteristic middle adjective. But Ravetz cautions that although it "might seem far-fetched and alarmist to claim that science is in danger of decline and dissolution, through its inability to make a healthy response to its new conditions," still, "the history of natural science in Europe shows that its steady growth over the past centuries has been an aggregate of cycles of growth and decline in different fields and places...it would be astonishing if it were not so, for then natural science would be the only sort of creative work exempt from such rhythm,"⁸⁶ and the more remote future poses some interesting questions.

Here the answer, I think, must be very much more tentative. Ravetz hints in worried tones of a decline in "morale",⁸⁷ manifested in "certain recognizable symptoms rather than hard cases."⁸⁸ Then there is the obdurate "disappointment in the recruitment to science, measured by the number of students electing to study science at university and also continuing into research,"⁸⁹ as well as a "decline in the quality of science teaching in the schools."⁹⁰ These are portents of some gravity, and not unexpected. It stands to reason that the mantle of explorer of nature was more

appealing than a grey flannel suit. Far more significant than any of these, however, may be the long run change in the intellectual climate.

The Zeitgeist is restless. One can observe in the developed countries -- the only kind this discussion has any reference to -- an exfoliant feeling of disillusionment with science and its attendant rationality, a disquieting intuition that the blossoming of science and technology was really the sprouting of Les Fleurs du Mal. In this movement, Paul Feyerabend's off-handed remark that

...we may construct a world in which it [science] plays no role whatever (such a world, I venture to suggest, would be more pleasant than the world we live in today).⁹¹

will bulk ever larger. Ravetz hopes for a "critical science" with "the relation of the scientist to the external world" being "fundamentally different."⁹² Perhaps a short term movement into an environmentally conscious Eco-biology might come to resemble such a science. I suspect, however, that this is too sanguine; that the withering away of natural science may already have begun.

FOOTNOTES

¹Liberalism is a pie which many philosophers enjoy cutting up; no single definition of the term is likely to conform to all the popular usages. For this paper "Liberalism" may be understood as the polite equivalent of the rather more unfashionable expression "bourgeois thought," that is, the thought forms the predominance of which attended and results from the rise of the modern industrial state. For more extended discussions and invaluable theoretical background cf. Karl Marx, "Vorwort," in his Zur Kritik der Politischen Oekonomie (Berlin: Duncker, 1859), and Karl Mannheim, Ideology and Utopia (New York: Harcourt, Brace & World, 1936), especially pp. 122-23. My interest in stipulating this definition is only to satisfy the following adequacy conditions for a more theoretical definition: 1) That Liberalism be considered historically, with no one particular epistemological or political view held to be the essence of Liberalism; 2) That the reformist posture of Liberalism be clearly recognized; 3) Most of all, that no one be unduly exercised when it is discovered that not every "Liberal" thinker subscribed to all the tenets which might be included in a Liberal catechism; figures such as Bacon present especially taxing cases here.

Of course, it is taken for granted that Keynesianism and Liberalism are now virtually co-terminus, and accordingly no argument is offered for the implied suggestion that Galbraith and Price are paradigmatic Liberals of the present day.

²Karl Popper, "Die Logik der Sozialwissenschaften," in Th. W. Adorno, ed., Der Positivismusstreit in der deutschen Soziologie (Frankfurt: Luchterhand, 1971), p. 114. Unless otherwise indicated, all the translations from the German in this paper were done by me.

³T. S. Kuhn, "Reflections on my Critics," in Lakatos and Musgrave, eds., Criticism and the Growth of Knowledge (Cambridge: Cambridge University Press, 1970), p. 263.

⁴For a sketch of the differences, whose import may be variously assessed, cf. Alan Musgrave, "Kuhn's Second Thoughts," in the British Journal for the Philosophy of Science, XXII, No. 3 (1971), pp. 287-97.

⁵Don K. Price, The Scientific Estate (Oxford Paperback; Oxford: Oxford University Press, 1968).

⁶John Kenneth Galbraith, The New Industrial State (2nd edition, revised; Boston: Houghton Mifflin, 1971).

⁷Price, Scientific Estate, p. 19.

⁸Galbraith, Industrial State, p. 284n.

⁹Cf. his Chapter 5, "The Spectrum from Truth to Power."

¹⁰Price, Scientific Estate, p. 111.

¹¹John Kenneth Galbraith, "Economics as a System of Belief," in his Economics, Peace, and Laughter (New York: New American Library, 1972), pp. 57-58.

¹²Ibid., p. 59.

¹³The Popper corpus is scattered through forty years of books and journals, in several languages. I use in particular "Die Logik der Sozialwissenschaften," in Positivismusstreit, pp. 103-23; this is Popper's own summary of his position. For comments on the paper cf. David Frisby's "The Popper-Adorno Controversy: The Methodological Dispute in German Sociology," in Philosophy of the Social Sciences, II, No. 2 (1972), pp. 105-19.

I have aimed for the minimal degree of complication commensurate with precision of argument, consequently this paper does not consider recent developments by Imre Lakatos of what is essentially a Popperian position. Cf. inter alia, his "Falsification and the Methodology of Scientific Research Programmes," in Growth of Knowledge, pp. 91-195. Neither does this paper attempt a detailed criticism of Feyerabend's methodological anarchism. However, it is worth noting that the residual Liberalism which his recent papers exude is not nearly as far removed from Popperianism as Feyerabend seems to think. In this connection Noretta Koertge's "For and Against Method," British Journal for the Philosophy of Science, XXIII, No. 3 (1972), pp. 274-89, missed the most important point, although it is heartening to observe the abandonment by the British Journal for the Philosophy of Science of its fond fantasy that it could masquerade as a journal of the philosophy of science, without simultaneously being a journal of politics, of anthropology, of economics, and of sociology.

¹⁴Kuhn has several times reformulated his position. Some have sensed tergiversation, e.g., S. E. Toulmin, "Does the Distinction between Normal and Revolutionary Science Hold Water?", in Growth of Knowledge, pp. 39-47. I use the revised edition of The Structure of Scientific Revolutions (2nd revised edition; Chicago: University of Chicago Press, 1970), with special attention to the "Postscript -- 1969".

¹⁵On the probable indistinguishability of the two,

cf. H. L. Nieburg, In the Name of Science (revised edition; Chicago: Quadrangle, 1970), pp. 110-14; his book is a pioneering study of the State and science, lamentably generally unknown among philosophers. A secondary aim of this paper is to make a start toward bringing together the literature in economics and the social sciences on science, and confronting it with the central tradition in philosophy of science, so that the criticism (and not "methodology") of science can be carried through as a critique of political economy in a very elongated sense.

¹⁶Cf. Jürgen Habermas, "Technik und Wissenschaft als Ideologie," in Technik und Wissenschaft als Ideologie (Frankfurt: Surkamp, 1968), pp. 72-73.

¹⁷Cf. Galbraith's Industrial State, but see also Ralph Miliband's "Professor Galbraith and American Capitalism," in Economics, ed., David Mermelstein (New York: Random House, 1970), pp. 531-42.

¹⁸Paul Baran and Paul Sweezy, Monopoly Capital (New York: Monthly Review Press, 1966).

¹⁹Robert Eisner, Review of New Industrial State, in Quarterly Review of Economics and Business, VII, No. 3 (1967), p. 83. Here I employ the traditional language of "factors" of production without prejudice to the issue of its long run utility for economic analysis; cf. Ernest Mandel, The Formation of the Economic Thought of Karl Marx (New York: Monthly Review Press, 1971), pp. 92-93, for a vigorous argument.

²⁰The most illuminating work on this subject, but recently published, is Jerome Ravetz, Scientific Knowledge and its Social Problems (Oxford: Clarendon Press, 1971), whose analysis provides a detailed background for that undertaken here.

²¹Cf. supra, Notes 3, 5 and 6.

²²For the term, and a sketch of the nascent theory, cf. Gabriel Kolko, The Triumph of Conservatism (Quadrangle edition; Chicago: Quadrangle, 1967). The theory varies distinctly in its emphasis on some topics from the treatments referred to in Notes 5 and 6, and is rather similar to that of Habermas, cf. supra, Note 4.

²³Ravetz, Knowledge and Social Problems, p. 44.

²⁴Ibid., p. 45.

²⁵Ibid., p. 48.

²⁶Ibid., pp. 55-56.

²⁷Ibid., pp. 54-55.

²⁸Ibid., p. 57.

²⁹Cf. Paul K. Feyerabend, "Classical Empiricism," in R. Butts, ed., The Methodological Heritage of Newton (Toronto: University of Toronto Press, 1970), pp. 150-170.

³⁰Popper, "Logik," p. 106. Those addicted to logomachy may debate whether this translation catches all the nuances of "...alle Kritik besteht in Widerlegungsversuchen."

³¹L. Pearce Williams, "Normal Science, Scientific Revolutions and the History of Science," in Growth of Knowledge, p. 50.

³²Margaret Masterman, "The Nature of a Paradigm," in Growth of Knowledge, p. 60.

³³Kuhn, Structure of Revolutions, p. 146.

³⁴Ibid., p. 199.

³⁵Erich Heller, "Karl Kraus," in his The Disinherited Mind (New York: World Publishing Company, 1965), p. 239. On Vienna, science and philosophy in twenties, cf. also William Johnston, The Austrian Mind (Berkeley: University of California Press, 1972), especially Chapter 13 (and p. 401 for Popper); "Kraus, Loos, and Wittgenstein," in Letters from Ludwig Wittgenstein, by Paul Engelmann, trans. L. Furtmüller (Oxford: Blackwell, 1967), pp. 122-32.

³⁶In what follows on Popper, I make heavy use of his own account of the genesis of his theories in "Science: Conjectures and Refutations," in his Conjectures and Refutations (Harper edition; New York: Harper & Row, 1968), pp. 33-65.

³⁷Ibid., p. 39. Given this crystal-clear formulation of his views, and the equally limpid reiteration of them in "The Demarcation Between Science and Metaphysics," in Conjectures and Refutations, p. 257 and p. 267, one is hard pressed to understand John Losee's apparent avowal that Popper concerned himself with the tension between scientific method and illicit "conventionalism", to the exclusion of concern with statements themselves. Actually, he was concerned with both. Cf. Losee's A Historical Introduction to the Philosophy of Science (Oxford: Oxford University Press, 1972), p. 190. This is not to affirm that Popper was inter-

ested in meaning; he was not.

Partly because he does not formulate his theories of science in the consuetudinary linguistic manner, Polany, who is interested in the kinds of commitments scientists make, is regularly traduced by his opponents, who don't understand him. Cf., for instance, Lakatos, "Methodology," in Growth of Knowledge, pp. 92-93.

³⁸Cf. his "The Demarcation Between Science and Metaphysics," in Conjectures and Refutations, pp. 253-292.

³⁹Popper, "Science," p. 39.

⁴⁰Masterman, "Paradigm," p. 72.

⁴¹Popper, The Open Society and Its Enemies, Vol. I: The Spell of Plato (4th revised edition; New York: Harper & Row, 1963), p. 295. The quotation continues as follows: "But the influence of commercial initiative seems to me rather important." The key word here is "initiative"; commerce is conceived as wholly external to the real product of science; Ravetz, of course, does nicely as a refutation of this view. Popper's attitude is manifested in his "In my opinion, the 'economic conditions' or the 'social relations' of science are themes which can easily be overdone, and which are likely to degenerate into platitude;" Open Society, Vol. II: The High Tide of Prophecy: Hegel, Marx, and the Aftermath, p. 107. Of course, if the link with epistemology is broken, platitudes are the likely result.

⁴²Popper, "The Nature of Philosophical Problems and Their Roots in Science," in Conjectures and Refutations, p. 72.

⁴³The Poverty of Historicism (3rd edition; New York: Harper & Row, 1961); for the Open Society cf. supra, n. 19.

⁴⁴Karl Popper, "The History of Our Time: An Optimist's View," in Conjectures and Refutations, p. 365.

⁴⁵Ibid., p. 369.

⁴⁶Ibid.

⁴⁷Ibid., p. 376.

⁴⁸Ibid., p. 375.

⁴⁹Cf. Popper's contribution to Revolution Oder Reform: Herbert Marcuse und Karl Popper -- Eine Konfrontation, ed. Franz Stark (3rd edition; Munich: Kösel-Verlag, 1972); his letter to Claus Grossner on Adorno,

Habermas, and "revolutionary students," in Claus Grossner, Verfall der Philosophie (Reinbeck bei Hamburg: Christian Wegner Verlag, 1971), pp. 278-89, and Grossner's essay, pp. 136-50; Albert's "Kleines verwundertes Nachwort zu einer grossen Einleitung," in Positivismusstreit, p. 339; and Imre Lakatos, "Methodology," in Growth of Knowledge, p. 93: "Thus Kuhn's position would vindicate, no doubt, unintentionally, the basic political credo of contemporary religious maniacs ('student revolutionaries')." Lakatos, whose own commitment to Popperianism might be not unfairly described as "devout", is here using the term "religious" simply as a club to intimidate his opponents.

⁵⁰The formula: "Revolution in the intellect, evolution in politics," comes from the Popper letter to Grossner, cf. supra, n. 37; "piecemeal" social engineering is a favorite Popperian slogan.

⁵¹Kuhn, "Reflections On My Critics," in Growth of Knowledge, p. 238.

⁵²Cf. Barry Castro, "The Scientific Opportunities Foregone Because of More Readily Available Federal Support for Research in Experimental than Theoretical Physics," in The Journal of Political Economy, LXXVI, No. 4, Part I (July/August, 1968), pp. 601-14; also Kenneth Boulding, "The Scientific-Military-Industrial Complex," in The Virginia Quarterly Review, XLIII, No. 4 (1967), pp. 676-77: "The scientific estate, alas, swills as enthusiastically at the trough of the contract state as their more worldly brethren in the profit-making, and alas, even in the non-profit-making, institutions." To those not professionally in the philosophy of science, this conclusion will of course partake of a certain obviousness.

⁵³Kuhn, "Reflections," in Growth of Knowledge, p. 264.

⁵⁴Ibid.

⁵⁵Popper, "Logik," p. 115.

⁵⁶Paul K. Feyerabend, "Problems of Empiricism, Part II," in R. G. Colodny, ed., The Nature and Function of Scientific Theories (Pittsburgh: University of Pittsburgh Press, 1970), pp. 275-353.

⁵⁷Feyerabend, "Consolations for the Specialist," in Growth of Knowledge, p. 198.

⁵⁸Ibid., p. 201.

⁵⁹Ibid., p. 209.

⁶⁰Ibid., p. 212.

⁶¹Ibid., p. 213.

⁶²Feyerabend, "Against Method," in Radner and Winokur, ed., Minnesota Studies in the Philosophy of Science, Vol. IV: Analyses of Theories and Methods of Physics and Psychology (Minneapolis: University of Minnesota Press, 1970), p. 20. This may well have been dismissed as hyperbole in many quarters. It is not; the condition seriously affects productivity in research laboratories, cf. D. Davies, "A Scarce Resource Called Curiosity," in D. Lamberton, ed., Economics of Information and Knowledge (Middlesex, England: Penguin Books, 1971), pp. 315-22. It was to be expected that as scientific education expanded, curiosity would become a scarce resource and thus its price would rise. Alas, curiosity as a human trait which cannot be bought, and here may be an irresolvable contradiction in capitalism. This argument should be considered in the light of the last section of this paper.

⁶³Feyerabend, "Against Method," p. 72.

⁶⁴Ibid., p. 20.

⁶⁵Ibid., p. 101, n27B.

⁶⁶Feyerabend, "Consolations," p. 212.

⁶⁷Feyerabend, "Against Method," p. 106, n41.

⁶⁸Ibid., p. 90.

⁶⁹Ibid., p. 91.

⁷⁰Kuhn, "Reflections," p. 242.

⁷¹Ibid., p. 247.

⁷²Ibid., p. 245.

⁷³Feyerabend, "Consolations," p. 208, n2.

⁷⁴Habermas, "Technik und Wissenschaft," pp. 72-73; I have translated from the German this passage and those which follow.

⁷⁵Habermas, "Erkenntnis und Interesse," in his book Technik und Wissenschaft, p. 155; the other two "investigation-processes" are irrelevant in this context.

⁷⁶Ibid., pp. 156-57. That knowledge-constitutive

interests can in fact, if not necessarily in Habermas, be affected by conscious human choice must be pressed with some urgency. (This is not thereby to imply that one can privately reconstitute the world anew just before climbing out of bed in the morning.) Habermas, in his more tenebrous formulations, seems sometimes to treat "interest" as if "es sich nicht um wirkliche, nicht einmal um politische Interessen, sondern um reine Gedanken handelt...", (Karl Marx, Friedrich Engels, Die deutsche Ideologie, Werke [4th edition; Berlin: Dietz, 1969], III, p. 39); Feyerabend, with his profusion of concrete proposals, is clear that he intends no such stale (and Quietistic) Kantianism; one can only regret the obstacles which Habermas' relentlessly opaque style creates to the enlistment of critical assent.

In "The Dialectical Foundations of Critical Theory: Jürgen Habermas' Metatheoretical Investigations," Telos, No. 12 (Summer, 1972), pp. 93-114, Trent Shroyer employs a notion, alleged to be derived from Habermas, of an "invariant" (technical, etc.) interest; a view to which this essay is diametrically opposed. Constraints of time and space impel me to prescind from all the excellent points made in his paper, and merely to suggest a few of the reasons that undergird my conviction that the notion of "invariance" is false both to theory and to practice.

First, Shroyer's view that "the basic orientation of man to nature remains an ever-transforming, yet logically invariant, relation of instrumental action" (p. 101), is historically inadequate. It is simply not true that all ages and cultures have confronted nature with an eye toward extensive and expeditious plunder (the "technical interest"). Not even all "science", but only the Western, post-Renaissance version of it approaches nature from such a warped and truncated perspective. Even the very suggestion of an "ever-transforming...relation of instrumental action" would have appeared preposterous to nearly every major school of thought prior to Vico.

Secondly, statements such as "Valid beliefs are universal propositions..." (p. 101) only pump more hot air into a scientific balloon, and thereby camouflage the chaotic reality of the actual problem-situation in the sciences, where, as Feyerabend has put it, "not a single theory ever agrees with all the known facts in its domain" ("Against Method," p. 36). In this instance one sees clearly how the so-called "Critical Theory" now depends for its plausibility on a regressive image of science, and how that theory helps to conceal the political foundations of contemporary scientific self-understanding, thereby supporting the pretensions of the Positivism it claims to unmask.

⁷⁷Paul K. Feyerabend, "Philosophy of Science: A Subject With A Great Past," in Roger Stuewer, ed., Minnesota Studies in the Philosophy of Science, Vol. V: Historical

and Philosophical Perspectives of Science (Minneapolis: University of Minnesota Press, 1970), pp. 172-83.

⁷⁸Paul K. Feyerabend, "A Note on Two 'Problems' of Induction," in the British Journal for the Philosophy of Science, XIX, No. 3 (1968), p. 253.

⁷⁹T. W. Settle, "The Point of Positive Evidence -- Reply to Professor Feyerabend," British Journal for the Philosophy of Science, XX, No. 4 (1969), p. 353.

⁸⁰For Popper's position, cf. especially "Science: Conjectures and Refutations," in Conjectures and Refutations, pp. 33-65. Here Popper manifests his awareness that the entire Popperian theory of science depends on the existence of a continuum between the acquisition of scientific knowledge and that obtained through common-sense; that as he elsewhere puts it: "...scientific knowledge can be more easily studied than common-sense knowledge. For it is common-sense knowledge writ large, as it were. Its very problems are enlargements of the problems of common-sense knowledge." ("Preface -- 1959," in The Logic of Scientific Discovery (London: Hutchinson, 1959), p. 22.) His position is that inductions simply don't exist; that they are but conjectures, for all the shouting and knitting of brows which might accompany them. Settle would have to explain why some conjectures, and only those, qualify for the honorific status of inductions.

⁸¹Settle, "Reply," p. 353.

⁸²Ibid., p. 354.

⁸³Ibid., p. 355.

⁸⁴The phrase originates with Leszek Kolakowski.

⁸⁵Feyerabend, "Against Method," p. 106, n41.

⁸⁶Ravetz, Knowledge and Social Problems, p. 67.

⁸⁷Ibid., p. 58.

⁸⁸Ibid.

⁸⁹Ibid.

⁹⁰Ibid., p. 59.

⁹¹Feyerabend, "Consolations," p. 228.

⁹²Ravetz, Knowledge and Social Problems, p. 430. The

unreflective incorporation of a technical interest into social science research projects accounts partially for the oft-remarked ease with which their results are converted into instruments of further manipulation. The consequences for social science of a withering away of its paragon are difficult to imagine, but should not be underestimated in their magnitude. The price of "rigor" in a discipline such as political science has been mortis. Devaluing natural science would make it more likely that problem selection would be governed by the character of reality, than -- as at present -- by the state of existing theory.

BIBLIOGRAPHY

- Albert, Hans. "Kleines Verwundertes Nachwort zu einer grossen Einleitung." Der Positivismusstreit in der deutschen Soziologie. Edited by Th. W. Adorno. Frankfurt: Luchterhand, 1971.
- Baran, Paul, and Sweezy, Paul. Monopoly Capital. New York: Monthly Review Press, 1966.
- Boulding, Kenneth. "The Scientific-Military-Industrial Complex." The Virginia Quarterly Review, XLIII, No. 4 (1967), pp. 676-80.
- Castro, Barry. "The Scientific Opportunities Foregone Because of More Readily Available Federal Support for Research in Experimental than Theoretical Physics." The Journal of Political Economy, LXXVI, No. 4, Part I (1968), pp. 601-14.
- Davies, D. "A Scarce Resource Called Curiosity." Economics of Information and Knowledge. Edited by D. Lamberton. Middlesex, England: Penguin Books, 1971.
- Eisner, Robert. Review of The New Industrial State by John Kenneth Galbraith in Quarterly Review of Economics and Business, VII, No. 3 (1967), pp. 82-85.
- Eugelmann, Paul. Letters From Ludwig Wittgenstein. Translated by L. Fürtmueller. Oxford: Basil Blackwell, 1967.
- Feyerabend, Paul. "Against Method." Minnesota Studies in the Philosophy of Science, Vol. IV: Analyses of Theories and Methods of Physics and Psychology. Edited by S. Radner and M. Winokur. Minneapolis: University of Minnesota Press.
- _____. "Classical Empiricism." The Methodological Heritage of Newton. Edited by R. Butts. Toronto: University of Toronto Press, 1970.
- _____. "Consolations for the Specialist." Criticism and the Growth of Scientific Knowledge. Edited by I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press, 1970.
- _____. "Note on Two 'Problems' of Induction." British Journal for the Philosophy of Science, XIX, No. 3 (1968), pp. 251-53.

- _____
 "Philosophy of Science: A Subject With a Great Past."
Minnesota Studies in the Philosophy of Science, Vol. V: Historical and Philosophical Perspectives of Science. Edited by Roger Stuewer. Minneapolis: University of Minnesota Press, 1970.
- _____
 "Problems of Empiricism, Part II." The Nature and Function of Scientific Theories. Edited by R. G. Colodny. Pittsburgh: University of Pittsburgh Press, 1970.
- Frisby, David. "The Popper-Adorno Controversy: The Methodological Dispute in German Sociology." Philosophy of the Social Sciences, II, No. 2 (1972), pp. 105-19.
- Galbraith, John Kenneth. The New Industrial State. Second Edition, revised. Boston: Houghton Mifflin, 1971.
- _____
Economics, Peace, and Laughter. New York: New American Library, 1972.
- Grossner, Claus. Verfall der Philosophie. Reinbeck by Hamburg: Christian Wegner Verlag, 1971.
- Haberman, Jürgen. Technik und Wissenschaft als Ideologie. Frankfurt: Surkamp, 1968.
- Heller, Erich. The Disinherited Mind. New York: World Publishing Company, 1965.
- Johnston, William. The Austrian Mind. Berkeley: University of California Press, 1972.
- Koertge, Noretta. "For and Against Method." British Journal for the Philosophy of Science, XXIII, No. 3 (1972), pp. 274-89.
- Kolko, Gabriel. The Triumph of Conservatism. Quadrangle Edition. Chicago: Quadrangle, 1967.
- Kuhn, Thomas S. Structure of Scientific Revolutions. Second, revised edition. Chicago: University of Chicago Press, 1970.
- _____
 "Reflections on My Critics." Criticism and the Growth of Knowledge. Edited by I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press, 1970.
- Lakatos, I. "Falsification and the Methodology of Scientific Research Programmes." Criticism and the Growth of Scientific Knowledge. Cambridge: Cambridge University Press, 1970.

- Losee, John. A Historical Introduction to the Philosophy of Science. Oxford: Oxford University Press, 1972.
- Mandel, Ernest. The Formation of the Economic Thought of Karl Marx. New York: Monthly Review Press, 1971.
- Mannheim, Karl. Ideology and Utopia. New York: Harcourt, Brace and World, 1936.
- Marx, Karl. Zur Kritik der Politischen Oekonomie. Berlin: Duncker, 1859.
- Marx, Karl and Engels, Friedrich. Die deutsche Ideologie in Karl Marx, Friedrich Engels: Werke. Vol. III. Fourth Edition. Berlin: Dietz, 1969.
- Masterman, Margaret. "The Nature of a Paradigm." Criticism and the Growth of Scientific Knowledge. Edited by I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press, 1970.
- Miliband, Ralph. "Professor Galbraith and American Capitalism." Economics. Edited by David Mermelstein. New York: Random House, 1970.
- Musgrave, Alan. "Kuhn's Second Thoughts." British Journal for the Philosophy of Science, XXII, No. 3 (1971), pp. 287-97.
- Nieburg, H. L. In the Name of Science. Revised Edition. Chicago: Quadrangle, 1970.
- Popper, Karl. Conjectures and Refutations. Harper Edition. New York: Harper & Row, 1968.
- _____. "Die Logik der Sozialwissenschaften." Der Positivismusstreit in der deutschen Soziologie. Frankfurt: Luchterhand, 1971.
- _____. The Logic of Scientific Discovery. London: Hutchinson, 1959.
- _____. The Open Society and Its Enemies. Two Volumes. Fourth revised Edition. New York: Harper & Row, 1963.
- _____. The Poverty of Historicism. Third Edition. New York: Harper & Row, 1961.
- Price, Don K. The Scientific Estate. Oxford Paperback. Oxford: Oxford University Press, 1968.

- Ravetz, Jerome. Scientific Knowledge and Its Social Problems. Oxford: Clarendon Press, 1971.
- Schroyer, Trent. "The Dialectical Foundation of Critical Theory: Jürgen Habermas' Metatheoretical Investigations." Telos, No. 12 (Summer, 1972), pp. 93-114.
- Settle, T. W. "The Point of Positive Evidence -- Reply to Professor Feyerabend." British Journal for the Philosophy of Science, XX, No. 4 (1969), pp. 352-54.
- Stark, Franz. Revolution Oder Reform: Herbert Marcuse und Karl Popper -- Eine Konfrontation. Third Edition. Munich: Koesel-Verlag, 1972.
- Toulmin, S. E. "Does the Distinction between Normal and Revolutionary Science Hold Water?" Criticism and the Growth of Scientific Knowledge. Edited by I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press, 1970.
- Williams, L. Pearce. "Normal Science, Scientific Revolutions and the History of Science." Criticism and the Growth of Scientific Knowledge. Edited by I. Lakatos and A. Musgrave. Cambridge: Cambridge University Press, 1970.

APPROVAL SHEET

The thesis submitted by A. Thomas Ferguson, Jr. has been read and approved by members of the Department of Political Science.

The final copies have been examined by the director of the thesis and the signature which appears below verifies the fact that any necessary changes have been incorporated and that the thesis is now given final approval with reference to content and form.

The thesis is therefore accepted in partial fulfillment of the requirements for the degree of Master of Arts.

MAY 1, 1973
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