Philosophy and Methodology of Military Intelligence: Correspondence with Paul Feyerabend

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Introduction

In 1989 I wrote a paper on the logic of estimate process in military intelligence[1]. Since its central ideas were drawn from philosophy of science, I sent a draft to Professor Paul Feyerabend. I knew that my paper will intrigue him: after all, he is the one who wrote a book ***Against Method***[2] in science**,** and here I come and dare to claim that military intelligence would gain a lot by adopting the scientific method!

Surprisingly enough, I have got a (relatively) positive response from him, and this had started a chain of letters, which lasted for some years. The main correspondence is given below.

Before going to the correspondence itself, I think it would be beneficial for the reader to summarize, in a nutshell, the original paper I sent to Feyerabend.

The Logic of Estimate Process - A summary

My starting point was Israel's intelligence failure in predicting the outbreak of war on 6 October 1973. Many believe that it was a result of a certain fixed notion ('The Concept'), universally and rigidly held within the military establishment: Egypt would not go to war without a long-range air strike capability against Israeli airfields; Syria would not go to war alone; Since Egypt had no such capability, the probability of war was believed to be very low.

Some consider the general acceptance of this Concept as the root of evil. The fact that Israel's intelligence heads had an a priori concept about the necessary preconditions for outbreak of hostilities is considered to underlie the wrong estimate. A good intelligence estimator, they say, must free himself of all commitment to any single conceptual framework. Others claim that an intelligence estimate is not possible without some kind of conceptual framework.

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1 I. Ben-Israel, "Philosophy and Methodology of Intelligence: The Logic of Estimate Process", ***Intelligence and National Security***, Vol. 4 No. 4, October 1989, pp. 660-718.

2 P. Feyerabend, ***Against Method***, New Left Books, 1977.

Should one aim to eliminate conceptual framework? Can this be done? Is it possible to make an intelligence estimate without a conceptual framework? Does such a framework have any 'positive' role? If so, how should a conceptual framework in intelligence be built? What risks lurk within it? How and when should it be dropped? All these questions connect with one central question: is it possible to indicate methods for intelligence estimates which are 'better' and more 'successful' than others? Or, to formulate it more bluntly: how should one carry out an intelligence estimate?

Similar questions may be asked, in fact, in almost every field where information is gathered under conditions of uncertainty, processed, and used for forecasting. Intelligence is nothing more than an institution for studying and clarifying reality, and hence there is a clear analogy between intelligence and science (which is also such an institution with the same goal).

The intelligence field has its own particular aspects: it usually involves risking human life, so the cost of error is extremely high; security problems dominate and most conventional techniques for filtering errors are often blocked by security restrictions. Not only is intelligence material itself considered classified, but also its method of work. The classification of method is a serious obstacle to the development of intelligence, since it prevents open, systematic discussion of the methodological question of 'how the intelligence estimate should be done' (as well as many other related questions, like those mentioned earlier concerning conceptual frameworks).

For these reasons, I chose to conduct my analysis in what might initially be viewed as a devious, roundabout way: by studying conclusions and results accumulated during centuries of research in an entirely different field – the field of philosophy of science.

I began the paper I sent to professor Feyerabend by challenging the traditional method used for intelligence estimate, contending that a method, which inductively derives its conclusions from known data, is wrong.

Next, I claimed that the 'business' of intelligence estimate is to derive predictions from information. This is also the case in science. There is thus an analogy between science and intelligence at the level of the logic of prediction. I then proposed an alternative critical method, based on Popperian doctrine in philosophy of science.

This method is far better than the traditional approach, but not quite good enough. I therefore suggested a number of modifications and amendments. The resulting amended critical method turns the process of estimate upside down: conjectures (hypotheses) must first be raised and only then can ‘facts’ be approached – and even then, not to verify the ‘chosen’ estimate, but to refute the competing ones.

There remains, however, a substantial difference between ‘science’ (dealing with ‘dead’ and passive matter) and ‘intelligence’ (whose research object is active human beings and societies with free will). This difference prevents the application of specific scientific categories and methods to intelligence (or to any other branch of social science). In researching people and societies, it is never possible to transcend the realm of conjecture and hypothesis.

Nevertheless, I showed that my proposed amended critical ‘method’ (the logic of research), which considers all science as a set of conjectures, is applicable, in principle, to the field of intelligence estimate.

Furthermore, I demonstrated that the practical difficulties in applying my method to intelligence could be overcome. In confronting these difficulties, some practical methodological rules were drawn.

I also showed that conventional alternative methods, such as historicism, are not valid for intelligence estimate, neither in pro – or antiscientific form.

I concluded the paper I sent to professor Feyerabend by analyzing a major historical example: the intelligence failure of the October 1973 war, in order to clarify the main differences between the various methods.

26 September 1988

To: Professor Paul Feyerabend,
University of California, Berkeley

Dear Sir,

I am sending enclosed a copy of my paper, “Philosophy and Methodology of Intelligence: The Logic of Estimate Process”, in the hope that you will find it interesting enough for reading and sending me your comments.

The basic idea of the paper is a very simple one. Since I consider philosophy of science to be valid for any attempt to construct a systematic epistemic knowledge, I do believe that it can be applied to any field of the so called social sciences, and in particular to the field of military intelligence.

At first sight, it might seem as if I claim something that is in the opposite spirit of ***Against Method***. Well, I do believe that it is not so. If I read you correctly, you are not against all methods whatsoever, but against a single ruling one.

Philosophy and history of science teach us that there is not an a priori ‘right’ method for tackling a specific problem. It doesn’t teach us that methods are totally dispensable.

I tried to explicate these ideas, as clearly as I can, in the enclosed paper. In a way, it is a ‘case study’ in one particular (underdeveloped) field of ‘science’.

The paper was accepted for publication in ***Intelligence and National Security***, which is not a journal for professional philosophers, and therefore I had sometimes, to go into otherwise oversimplifications.

I shall be grateful for any critical comment you care to make.

Looking forward to hear from you,

Yours sincerely,

Dr. Isaac Ben-Israel

The Institute for History and Philosophy
Of Science and Ideas,
Tel-Aviv University

30 November 1988

Dear Isaac Ben-Israel,

Thank you very much for your fascinating study of the intelligence process. The suggestions you make are eminently reasonable, you have recognized that there are bound to be conflicting requirements and have recognized the need for a new kind of mathematics (your p. 74 – let me add here that foundational studies in quantum mechanics have moved away from differential equations into lattice theory and algebra, the direction suggested by you). But I wonder if the detour through the philosophy of science was really necessary.

To start with, the critical method was not “born in the philosophy of science” (p. 31), it is old hat; it was used by the discoverers of new continents, by businessmen like Marco Polo, by Generals like Clausewitz (whom you quote) and it was matter of course for the native tribes in Kenya (in the thirties) who, being faced with invaders of the most varied kind became more critical than the local missionaries who met only other missionaries.

Secondly, this ‘naively’ (i.e. unacademic) critical approach will most likely be more effective for it can also work in partly closed surroundings. Businessmen were and still are rivals, there can be a flow of information in some direction, not in others and so criticism here is adapted to conditions necessary for intelligence work but absent (to some extent!) from the sciences [I think it is an illusion to expect an <u>**opening**</u> of intelligence work. This not only <u>**will not**</u> occur, it <u>**must not**</u> occur as long as the present political situation prevails][3].

Thirdly, there is now the question of implementation, the most important question. In second-world-war England an improvement of intelligence work was achieved not by reforming the intelligence establishment via theory, but by introducing a second intelligence network, consisting of laypeople (in the field of intelligence) entirely: actors (like Noel Coward and Leslie Howard), scientists (like Turing) and others who had never done any intelligence work and were therefore unconfined by intelligence-prejudices (except the prejudice of secrecy). <u>**I may be wrong, but such a replacement seems to me more effective than a new training for the old cadres**</u> – and so, again, philosophy of science can be dropped from the scene. Besides, it does not even help <u>**within**</u> the sciences, which proceed in their own irrational way from discovery to discovery.

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[3] Feyerabend added this note in the margins of his letter.

So, in sum, I find your arguments excellent, your conclusions plausible – within the framework you have set yourself but I find this framework itself more a hindrance than a help. This, of course, may be my own sizeable personal prejudice. At any rate, thanks for sending me an interesting piece of material.

Paul Feyerabend

1 January 1989

To: Professor Paul Feyerabend,
University of California, Berkeley

Dear Sir,

Thank you very much for your kind letter concerning my paper on the logic of intelligence process. I am really grateful for the time you spent on reading it and especially for the critical comments you cared to make.

Your main criticism concerns the ‘link’ between intelligence process and philosophy of science: you take it to be unnecessary and “more a hindrance than a help”. If I read you correctly, you mean that (A) my analysis of the intelligence process and my suggestions are independent of philosophy of science, and (B) philosophy of science does not even help within the sciences.

Well, I accept both (A) and (B) above: science (as well as intelligence!) is a practice, and it does not depend at all on philosophy. If philosophy could improve science, it would probably be compulsory for students of science (and it is not)! Philosophy does have some effect on science, but it is a very indirect one through the influence on the cultural climate (with science as one of its manifestations) and not on the internal progress of some particular field of science. It is a well-known fact that the so-called ‘revolutions’ of the 17th and the 20th centuries were not confined to ‘science’ only. They included the arts, literature and poetry, architecture, military practice, social constructs, etc. Science is only one field of humane civilization, and it cannot be conducted in ‘free space’, isolated from its surroundings.

Query: are there societies and cultural climates that are ‘better’ for scientific progress? I believe the answer is in the affirmative, though I agree that these ‘better’ climates are not sufficient for scientific progress/ Moreover, they are not even necessary. For example, I do believe that ‘openness’ (glasnost!) is better for scientific progress, but I know that it is neither sufficient nor necessary for it.

In a way, the relation of philosophy to the practice of science (as well as to intelligence) is analogous to the relation of an elevator to the practice of reaching an apartment at the top of a very high building: the elevator is neither sufficient (it wouldn’t help without electricity and a key), nor necessary (one can still use the staircase); Nevertheless, it helps a lot!

The remarks above may seem a little confused, indecisive and ambiguous. But this is also the face of ‘reality’. Therefore, our efforts to understand Nature (the scientific effort) cannot be described entirely in 'rational' and 'logical' terms. This is not to say that we are not allowed to inject some order ('logic') into that mess. I think that this is what we are doing in the so-called 'philosophy of science', and this is what I tried to do in my paper.

I agree with you completely about the origin of 'critical method': it was not born in the philosophy of science (and I shall correct my paper accordingly). Nevertheless, I had my own reasons for introducing it through the framework of philosophy of science. Let me explain it in some detail.

-I-

To begin with, intelligence theory is in a very poor state. It almost doesn't exist. Now, not every practice needs a theory. But intelligence is (as the very term itself hints) a highly theory-laden intellectual activity. Therefore, I believe there is place for a revolution here. Unfortunately there are not many supporters of this view.

Some of the opponents (I shall call them group-A) do not understand the role of theory in their activity. They regard it as simply observing the enemy and deducing some conclusions from observational facts. Others (group-B), realizing the complexity of reality, do not believe in the human ability to arrange it in a systematic way.

I use philosophy of science as a weapon against these two groups. To the first group I say something like this: "don't be so naive. Your 'observations' are theory-laden. You must admit it in order to fight your prejudices ('false idols'). Unless you do that, you will remain in the same backward state as was science before Bacon and Newton". In other words, I use science and philosophy of science against group-A, as a tool for propaganda. I want them to convert to my view, and I use the highly respected phenomenon of science as an example. An example, of course, is not a proof. But I want to convince, not to prove.

This high respect for science stands at the root of the view of group-B. Science, they say, is a rational activity, which is successful because it deals with dead matter; Intelligence, on the other hand, is totally different: it deals with human irrational nature, and it cannot be done in a rational way.

Well, against this group I use philosophy of science in order to demonstrate the falsity of their view about science: it is not so 'clean' and rational, as they believe. And if science doesn't proceed in a rational way, then the basis for their contention drops. Again, philosophy of science does not prove that a similar progress is possible in intelligence too, but it eliminates the arguments against such a possibility.

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Wishing to revolutionize the theory of intelligence, I found myself in an urgent need for terminology and conceptual framework. Trying to make some short-cuts, I adopted, as a starting point, the terminology and concepts of philosophy of science. I found it suitable to start with because of the apparent analogy between science and intelligence (which I explained at length in my paper). I admit that it is not ideal for my purpose. I even suggested some amendments. But still, I believe in its being a good start more than a “hindrance”.

-III-

Thirdly, there is a message here for philosophy of science too. My paper is, essentially, a case-study of a (relatively unstudied and underdeveloped) special field of human activity – the field of intelligence. I believe that philosophy of science can draw some lessons from this study (in a way, intelligence is only one more way to study reality).

In sum, the detour that I took through philosophy of science had its own reasons: (1) science sells good; (2) it pulls the carpet from under the legs of some opponents of my call to reform intelligence theory; (3) it supplies an initial stock of basic concepts and terms for that purpose; and (4) it gives philosophy of science an opportunity to look inside a practical field of (semi) scientific inquiry.

Having said all that, I must admit that I am fascinated by your idea that the half-baked unacademic form of ‘criticism’ is more suitable for intelligence than its philosophical mate. This idea seems very interesting to me, and I feel that I need some time to ‘digest’ and explore it. Will it be too much to ask you for some elaboration of this point? (for example: What is the real character of the naive approach? i.e., what are the differences between it and the scientific concept of ‘criticism’? How does it function practically?).

I also like very much your suggestion to replace intelligence officers instead of trying to plant new ideas in their minds. But I wonder if it is practical (well, maybe it will be so in a state of emergency, as was the state of England during World-War-II).

Looking forward to hear from you,

and a happy new year,
yours sincerely,
Dr. Isaac Ben-Israel

Well, now that we seem to be involved in a longish debate, we should omit inessentials and stick to basics, viz., first names, therefore,

22 January 1989

Dear Isaac,

I agree with you that science is not isolated but subjected to many influences, philosophical influences among them. But while the philosophical ideas that affected the sciences in the past were closely connected with scientific practice and shared its fruitful imprecision, the ideas that come from modern philosophy of science (up to and including Popper and to some extent even Kuhn) are part of a school philosophy that gives some general and very mislead outlines but never descends to details.

Curiously enough the trend was started by neopositivism which prided itself of being a ‘scientific’ philosophy. However, ‘scientific’, for neopositivists did not mean ‘in contact with scientific practice’ but ‘in agreement with experience and the rules of logic’ where both ‘experience’ and ‘logic’ were defined in a very simplistic manner and independently of scientific research. The debates between the various schools of neopositivism (Quine and Popper included) may have been very dramatic for those immediately affected – but they had no effect whatsoever on the major scientific discoveries of the 20th century: relativity, quantum theory, hadron-unification by quarks, the electroweak theory, the issue between Big-Bang theories and Steady-State theories, the discovery of the structure of DNA, the ‘New Synthesis’ in biology and on basic scientific debates (such as the debate between Bohr and Einstein on the foundations of the quantum theory). All they did was to give historically incorrect accounts of the origin of relativity (here see the wonderful article by Holton on Einstein and the Michelson experiment), of quantum theory and so to confuse people instead of helping them. The only place where this mistaken and simplistic philosophy is being taken seriously is in the ‘weaker’ subjects, i.e., in some social sciences and here it is taken seriously only by people who have no original ideas and think that methodology might help them getting ideas. So, to summarize: philosophy is excellent if it is sufficiently complex to fit in with scientific practice. The philosophies you mention are not; they are “castles in the air” to quote Wittgenstein, they deceive people but do not help them.

Are there philosophies of science I would accept? Yes, there are – and they are being introduced by younger people who know science and its history in detail and describe what is happening. Read for example Andrew Pickering, ***Constructing Quarks*** and, especially, Peter Galison, ***How Experiments End***. Galison points out (a) that most existing philosophies of science deal with theories and treat ‘facts’ or ‘experiments’ in a summary way, (b) that experiments, especially large scale experiments such as those carried out at CERN and other institutes (which are financed by international agreements, involve hundreds of people and massive equipment) have a life of their own and (c) that agreement concerning a particular result and its 'meaning' is reached by a complex social process whose features change from one experiment to the next. Item (c)) is very interesting for your case for here, too, there are international agreements, different groups are involved having different ideologies (in physics the theoreticians think differently from the experimentalists and among the latter the data evaluators think differently from data producers etc. etc.) and the whole process is rather open ended. I would recommend to you to have a look at Galison's book and at an article by Holton, on Millikan and the charge of the electron, mentioned in it. Pointing out that scientists, when doing research, propose bold hypotheses and try to refute them is as unenlightening in such cases as the remark that scientists, when doing research, think – and as false: scientists often stick to timid hypotheses, never mind the evidence and often proceed intuitively, without explicitly discernible thought. I don't think it is bad to provide them with <u>**rules of thumb**</u> such as 'try to falsify your hypotheses' or 'look for experimental support' which may be considered, but also disregarded, but it is deadly to elevate such rules into '<u>**principles**</u> of rationality' – but just that is being done by the neopositivists and the Popperians.

Now, after this long speech (which, I hope, hasn't exasperated you), some details. On page 2 you say that[4] "philosophy… helps a lot". Well, my first remark is that anything you consider in the sciences occasionally helps, occasionally hinders research. This applies to mathematics, experiment, philosophy and what have you. There are many episodes where emphasis on experimental results impeded research and there are other episodes where emphasis on mathematics led to empty talk (some people believe that the so-called 'theory of everything' is such empty talk). Same about philosophy, especially about school philosophies that were constructed independently of scientific practice.

You say that you use the philosophy of science as a weapon against two groups, those who confound facts with theory (group A) and those who don't believe that humans can conquer certain domains of reality (group B). I would say that in using the philosophy of science you use a weapon that is (a) unwieldy and (b) ineffective (except when you are dealing with philosophers of science, of course).

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[4] See p. 11 above.

Lawyers at a trial convince a witness who says ‘but I saw it!’ that he **inferred**, but did not **see**; they do this without any detour through three or four different philosophies of science – and their arguments are much more effective than any such detour would be. Why? Because they appeal to commonsense and common experience and people can identify with it. People, ordinary people – and I guess that many intelligence experts are ordinary people in this sense – prefer appeals to commonsense to theoretical shenanigans. A lawyer who brings up Popper just doesn’t know how to conduct his case. As regards group B it suffices to mention **an example** where a structure was found in an apparently very disorderly area – **one** example, a **good** and simple example achieves much more than even the most sophisticated theoretical conversation. Philosophy of science as practiced today **simply is not good rhetoric** (except for those already immersed in it).

This is also the reason why I don’t regard the philosophy of science as a good starting point for constructing a conceptual framework in a theory of intelligence. First, because a theoretical framework may not be needed (do I need a theoretical framework to get along with my neighbor?). Even a domain that uses theories may not need a theoretical framework (in periods of revolution theories are not used as frameworks but are broken into pieces which are then arranged this way and that way until something interesting seems to arise). And, secondly, because frameworks always put undue constraints on any interesting activity. But I emphasize that this is just a feeling of mine and there certainly does not exist any rule that forbids you to start in this manner. I also feel that the inverse process is much more promising: the philosophy of science certainly can learn a lot from what is going on in hairy areas such as intelligence. As a matter of fact, I think that the problematic nature of intelligence work gives us a much better idea about man’s relation to ‘reality’ than physical science where things seem to go much more smoothly.

Final page – 4 – you repeat my recommendation of a “half baked, unacademic form of criticism”[5] and say that you are fascinated by it. There is another form of unacademic criticism that is not half baked but has a long tradition behind it, and I mentioned it above: the criticism a clever lawyer makes of the ‘evidence’ presented by a witness. I would strongly recommend paying attention to this kind of criticism for it is much more effective than the abstract considerations that emerge from the various school philosophies of today. Concerning the kind of ‘evidence’ that comes

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[5] See p. 13 above.

up in intelligence matters a good lawyer is at least 100 times more effective than a philosopher.

As regards my suggestion to replace intelligence officers by intelligent people from other areas, actors, academics and so on, I recommend to you the book ***A Man called Intrepid*** which describes the situation in Great Britain in the Second World war. Noel Coward, Leslie Howard, Turing were all involved and did much better than the intelligence establishment which was tied down by tradition and silly rules. Of course, it was a state of emergency but I think the same is true of the Near East of today.

Well, that is all for today. Let me conclude by saying that a case study of particular intelligence episodes would be an excellent way of improving the arid generalities of much of what goes for philosophy of science of today, so you should really invert your program; not, what can the philosophy of science do for intelligence, but what can intelligence do for the philosophy of science.

All the best!

Paul

15 April 1989

Dear Paul,

First, I owe you an apology for taking so long to respond to your last letter. I was away some months, and I am only now getting to read through the mail. However, let me go directly to our debate (as you did in your last letter).

On the General Situation

Let me begin with summarizing the general situation so far. In response to my paper on “the Logic of Intelligence Process” (LIP), you questioned the necessity of the detour I made through philosophy of science. To this I gave 3 different answers: (1) I wanted to **convince** the professional community to reform intelligence theory and thus I chose science as a paradigm (because it sells well and supplies strong arguments against certain groups of opponents); (2) philosophy of science provides off-the-shelf theoretical framework ready for (almost) immediate application to intelligence; and, finally, reversing the direction of my arguments, (3) there is a lesson here for the philosophy of science to learn from my case study in a remote field of ‘science’ (intelligence).

In response, you rejected reason (1), questioned (but not rejected) the validity of (2) and accepted (3). According to your view, (a) science doesn’t sell so well (lawyers sell a lot better); (b) theoretical frameworks are not **always** needed and their necessity for intelligence is questionable. Furthermore, philosophy of science (at least the Popperian school) is empty and cannot do any good.

Well, one good reason is enough. In fact, I can accept your criticism entirely and still justify my way (‘the detour’) because of (3) above.

Nevertheless, discovering some years ago that a complete agreement is not a sufficient reason for stopping a good debate, I feel that trying to counter you on your own ground will be beneficial for the subject I am interested in. So, let me try.

On the Value of Popper’s Philosophy

As a physicist, I have great sympathy toward your statement about the emptiness of what you call ‘modern school philosophy of science’ (including, and most especially, Popper’s). The emphasis it puts on logical considerations, at the expense of details, is really disturbing (as a physicist, it is even more disturbing to read papers of certain ‘philosophers’ about the implications of, let say, quantum theory, when it is clear that these authors do not have the slightest idea what it really looks like).

I agree that Popper’s philosophy is wrong. In fact, I rejected it in my paper (LIP). But, you say much more: it is not only wrong, but misleading and useless as well. Well, I admit that sometimes it is misleading; however, it is not always the case (depending on the attitude of its audience). But, I don’t think it is always useless. I am afraid that here you miss one of the most prominent characteristics of Popper’s philosophy: it is always **stimulating**. Sure it is wrong, and it takes time to show its falsity. But, the process of exposing its fallacies is a fruitful one. My LIP, and even some of your own best papers (e.g., “Consolations for the Specialist” or Chap. 9 in your ***Philosophical Papers***, Vol. 2) are good demonstrations of this.

On Emptiness and Informativeness

If I remember well, you once said that the **content** of philosophy of science **evaporated** with the shift taken by logical-positivists (including Popper) from **details** to **structure** of scientific theories (I don’t remember the exact reference).

This line of reasoning seems to be a product of positivistic attitude by itself: it is based on the famous positivistic Analytical Thesis dividing all statements into two groups – **empirical** and **formal** (analytic, logical, you name it). Taking this assumption for granted, it is clear that any philosophy can be either **informative** (that is – based on empirical evidence) and therefore, non-logical and perhaps even irrational, or **logical**, that is, non-informative (empty). In fact, a teacher of mine in the past and a colleague at present, Professor Zev Bechler, divides all possible philosophies into these two types (he calls them Platonic and Aristotelian accordingly).

I think that these distinctions are based on wrong assumption (the Analytical Thesis). Well, it is well known that Kant thought there is a third group (the synthetic a priori), but he is not so popular nowadays, after the collapse of some of his central examples (Newton's physics, Euclid's geometry, etc.).

Personally, I don't think he was totally wrong. I agree that his examples are outdated, but not his basic ideas. In fact, I think that his philosophy of science can be updated (and I even carried out part of this program in my Ph.D. thesis). Kant knew something, which was forgotten by the logical-positivists: he knew that philosophy of science cannot be detached from **philosophy of man**. For him, philosophy of science, epistemology and metaphysics meant the same. This is the reason that in his ***Logic*** (edited by Jaesch in 1800) he adds to his famous three questions (what can I know? what can I do? What can I hope for?) a forth one – what is man? and he says that "the first question is answered by metaphysics, the second by ethics, the third by religion and the fourth by anthropology. **Basically one could count all these to anthropology, because the first three questions relate to the last one**"[6]. Unfortunately I have written on this subject only in Hebrew. However, enclosed[7] is a draft of an unfinished paper on Kant's philosophy of science as I interpret it. I would like very much to hear your opinion on it.

If I am right, there is an option here for philosophy of science which is relativistic (in the epistemological level) and yet realistic, and at the same time linked tightly to philosophy of man.

On Scientists vs. Lawyers

Now, let me come back to your remarks.

I have already said that I like very much your idea about the practical criticism of, let say, Columbus, sailing westward to 'test his worldview ('theory'). I still feel that this idea should be explored thoroughly.

However, in your last letter you say that

> "There is another form of unacademic criticism that is not half baked but has a long tradition behind it […]: the

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[6] My underlining.

[7] The paper, "Kantian Metaphysical Foundation of Relativity Theory" is not fully given here, since it played no major role in the followings. Instead, I attache only the annex to this draft, since it bears some relevance to topics discussed below.

> criticism a clever lawyer makes of the 'evidence' presented by a witness",

and you

> "strongly recommend paying attention to this kind of criticism for it is much more effective than the abstract considerations that emerge from the various school philosophies of today. Concerning the kind of 'evidence' that comes up in intelligence matters a good lawyer is at least 100 times more effective than a philosopher."

Well, I am afraid that I don't find this recommendation to be suitable neither for intelligence nor for science. Let me explain why.

Lawyers, as you rightly notice, are experts of persuasion. But their techniques 'work' only in a well-defined environment, namely, the court. The 'rules of the (legal) game' are carefully formulated, known to every 'player', and that is essential for their work. Actually, they rarely convince the jury that the defendant is not guilty; instead, they convince the jury that they cannot find him guilty **given** certain laws. Can you imaging their work without a detailed codex, trying to prove that their client is innocent without knowing the rules of the game? The story of Joseph K is a fine demonstration of this absurdity. Intelligence officers (and scientists as well) don't have any 'rules' that can be taken for granted. For them, **everything** is up for grabs (at least it should be so).

Thus, I don't think that the 'criticism' applied by lawyers is suitable for our purpose: it is a sterile one, effective only when the 'enemy' and the 'war' rules are given beforehand. Scientists and intelligence officers do not have this luxury. They have to struggle with unknown enemy (or Nature) and there are no real constraints for this secret war. Anything goes!

On Intelligence in the Second World War

This character of intelligence work has been remarkably demonstrated by the British secret war during the Second World War. You mentioned (twice) the book ***A Man Called Intrepid***, telling the story of Sir William Stephenson (by the way, he passed away last month). I read the book many years ago and I share your appreciation of it. Nevertheless, I think that the best description of British intelligence during that period, from the point of view of **estimate** and **analysis**, is given by R. V. Jones in his ***Most Secret War***. Jones was the man who invented and founded the disciplines of scientific intelligence, starting the war as a junior officer and ending it at the top of British intelligence as Director of Scientific Intelligence. Reading carefully his story, it is evident that his 'secret of success' was the way he used his head. He certainly used it differently (see chap. 37 for example).

In a way, his story supports your recommendation to replace old cadres by young fresh minds; but I think it also proves this is not sufficient. The new system may suffer from a lack of experience and be worse than the older. Usually, what is needed is some **combination** of fresh minds with a professional experienced establishment. Of course, the relative weight of these two should be carefully balanced (one element can easily overweight the other).

All in all, I do believe that the story of British intelligence during the Second World War supports my view concerning the type of reform needed in intelligence theory.

By the way, both Jones and Stephenson were recruited and introduced to Churchill by Professor Frederick Alexander Lindemann (1886-1957), who served together with Sir William Stephenson as pilots in the same squadron in First World War, and who supervised at Oxford the Ph.D. thesis in physics of young Mr. Jones between the wars.

Professor Lindemann, (later Lord Cherwell) is also the father of another idea which is relevant to my subject: **operations research**, that is, applying scientific methods to questions of war which were considered, up to that time, as an art. The outstanding success of this technique should teach us some profound lessons about the (non) demarcation of science and humanities.

Well, I feel that a new subject emerges here. So I will hold myself and keep it for the next occasion.

Looking forward to hear from you,

yours sincerely,

Isaac Ben-Israel

Kantian Metaphysical Foundation of Relativity Theory (A Draft)

Isaac Ben Israel

Annex: Summary of Kant's Philosophy of Science

We can summarize Kant's philosophy of science with the following scheme:

(1) Let us consider a certain **law of nature**, L. L is either true or false. In case it is true, then, being a **law**, it must also be **necessary**.

(2) The Copernican Assumption: If L is necessary (i.e. it is a **true** law of nature), then its necessity can have no source other than the **structure of human cognitive mind**.

(3) Therefore, we can use L to **reveal** this structure. We can use the alledged necessity of L in order to derive a corresponding principle, P, which governs the human cognition. Doing so, we should always bear in mind that:

(a) P is 'derived' from L in the same way as theoretical entities are 'derived from factual evidence. That is, P is merely a **hypothesis** that enables us to **explain** certain phenomena of epistemology.

(b) If L proves to be false, that is - if our belief in its being a true law of nature turns to be not so well founded, then we should have to abandon P as well.

(c) P will be a suitable and **well-established** hypothesis if we succeed in showing that it is a **necessary condition** for L, i.e. that L could not be a true law of nature **unless** P holds.

(4) Such a proposition P is **synthetic a priori**:

(a) It is **synthetic** because it is not logically or linguistic necessary (we can perfectly imagine a consistent possible world populated with creatures that have a totally different cognition faculties from ours).

(b) Experience cannot confirm or falsify it, because experience itself is based on P (as a result of the Copernican assumption). Therefore P is **a priori**. In other words, P is epistemically necessary, i.e. it is necessary for (and only for) the human mind with its own peculiar understanding.

(5) The method, described above, of deriving synthetic a priori principles from experience is called **'transcendental'** method.

(6) Using the set $\{P_1, P_2, P_3 \ldots\}$ of synthetic a priori principles that were obtained by the transcendental method (from the laws $L_1, L_2 \ldots$), we can continue and logically derive from it some more propositions,

S_1, S_2, etc. All these propositions will be a priori, because they reflect the principles that govern human understanding, and therefore their truth does not depend on the specific content of our experience.

(7) We must distinguish carefully the **epistemic** status of the principles P1, P2… from the status of their **justification**. Every P_i is a priori although the propositions that take part in its justification are based on experience.

(8) Therefore, No one of these principles (not P_1, P_2… nor S_1, S_2…) is (absolutely) **certain**. Their truth is based on an empirical basis (i.e. the truth of L_1, L_2…) and hence it can be doubted. The synthetic a priori principles are **hypotheses** about our mind, which must be tested against our experience. But of course, if their empirical basis is firm, then they will be firm and certain as well.

(9) One can show that every L_i is a priori in itself, because it can be derived from the synthetic a priori principles P_1, P_2… This procedure is, of course, circular. Nevertheless, it is not worthless. It completes the analytical side of the derivation and reveals how the fundamental laws of nature are consequences of our formal conditions of understanding; and thus it demonstrates how a certain law of nature (namely, the vast observation data it carries) can be counted as supporting evidence in favor of the hypotheses P_1, P_2…

30 May 1989

Dear Isaac,

While you were gallivanting about I got married (for the fourth time, but for the first time seriously) and now my first priority is to find a home for both of us (in Italy – my wife is an Italian), then to found a family and then to resign from all my jobs to take care of it or, at least, to linger nearby, in case some catastrophe arises. Still, I at once reply to part of your letter (not to the Kant essay – that one I shall read later).

Starting from the end I most enthusiastically subscribe to the 'new subject' which is going to emerge from the non-demarcation of the sciences and the arts-humanities. As a matter of fact, my basic objection to any philosophy of science that constructs a methodological system is that it overlooks the art-aspect of science which requires **rules of thumb**, lots of them rather than **methodological principles**.

[Popper is ambiguous on this point; on the one side he started his lectures at the LSE in 1952 with the comment: "I find myself in a paradoxical situation; I am a professor of scientific method – but there is no scientific method; there are only rules of thumb"; and then he proceeded to develop his falsificationism. On the other hand he often wrote and spoke as if overruling this set of rules of thumb was a crime against reason herself. I accept falsification as a rule of thumb. It is a very old rule: for the sophists the use of counter examples was one of the most efficient ways of advancing an argument. Ancient philosophers also knew about its limitations; thus Plato, in various of his dialogues called the brute use of counter examples **antilogike** – word bashing, and recommended a more sophisticated procedure. It is a very useful rule – but it is not a condition of rationality and' historically, falsification is not the most frequent and most efficient motor of scientific change (as Popper asserts in his ***Postscript***).]

The art aspect of science becomes very clear from research done by a new (post-Kuhnian) generation of historians. An example is Peter Galison, ***How Experiments End*** (do you know the book? I found it most interesting). Here he discusses three episodes, the Einstein-de Haas experiment, the discovery of the muon, and the acceptance of neutral currents. The important point is that the distinction between a context of discovery and a context of justification, so important for principle-ridden philosophies of science, simply does not exist and, that unanimity concerning a certain effect is the result of developments and debates that have much in common with what precedes the conclusion of a political treaty; no party is really satisfied, there are compromises, because there are many parties (the Western group under Millikan and an Eastern group in the muon case)]. And a 'fact', then, is the result of such compromises on the basis of shared or partly shared rules of thumb. Reading these stories (and Andy Pickering's ***Constructing Quarks***) made it clear to me that a principle-bound philosophy of science just finds no point of attack in the scientific material. It is not 'false' – it is irrelevant.

But – and here I now accept your view – it is not therefore entirely useless. A scientist may find comfort or inspiration in relating some of his rules of thumb to a methodological system and in modifying them as a result. What affects his research, still are rules of thumb; but what affects his acceptance of some rules of thumb over others and what gives him the confidence to use unusual rules of thumb may be a system. However, applying the system directly, or turning the suggested rules into principles because they come from the system – that is a very dangerous thing. This is how I summarize, for my use, what you write in the bottom part of page 2.[8] And this is the extent to which I grant a 'stimulating' quality to popper's philosophy, insofar as he regards it as a system.

I emphatically agree with the need to view epistemology etc. as parts of anthropology (though I would rather say 'politics', for anthropology is a special subject run by intellectuals while democratic politics, ideally, is run by all). I always was very impressed by the way in which Aristotle criticized Parmenides' arguments in favor of an unmoving and indivisible ONE. There were tow criticisms. The one was logical: it looked at the argument and tried to show its faults. But there was a second criticism which I would formulate as follows: Parmenides' ONE makes nonsense of life in the city hence, anybody who chooses life in the city has to reject Parmenides. For me this means that **<u>the definition of what is real and what is 'mere appearance' (or what is objective and what is subjective) depends on what kind of life one wants to live</u>**. It is the result of a political decision. Hence, epistemology without politics is incomplete and arbitrary. Adding anthropology is a little better but still remains within the domain of thought of a small minority.

Lawyers: I agree with your objection but add that lawyers can also work outside well-defined frames and that their practice inside the frame gives them an experience that is better suited for the discussions of intelligence problems than the experience of philosophers of science which is purely abstract and out of touch with human nature. (Joseph K is about the system only.) I also agree that while fresh blood is good, a combination of fresh blood and professional experience is even better **<u>provided</u>** the

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[8] See p.21 above.

institutional arrangements neither offend the experienced old-timers nor give them too much power. But that is difficult to do.

Thank you for mentioning Jones' book; I had heard of it, but now I know a little more and shall try to get it. One thing is sure: the field of intelligence certainly is an excellent testing ground of rules of thumb, principles, methodological systems.

And now I have to run. All the best – I'll soon write again. Long time ago I had some exchange with Zev Bechler. I also used some of his papers in my class on philosophy of science (mainly what he wrote about Newton). Give him my regards!

Paul

15 June 1989
Dear Paul,

Let me open this rather short letter with an old Jewish greeting: Mazel Tov (good luck) for you and your new wife [by the way, is her name Grazia? The intelligence estimator inside me couldn't resist the temptation to guess her name…].[9] Please give my heartiest congratulations to her. Interesting enough, our ancient fathers probably thought that what one needs for a successful marriage is **luck** (but I guess I don't have to tell this old wisdom to a man who gets married for the fourth time, even if the previous three were not serious…).

Marriage, after all, is a serious business. So I shall not mix it with any philosophical chatting and postpone my reply to your last letter to another opportunity (maybe after you will comment on my unfinished paper on Kant). Meanwhile I shall have time to finish reading Galison's ***How Experiments End*** (you see, I followed your recommendation and I'm reading it now).

So, Mazel Tov again,

and looking forward to hear from you,

yours sincerely,

Isaac Ben-Israel

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[9] I added this note at the bottom of my letter. My (successful) guess of her name was based on the acknowledgement of Feyerabend in his book, ***Farewell to Reason*** (Verso, 1987 two years before the start of our correspondence) to "my beautiful, good and very patient friend Grazia Borrini".

5 October 1990

Dear Paul,

It has been a long time since I received a letter from you. Since my last one included only greetings for your marriage, I decided not to wait any more for a reply (and the promised critique of my paper on Kant) and to comment on your last letter from May 1989.

By the way, my paper on "the logic of estimate Process" (LEP), which started our exchange of letters, has meanwhile been published, and a preprint is enclosed. Unfortunately it was too late to incorporate some of your comments but I managed however to make some minor changes (cf. Note 28 in p. 715).[10]

-I-

Your remark on the way Aristotle criticized Parmenides' ONE is very interesting and raises immediately a host of important questions (by the way, can you give me the exact reference of what you call Aristotle's 'political argument'? unfortunately I couldn't locate it). Let me write down few of these questions.

First, and perhaps the most important, - what exactly do you mean by saying that "**what is real and what is 'mere appearance' (or what is objective and what is subjective) depends on what kind of life one wants to live**"? How does one decide what is real? Can he decide anything he wants (provided it fits his needs), or is he limited by some **external**

[10] This note reads as follows: "The critical method was studied in philosophy of science, but it was not created there: 'it is old hat; it was used by the discoverers of new continents, by businessmen like Marco Polo, by Generals like Clausewitz […] and it was matter of course for the native tribes in Kenya (in the 1930s) who, being faced with invaders of the most varied kind became more critical than the local missionaries who met only other missionaries. […] This 'naively' (i.e. unacademic) critical approach will most likely be more effective for it can also work in partly closed surroundings. Businessmen were and still are rivals, there can be a flow of information in some direction, not in others and so criticism here is adapted to conditions necessary for intelligence work but absent (to some extent!) from the sciences' (Paul Feyerabend, a private letter to the author, 30 November 1988)."

constraints? In other words, is the definition of what is real **arbitrary** or is there a supreme judge, namely, the **external world** itself?

Well, one can immediately reply, as you do (following Protagoras) in ***Farewell to Reason*** (***FtR***, p.44), that although every society can choose and define its own 'reality' in accordance with its own specific needs, it doesn't mean that this choice is arbitrary: after all, it should fit those needs! Relativists, like Herodotus and Protagoras, don't have to assert "that institutions and laws that are valid in some societies and not valid in others are therefore arbitrary and can be changed at will […]. One can be a relativist and yet defend and enforce laws and institutions" (***ibid***.).

Still, this doesn't answer the question of **external** factors. The 'reality' (or to use a more appropriate term, the 'objectivity') of social laws and institutions is one thing, and the reality (and objectivity) of physical phenomena is another thing. One can easily be a relativist in social and cultural matters and, at the same time, be a 'metaphysical' realist (that is, believe that the laws and ontology of the external world, unlike social laws and institutions, do not depend on the way we want to live).

You argue (quite convincingly, allow me to say) for social and cultural relativism: your relativism is "about human relations" (***FtR***, p. 83). This is a popular doctrine today (as you notice yourself, cf. ***FtR***, p. 77). You also argue for epistemic relativism. This quite convincing too; after all, how can we be sure that we **know** the truth, even if there is one? The question I would like to formulate now is whether this contradicts metaphysical (ontological) realism (concerning the physical world). In other words, **does social or epistemic relativism necessarily imply metaphysical (i.e. ontological) relativism?**

I admit that I don't see any necessary logical relation between the tow: it seems clear to me that arguments for social, cultural and even epistemic relativism do not establish the case against (scientific) realism. We can easily decide what social laws and institutions we would like to have, but we cannot do it, at least to the same extent, with physical laws. As I see it, in the case of external world you cannot ('politically') **decide** what there is. The external world confines us to a very narrow space of liberty. True, **there is** latitude here. Reality doesn't force us to hold one and only one view about it; but this latitude is not unlimited. One cannot arbitrarily decide **whatever** one likes.

Take for example Galison's ***How Experiment End***. You mention it in your letters and papers (cf. "Realism and the Historicity of knowledge", ***The Journal of Philosophy***, Vol. LXXXVI, No. 8, August 1989) recommending it as worth reading and as a good example of new and better philosophy of science. I enjoyed reading it although I had already known its moral – that experimental ‘facts’ are sometimes the results of debates and compromises between rival groups on the basis of partly shared beliefs (ibid., p. 394. I guess any physicist who actually practiced experimental physics knows it). This fading of the line between the context of discovery and the context of justification, exactly in the sense Galison describes, is a direct result, I think, of the theory-ladenness of ‘observations’.

In fact, I have already written in my LEP (pp. 672-673) that:

> “Any observation report, including those in physics, is **theory-laden**; it can never be ‘pure’ observation and always presumes certain hypotheses. Every observation contains, apart from sensual element, an element of interpretation. […]
>
> This problem of interpretation is common to many kinds of human research and knowledge. It is not more acute in intelligence than in the physics of elementary particles, for example, where no one can ‘see’ (with the naked eye) any particles and where any observation depends on clusters of theories, hypotheses and interpretations of measurements”.

Galison makes an extraordinary (and successful) effort to show how strong is the mutual interaction between theoretical presuppositions and actual ‘results’ of experiments. One can read his study as a historical account confirming the views of Duhem, Kuhn and you. However, I believe that the main lesson one can derive from it goes, perhaps, against the dominant line of contemporary philosophy of science. True, ‘experimental results’ are a product of negotiations between rival parties, **but** only to a certain extent. It is very interesting to notice that Einstein ‘measured’ the very g=1 (in Einstein-de Haas experiment), he theoretically expected, instead of the now believed value of $g \cong 2$. But, what is more interesting is that even such an authority as Einstein’s (in 1915!) was not enough for the scientific community to stop experimenting and publishing **different** results. Doesn’t it show that one cannot **arbitrarily** decide **whatever** he likes?

Reading your papers through the years, I always had the feeling that you tried to stick to (ontological) realism despite your preaching for epistemic relativism (sometimes you even expressed this tendency explicitly; Cf. Chaps. 2 &11 in your ***Philosophical Papers***, Vol. 1, [***PP1***]). I must admit that after reading your ***FtR***, I’m not so sure about this any more. Perhaps you will care to comment on this issue.

-II-

Before going on to other matters, allow me to describe my view on the question of realism-relativism. Basically I am an empiricist (who isn’t?), that is, I believe that everything we **<u>know</u>** about the external world comes through our senses (filtered, processed and distorted as it is by our 'internal' cognitive faculties). Every (scientific and non-scientific) **<u>knowledge</u>** we have depends on some (partly explicit and mostly implicit) assumptions and is true only as long as we accept some other propositions as true. **<u>Knowledge of the external world is always relative</u>**. Now, if so, what is the point in holding a realistic position? If it is clear that we cannot know the external world 'as it is' (to use Kant's phrase), why assume that **<u>there is</u>** a 'world' which is causally independent of us, the observers?

In order to start answering this question one should realize that **<u>everything</u>** we know is only a **<u>conjecture</u>**: **<u>If</u>** it is true that the world is going in that way and not in this, **<u>then</u>** a certain proposition **P** is supposedly true. There is no ultimate established body of knowledge. No knowledge can be more than a **<u>theory</u>** (i.e., a set of conjectures) about the world, and this includes philosophical contentions as well.

Now, how do we judge 'theories'? What are the criteria for holding this theory and not that? There are many possible replies to this question. I prefer the one which judges a theory by its compatibility to some basic set of (accepted) 'facts' (if it doesn't, there is a problem somewhere in the theory or in the set of 'facts' or in both).

So, why do I hold epistemic relativism? Because it is the only position (theory) I know which fits certain basic 'facts' (historical facts, logical arguments, etc. Your ***Against Method*** [***AM***] is full of such 'facts'). I other words, I hold epistemic relativism because it is a plausible theory. It explains a lot of 'facts' I 'know'.

And do I want to hold metaphysical realism? Exactly for the same reason. It is too a (very) plausible theory without which it would be difficult to understand certain 'facts'; for example – the universal and overwhelming agreement between human beings as to certain simple observations of what happens in the world ("The conception of a world that really exists is based on there being far-reaching common experience of many individuals, in act of all individuals who come into the same or a similar situation with respect to the object concerned" wrote Schrödinger to Einstein in 18 November 1950).

Another strong 'evidence' for realism is the existence of error. Without realism it would be highly difficult to explain the 'fact' that we do err in our judgments of the external world.

Summarizing the arguments above, **<u>there are very good reasons for being a realist and relativist simultaneously</u>**. Furthermore, I believe that it is even possible. In fact, my main task in my Ph.D. dissertation was to construct a multi-layer model of modal propositions (I call it 'the Modal

Onion Model' – MOM) and use it to demonstrate the possibility of holding a certain proposition to be necessary and contingent at the same time (of course not for the same **<u>type</u>** of necessity). The model enables one to be a relativist and realist at the same time **<u>coherently</u>**. I would like to elaborate on it a little, but before doing so, let me discuss first the relationship between realism and relativism.

-III-

Following your general advice, let me discuss this relationship through a concrete example: motion. Newton's concept of motion is **<u>absolute</u>** and **<u>real</u>**. Einstein's concept is **<u>relative</u>**, yet as **<u>real</u>** as Newton's. Hence, at least in the case of motion, **<u>'relative' is not the opposite of 'real' but of 'absolute'</u>**. I think that this conclusion can be generalized: **<u>relativism and absolutism are exclusive; relativism and realism are not.</u>**

Now, you condemn absolutism. Science, you claim, "never obeys, and cannot be made to obey, stable and research independent standards" (***PP1***, p. xiii), that is, there are no absolute standards in science. As a matter of fact, there are no absolute standards in any field of human activity. In your ***FtR*** you write: "The assumption that there exist universally valid and binding standards of knowledge and action is a special case of a belief whose influence extends far beyond the domain of intellectual debate. This belief may be formulated by saying that there exists a right way of living and that the world must be made to accept it" (pp. 10-11), and you observe that this belief was the driving force behind many evils which were done through the history of human kind.

So far so good. Absolutism has to be rejected. Hence, relativism has to be preferred. But, doing so, why do we have to reject realism? Your claim that "realism […] reflects the wish of certain groups to have their ideas accepted as the foundations of an entire civilization and even of life itself" (***PP1***, p. xiii) confuses between realism and absolutism. Surely one can be a realist (i.e. believe that the world exists independently of our knowledge of it) and still admit that this knowledge is fallible and not absolute.

I planned to go on and elaborate here on my Modal Onion Model, but I realize now that this letter became rather long. So I will keep the rest of it for the next exchange of letters.

Mazel Tov again,

and looking forward to hear from you,

yours sincerely,

Isaac Ben-Israel

11 October 1990

Dear Isaac,

I am about to leave for a trip to the South of Switzerland, California and Italy, therefore I cannot give you a **detailed** reply to your letter. However I enclose a paper[11] that deals with precisely the problems you raise and may, perhaps, give an answer here and there.

In your terminology – I do indeed assert that social relativism entails metaphysical realism, **though only to some extent**. [The underlined passage describes the extent to which I now differ from that said before: relativism is possible **because the world permits it – to a certain extent][12]**. My argument is a metaphysical argument: reality (or Being) has no well-defined structure but reacts in different ways to different approaches. Being approached over decades, by experiment of ever increasing complexity it produces elementary particles; being approached in a more ‘spiritual’ way, it produces gods. Some approaches lead to nothing and collapse. So I would say that different societies and different epistemologies may uncover different sides of the world, provided Being (which has more sides than one) reacts appropriately. I know, all this sound quite mystical but I think it can be worked out to sound more plausible. At any rate, the typescript[13] and the printed paper are first steps.

Incidentally, there is no way of finding out the limit to which the world permits relativism because **Being itself** cannot be known (I have argument for that, too). What **can** be known is **manifest Being**, i.e. the response of Being to a particular approach.

I looked briefly into your printed paper which looks very good. I noted you ascribe proliferation and criticism based on it to Popper. This is a little unjust to Mill (***On Liberty***) who gave much better argument for both.

Best wishes!

Paul

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[11] The paper enclosed was P. Feyerabend “Realism and the Historicity of Knowledge”, ***The Journal of Philosophy***, Vol. LXXXVI, No. 8, August 1989.

[12] Feyerabend added this note at the bottom of his letter.

[13] The typescript that was enclosed had the title “Ethics as a Measure of Scientific Truth” and was dated from 16 August 1990.

25 October 1990

Dear Isaac,

You ask what I mean when saying that what is real and what is not depends on the kind of life one wants to lead. Homer does not have a single great distinction real/apparent. His world contains many different things, people, animals, thunderstorms, gods, dreams among them. They all can be experienced, though under different and sometimes very complex conditions (just think how difficult it is to get a glimpse of a very shy bird -–you can hear him, but to get a full view of him is almost impossible). Now the Presocratics and then Plato replaced this complex net by a bifurcation real-unreal.

Why? In Plato the reason is clear – it is political: to have the stable society which, according to him, is needed for happy human beings, you have to tie your existence to what is stable. These are the most important ingredients and Plato plus some Presocratics call them by names, which one could roughly translate as ‘real’.

Much more recently some molecular biologists, Delbruck among them, have suggested to abolish botany, zoology etc. in favor of a comprehensive molecular biology. Now persons for which direct contact with people, plants, and animals forms the basis of their lives will not reject molecular biology. They will regard it as adding interesting details and will interpret it as an instrument of prediction, when it conflicts with thw macro-phenomenological way of life.

And so on: the ‘external world’ is that **<u>part</u>** of the many strange phenomena that surround us which conforms most closely with our most important beliefs, attitudes etc.

Think of ‘health’ as an example. For some, ‘health’ is a good working condition of the body-machine – for others it is happiness and benefit for others – even if the body is not quite what it ought to be according to the body-plumbers.

Now I don’t think this is relativism. Relativism presupposes a fixed framework. But people with different ways of life and different conceptions of reality can learn to communicate with each other, often even without a gestalt-switch, which means, as far as I am concerned, that the concepts they use and the perceptions they have are not nailed down but are ambiguous. They are ambiguous to an extent that you cannot even speak of theory-**<u>ladennes</u>** – which assumes that there is something non-theoretical carrying the load. The best thing is to drop this dichotomy theory/observation which is still an aftereffect of the old belief that something called ’observation’ is the final arbiter of everything. Where is 'observation' in Machamer's more complex examples? Computers do the dirty work and the scientist simple reads the results – he 'observes' computer printouts (see the enclosed short note[14]).

Of course, one cannot **arbitrarily** decide whatever one likes – at least as long as one is part of a community. On the other hand, this community has neither a wise man nor a wise process ('experience', or 'experiment') to turn to, all it has are the results of temporary political treaties – and these results will last only as long as the political situation remains fairly stable (compare new experimental equipment with a new political ideology, like fascism).

You are right about my tendency to emphasize realism but I have changed on that. I now distinguish between an **ultimate reality**, or Being. Being cannot be known, ever (I have arguments for that). What we do know are the various **manifest realities**, like the world of the Greek gods, modern cosmology etc. These are the results of an interaction between Being and one of its relatively independent parts (a section of history, a tradition, an ambitious group like the group around Delbruck that started molecular biology, or even an individual): what we claim to recognize id not independent of us; what is independent of us is and remains unknowable.

This is kind of an **ontological relativism**, which is possible because Being is built accordingly: it reacts to **some** approaches, not to all, but not to others. In this sense (which I have still to work out) I would say that relativism plus realism are compatible.

How does that sound? And now I have to get back to my taxes.

Best wishes!

Paul

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[14] Enclosed was "Science without Experience", taken from P. Feyerabend, ***Realism, Rationalism & Scientific Method – Philosophical Papers Vol. 1***, Cambridge University Press, 1981, Chap. 7.