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NOTES TO POTENTIAL CONTRIBUTORS

All submitted manuscripts will be refereed either by members of the Board of Advisors — in the process of constitution at the time of publishing issue # 1 of **SORITES** — or by other specialists; as far as possible, each manuscript will be refereed by philosophers not unsympathetic to the paper's philosophical outlook or orientation.

No manuscript may be submitted if it is being considered for publication elsewhere.

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All submitted papers must be written in English. The author's local variety of English (including the spelling) will be respected — be it Indian, Filipino, Australian, American, Western-African, British, Southern-African, Eastern-African, Jamaican, etc. All editorial material will be written in BBC English, which is the journal's «official» dialect.

There is no settled length limit for papers, but we expect our contributors to stand by usual editorial limitations. The editors may reject unreasonably long contributions.

We expect any submitted paper to be accompanied by a short abstract.

We welcome submissions of in-depth articles as well as discussion notes.

Ours is a journal granting a broad freedom of style to its contributors. Many ways of listing bibliographical items and referring to them seem to us acceptable, such as '[Moore, 1940]', or '[M:5]' or '[OQR]'. What alone we demand is clarity. (Thus, for instance, do not refer to '[SWT]' in the body of the article if no item in the bibliography collected at the end has a clear '[SWT]' in front of it, with the items sorted in the alphabetic order of the referring acronyms.) We prefer our contributors to refer to 'Alvin Goldman' rather than 'Goldman, A.', which is obviously ambiguous. We dislike implied anachronisms like [Hegel, 1989]' or '[Plato, 1861]' — but you are entitled to ignore our advice.

How to submit?

(1) We will be thankful to all contributors who submit their papers in the form of [I.B.M.-PC] WordPerfect 5.1 files. There are several convertors which can be used to turn docs from other word processor formats into WP5.1 format. (Notice that with WP5.1 you can write not only almost all diacritically marked characters of any language which uses the Latin script, but moreover all of Greek and virtually all symbols of mathematical logic and set theory.)

(2) In case a contributor can neither use WP5.1 nor have their doc converted into WP5.1 format, we advise them to conform to one of the following recommendations ((2.1) or (2.2)); otherwise they can send us their file in its original format but, for the time being, we do not promise success in converting those files from other formats into WordPerfect 5.1.

(2.1) The best thing to do when WP5.1 format is not available is to use [stripped and extended] ASCII format, which means: text files (not binary ones) written using any printable ASCII characters of Code-page 437 (USA or default), i.e. any character except ASCII_00 through ASCII_31; with CRs (carriage returns) only between paragraphs — not as end-lines. Such files will here be called 'ASCII files'. We expect them to bear the extension '.ASC'.

(2.2) Another alternative (which is in itself worse, but which nevertheless may be more practical in certain cases) is to use the DOS text format, with no character outside the range from ASCII_32 through ASCII_126, no hyphenation, a CR at the end of each line and two CRs separating paragraphs. Such files will be here called 'text files'; we expect them to bear a '.txt' extension.

(3) In case (2.2) the contributor can include their paper into an e_mail message sent to one of our editorial inbox (sorites@olmo.csic.es)

(4) Before sending us their file the contributor is advised to compress it — except in case they are sending us a text file through procedure (3) above. Compression reduces disk-storage and shortens transmission time. We can extract and expand files archived or compressed with Diet, ARJ (both warmly recommended), Tar, Arc, Zip (or PKZip), GZip, Compress (i.e. .Z files), LHA, Zoo, RaR, and some versions of the MAC archivers PackIT and StuffIT.

(5) The most expedient way for contributors to send us their submitted paper is through anonymous FTP. At your host's prompt, you enter 'FTP olmo.CSIC.es'; when you are prompted for your username, you answer 'FTP' or 'anonymous'; when you are next prompted for your password, you answer with your e_mail address; once connected, you enter 'cd pub/sorites/incoming', then 'binary', and then 'put xxx' — where xxx is the file containing your submitted paper and a covering letter. (If the file is an archive, the extension must reveal the archiving utility employed: '.gz', '.Arj', '.RAR', etc. (DIETed files needn't bear any special denomination or mark; they will always be automatically recognized by our reading software.)

(6) Whenever a paper is submitted, its author must send us a covering letter as an e_mail message addressed to one of our editorial inboxes.

(7) If a contributor cannot upload their file through anonymous FTP, they can avail themselves of one of the following alternatives.

(7.1) If the file is a '.txt' file (i.e. in case (2.2)), simply include it into a e_mail message.

(7.2) In other cases, an 8-to-7 bits converter has to be used, upon which the result can also be included into an e_mail message. 8-to-7 bits convertors «translate» any file (even a binary file) into a text file with short lines which can be e-mailed. There are several useful 8-to-7 convertors, the most popular one being UUenCODE, which is a public domain software available for many different operative systems (Unix, OS/2, DOS etc). Another extremely good such convertor, very easy to use, is Mike Albert's ASCIIZE.[1] We can also decode back into their binary original formats files encoded into an e-mailable ASCII format by other 8-to-7 bits convertors, such as: TxtBin, PopMail, NuPop, or University of Minnesota's BINHEX, which is available both for PC and for Macintosh computers. Whatever the 8-to-7 bits encoder used, large files had better be previously archived with Arj, Diet or any other compressor, the thus obtained archive becoming the inputfor an 8-to-7 bits convertor.[2]

(7.3) An alternative possibility for contributors whose submitted papers are WordPerfect 5.1 docs is for them to use a quite different 8-to-7 bits convertor, namely the one provided by the utility Convert.Exe included into the WordPerfect 5.1 package. (WordPerfect corporation also sells other enhanced versions of the convertor.) Unfortunately the convertor suffers from many imperfections; owing to one of them, a separate e_mail message is mandatory in this case informing us of the procedure.[3]

(7.4) You also can submit your manuscript in an electronic form mailing a diskette to the Editor (Prof. Lorenzo Peña, CSIC, Institute of Philosophy, Pinar 25, E—28006 Madrid, Spain); diskettes will not be returned, and regular-mail correspondence will be kept to a minimum.

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[1]Mike Albert's address is P. O. Box 535, Bedford, MA 01730, USA.

[2]For the time being, and as a service to our readers and contributors, we have a directory called 'soft' hanging from our directory sorites at the node olmo.csic.es. The directory contains some of the non-commercial software we are referring to, such as archivers or 8-to-7 encoders (or 7-to-8 decoders).

[3]The procedure is as follows. Suppose you have a file called 'dilemmas.wp5' in your directory c:\articles, and you want to submit it to **SORITES**. At your DOS prompt you change to your directory c:\articles. We assume your WordPerfect files are in directory c:\WP51. At the DOS prompt you give the command '\wp51\convert'; when prompted you reply 'dilemmas.wp5' as your input file whatever you want as the output file — suppose your answer is 'dilemmas.ker'; when prompted for a kind of conversion you choose 1, then 6. Then you launch you communications program, log into your local host, upload your file c:\articles\dilemmas.ker using any available transmission protocol (such as Kermit, e.g.). And, last, you enter your e_mail service, start an e_mail to to sorites@olmo.csic.es and include your just uploaded dilemmas.ker file into the body of the message. (What command serves to that effect depends on the e_mail software available; consult your local host administrators.)

(8) Such submitted papers as are not WordPerfect 5.1 files require some preparation.

(8.1) Ours is not a logic journal, but of course one of the glories of analytical philosophy is its rigour, which it partly owes to auxiliary use of symbolic notation in order to avoid ambiguities, make matters of scope clear or render arguments perspicuous. ASCII translations of symbolic notation are problematic, especially in cases of nonclassical logics, which may use sundry negations, disjunctions, conjunctions, conditionals, implications and also different universal and particular quantifiers (e.g. existentially and nonexistentially committed quantifiers, a familiar dichotomy in Meinongian circles). While using WordPerfect 5.1 you can represent a huge variety of such nuances, it is impossible to express them within the narrow framework of text or even ASCII files (i.e. even when the 224 printable [extended] ASCII characters can be used). Still, for some limited purposes, a translation of sorts can be attempted. You are free to choose your representation, but the following translation may be a reasonable one: '(x)' for universal quantifier, '(Ex)' for existential quantifier; '&' for conjunction; 'V' for disjunction; '->' for implication (if needed — something stronger than the mere 'if ... then'); 'C' for conditional; '=>' for an alternative (still stronger?) implication; '_pos_' for a possibility operator; '_nec_' for a necessity operator.

(8.2) In ASCII or text files all notes must be end-notes, not foot-notes. Reference to them within the paper's body may be given in the form '\n/', where n is the note's number (the note itself beginning with '\n/', too, of course). No headings, footings, or page-breaks. In such files, bold or italic bust be replaced by underscores as follows: the italized phrase '*for that reason*' must be represented as '_for that reason_' (**NOT**: '_for_that_reason_'). A dash is represented by a sequence of a blanc space, two hyphens, and another blanc space.

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Madrid. April 10, 1995

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Editorial Pronouncement: In Defense of Analytical

EDITORIAL PRONOUNCEMENT:

IN DEFENSE OF ANALYTICAL PHILOSOPHY

Section 1.— The opposition between analytical and continental philosophy

As happens with so many things, contingent associations have led to what can be viewed as an ironic result. The analytical tradition in philosophy was started in the continent of Europe by a German philosopher, Frege; among its founders there is at least another German-speaking philosopher, Wittgenstein — nothing to say of many closely related philosophers, especially in the former Austro-Hungarian Empire, who directly or indirectly gave a powerful contribution to the formation of the analytical tradition: Brentano — even Bolzano —, Meinong, the Vienna Circle, Lukasiewicz, Carnap, Gödel, etc.

No metaphysically necessary link exists between the Anglo-Saxon world and the analytical tradition in philosophy. In fact at the beginning of the 20th century the philosophical life in both England and the US was under the sway of tendencies whose affinity with analytical philosophy is far from obvious and which were definitely rejected and abhorred by Russell and Moore when they embarked on the analytical voyage (even if nowadays our assessment of Bradley, Bosanquet and co. Would be much less flippant).

The current situation is rather confusing. For whatever historical reasons, the philosophical tradition which uses the method of definitions-and-argument — a method developed with an enormous rigour by the Scholastic philosophers in the late Middle Ages and the Renaissance and Baroque period — goes by the name of 'analytical'. Its opposite does not go by the name of 'synthetical' (the misnomer would be outrageous and grotesque), but that of «continental», i.e. belonging to «the» continent. No need to be fussy here about what that continent is, whether it encompasses Calcutta, Peking and Teheran, or perhaps also Bamako and Maputo, or whether the Urals constitute a «natural» demarcation line. From our view-point it is more interesting to find out for how long and to what extent such philosophy as has been developed in France, and Germany, and the Netherlands, and Italy, and so on, has been «continental». Orthodox or quasi-orthodox Marxists (such as Lukács) were clearly non-continental in character and style. Nor is it easy to count as continental the philosophical output of thinkers such as Husserl and most members of his phenomenological school, Nicolai Hartmann, Maurice Blondel, Benedetto Croce. It is even unfair to look upon Bachelard, Ferdinand Gonseth and other French-speking thinkers much in favour until 1950 as really, truly «continental». Thus after all «continental» philosophy could be roughly characterized as such sort of philosophy

as has prevailed since the end of the second world war in France, Germany, Italy ... And... (A purely enumerative stipulation here).

Unfortunately, what thus emerges is not a coherent trend. What is more: the picture is no longer accurate. Analytical philosophy has for many years hold a number of bridge-heads in continental Europe (in the Benelux countries, Switzerland and Austria, for instance), but it is now on the ascent everywhere. The disappearance of the third-party of orthodox Marxism may be one of the reasons, but there are many others. One of them may be shear fashion, or infatuation (which is really no explanation after all). Another reason may be the growing influence of whatever comes from the Anglo-Saxon world, good or bad. Probably, though, continental philosophy is in a much deeper crisis and trouble than analytical philosophy. When even watered-down rationalistic aspirations are dourly given up and all semblance of clarity is jettisoned, you can be sure people will soon begin to look after other paradigms.

And what about another third party, an independent third world philosophy? Much has been said about an indigenous African philosophy, or about a genuinely autochthonous Latin American philosophy; they would share neither the methods nor even the subjects of Western or European philosophy, and the very term 'philosophy' would apply to them in an entirely irreducibly idiosyncratic sense. As far as we know, what little has come from such schemes has been an adaptation of this or that style of Euro-continental philosophy. Furthermore, such philosophical nationalisms seem to be on the decline.

Not that everybody has been happy with the choice of being either an analytical philosopher or a continental one. Neo-neo-Scholastics can look upon themselves as neither. Yet more often than not, either their style is so reminiscent of that of the Scholastics of yore, so close to that of analytical philosophers that the latter view them as close relatives, or on the contrary it is so suffused with «end-of-philosophy» or «post-metaphysical» style that they wouldn't be unwelcome in such continental circles as are not completely narrow-minded.

However, may people loathe such an enforced choice and endeavour to build bridges. We wish them good luck; we do, indeed! It would be so nice to be able to go into Jaspers's implicit arguments, to consider whether such or such a premise in one of them has been cogently argued for or how to improve on the argument or how to find another not entirely dissimilar to the same effect!

But ours is not an ecumenical enterprise. We feel committed to a strong analytical attachment. On the other hand, we find some residual justification for the continental's complaints about analytical trifles (more on that below). Suppose you are backing up Locke's theory of the legitimacy of private ownership through labour. That's very interesting — even exciting and by no means nugatory. But, alas, a weakness emerges in the second premise of your sixth argument, which gives rise to a huge secondary literature. A minor item in that literature is also open to criticism, triggering a profusion of refutations, replies, counter-replies, and a bulky tertiary literature. And so on. That is a caricature, of course. But we do not like our now newborn journal to become a repository of such a kind of academic exercises. Not that discussion notes are ruled out — quite the contrary is true. But we hope that the bulk of each issue of ***SORITES*** will be concerned with matters of substance. We want to show — as do many well-established publications — that «analyticity» in

approach is compatible with a broad scope and importance of the subjects dealt with. Analytical philosophy is not «analytic» (or «un-synthetic») philosophy.

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Section 2.— Analytical, not analytic, philosophy

Whether there is a dichotomy of analytic and synthetic statements, what in principle or initially those terms purportedly mean seems — at least *prima facie* — clear, namely: an analytic statement is one which performs or displays an analysis of the subject by finding the predicate as a part thereof. (The source of such ideas was Leibniz through Kant.)

It is no mere coincidence that analytical philosophy is so-called. In fact, a number of analytical philosophers have thought, and still think, that a major area of philosophical interest is something like conceptual analysis, and thus asserting analytic statements. That such a view gives rise to the paradox of the analysis is not our present concern. What we here want to emphasize is that analytical philosophy as a whole is by no means opposed to «synthetic philosophy». Analysis is no special method or feature of analytical philosophy. The school of conceptual analysis is just one among the very many flourishing schools within the broad domain of analytical philosophy. What is more, there are grounds to suspect that the days of glory of the school of conceptual analysis have been long past. We are not discouraging a resurgence of the school — we are convinced that deeply motivated philosophical tendencies never die and that their renewal may be fruitful and stimulating. We go further than that in recognizing what probably all of us, analytical philosophers, owe much to the school of conceptual analysis. (Quine himself can be read as frequently indulging in conceptual analysis, and after all such is the case each time a philosopher claims that, unless such or such thesis is countenanced — or alternatively withhold — no sense can be made of the use of a certain word.)

Be it as it may, such «conceptual analysis» is neither a necessary nor a sufficient condition for membership in the analytical philosophy community. It is not necessary, since a staunch rejecter of the analytic/synthetic dichotomy will have no use for conceptual analysis (except perhaps as a quasi-rhetorical way of recalling certain pretty obvious truths without thereby renouncing his general view that even such sentences as we take to be the most obviously true may turn out to be false in the face of a recalcitrant experience). It is not sufficient either, since such ways of arguing are commonly resorted to in all schools of thought — in so far, at least, as arguing is not entirely dismissed as a way of doing philosophy.

There seems to be only one feature making up the hard core of analytical philosophy: it is the argumentative way of doing philosophy. Nonanalytical philosophers may differ among themselves in their respective degree of argument-abhorrence. A few among them take themselves to pursue philosophical inquiry as a rational, argumentative task. Analytical philosophers are likely to find their attempts unconvincing in so much as their arguments are found fault with on count of obscurity or looseness — with inference rules quietly left in the background, and the inferential structure either veiled or muddled or in some cases plainly wrong. However such charges are extremely recurrent within the analytical philosophy community itself. Withal, those drawbacks are matters of degree. Thus, in so far as a philosopher engages in something which may reasonably be looked upon as a

genuine, if imperfect, argumentative kind of thought, he deserves to be welcome to the analytical philosophy community.

The design of limiting philosophy to what can be pursued as a rational, argumentative enterprise has been objected to on the ground that reason is not enough. Since to reason is to infer, a purely rational inquiry will be able to draw conclusions from premises, but will be constrained to resort to unproved premises, premises which are thus not secured or attained by reason but through insight, or «judgment» or perhaps rule-free meditation or the like.

Although Aristotle held some such views, it is doubtful that he fell back on «insight» as a method of philosophical inquiry. Be it as it may, we tend to think that philosophers are doing philosophy only when they are arguing, not when they are taking something for granted on the basis of their «intuition», or their «insight» or their [reasonless] meditation or anything of the sort. Not all a philosopher does is philosophy — not even each process of thought he engages in when he goes about philosophizing.

When a philosopher argues «p_1, ..., p_n; hence q», his premises may fail to be philosophical or philosophically arrived-at, but the whole reasoning may count as philosophical all the same. After all each of us takes quite a lot for granted at any particular moment.

Yet, philosophizing analytically — in that sense — is compatible with doing grand philosophy in the old style. Philosophers have always been as good as humans can possibly be at casting doubt on their own enterprise — or at least at undermining it. Self-immolation for the sake of taking the philosophical scrutinizing and criticism a step forward — making it into self-criticism — has been a hallmark of philosophy since time immemorial. Such a fanatical passion for reason has sometimes become irrational — unreasonable. Probably the root of the suicidal fury has been the all-or-nothing rule. Anyway, even since Kant — in some sort of way since Descartes — certain outstanding philosophers and hosts of retainers and continuators have heralded the end of grand philosophy in the old style — the end of metaphysics in particular, where ‘metaphysics’ would be any intellectual inquiry beyond a sharply drawn boundary of licit research.

Although most members of the analytical community would nowadays consider Frege and Russell the founders of the «movement», it is true that for a number of years or decades the most influential analytical-philosophy schools were those of the Vienna Circle, neopositivism, logical empiricism, and antimetaphysical linguistic analysis — under the influence of Moore and the latter Wittgenstein. It is ironic that, whereas outside analytical philosophy metaphysics have died down — both in name and in word —, and even such ontological systems as Nicolai Hartmann’s or Blondel’s are no longer in favour at all, within analytical philosophy the opposite has happened: although positivistic mistrust towards too systematic constructions or towards raising «ultimate issues» has never disappeared — and surfaces from time to time —, inquiry into metaphysical matters has become more and more popular and fashionable, with lots of discussions going on about the difference between necessary and contingent truth or existence, the reality of universals, individuation, identity, the structure of facts, the nature of space or time, whether there are or not categorial differences in the world, and so on. New ontological issues, such as supervenience, have become possible thanks to a development both of logic and of inquiry into modal metaphysics.

Likewise, theory of knowledge has also flourished in a way that positivism had endeavoured to thwart and banish. Both nonrealism and realism — whether as a full-fledged metaphysical realism ir in some milder variety — are vying for widespread acceptance within the community.

Nevertheless, we feel bound to give the devil his due. Unfortunately it is true that very often analytical philosophers focus on minutiae and lose sight of major issues. Such a rebuke is neither wholly baseless nor entirely fair. The analytical philosophy community can pride itself on having given birth to important philosophical systems — e.g. modal realism, noneism (one of the neo-Meinongian schools), Quine's holism or Castañeda's system, to mention but four of them.

We fail to see any cogent reason why analytical philosophy cannot go on producing new grand systems of philosophy. Moreover, there is no reason why general philosophical systems cannot arise within the analytical philosophy community. In fact, as many analytical philosophers have stressed, different philosophical — and even nonphilosophical — fields are linked by deep inferential connections. Any approach in ethics or philosophy of law can be argued to rest on implicit or explicit metaphysical and epistemological assumptions. On the other hand, metaphysics and theory of knowledge are not as neutral on ethical or political issues as might appear at first sight — a denial of identity through time may fail to be ethically innocuous or immaterial. Thus, it seems worth-while to set up wide-scope philosophical approaches. An approach of that kind considers all general fields of philosophical study and pursues the inquiry in each of them holistically taking into account what it has proposed or is going to propose in all the other fields. Thus, such an approach can reject identity through time on the ground that the thesis fails to explain our uneasiness concerning the [purported] right of everybody to wrong himself as he pleases (for, clearly, without identity through time there is no single continuing entity which both performs the wrong now and suffers from it later). This is a mere example, of course. Infinitely many inferential links can be established between different fields. All of them may be legitimate (we needn't share Hume's qualms over alleging in support of a claim that a denial thereof would entail practical dreary consequences — provided we do not boast to have proved more than we have, and by the way remember that *a person's modus tollens is another person's modus ponens*).

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Section 3.— Our tasks, goals and means as co-workers in the analytical-philosophy community

In a global society as the human collectivity is becoming nowadays, the overcoming of regional barriers and boundaries is increasingly compelling. Paradoxically, more often than not such overcoming as takes place leads to a hardening of the remaining frontiers, which thus tend to become impervious barriers, impassable walls. New regional blocs are organized with the less fortunate ones being let down in the ensuing jostling.

We think that the human collectivity needs global solutions the current difficulties it faces. From our modest philosophical perspective we hope we can contribute something of value to that end: (1) by spreading the good way of coping with theoretical issues in any field — through reason; (2) by promoting world-wide cooperation and exchange in our own domain — philosophy; (3) by broaching — in

our own rational, argumentative way — hot issues in applied philosophy which are of concern to everybody; (4) by encouraging contributions from all continents and from any background compatible with our analytical standards of argumentative rigour and intelligibility.

What are those standards? We do not want to impose our own views of what is to count as analytical philosophy; still less to promulgate peculiar standards of analyticity which are as contentious and debatable as anything else. Nor are we, the editors, necessarily of one mind on such issues — or on any other issue. Ours is a pluralistic enterprise. Nevertheless, we feel bound to outline three loose criteria on what papers we are going to regard as genuinely belonging to the analytical-philosophy line of the journal.

(1) Standards of clarity. As far as possible, use words people understand and use them with their ordinary sense and syntax. When departing from that rule, justify it and define the technical usage. If compelled to coin neologisms, justify the procedure and spell out their meaning as clearly as possible.

(2) Standards of argumentativity. Prove as much as possible. When arguing, tell the readers what inference rules you are relying on and how those rules support the cogency of your argument. Put forward your arguments in such a way that the inferential patterns are revealed. Justify the inference rules themselves, as far as possible. As far as possible, keep clear of appeals to intuition.

(3) Standards of scholarship. Take into account what other authors in the analytical tradition have written on the subject you are dealing with. Refer to [some part of] the relevant literature.

What's the rationale for choosing those standards rather than others? The philosopher starts doing philosophy out of a common lore, as does science. That common lore may be called 'common sense'. There is no infallibility about common sense, of course. Many errors as well as many reasonable approaches to the truth are contained therein. Yet, the philosopher's whole enterprise is bound to be pointless and doomed to fail unless such a starting point is more or less acceptable in so much as it provides a language through which truth and reality can be accessed, however precariously or imperfectly. Also it must provide some rough criteria of proof or demonstration, which of course have to be polished, distilled and improved upon. Logical argumentation is nothing else but a refinement of customary ways of reasoning. Finally, scholarship is just a development of the usual requirement that more eyes see more than fewer eyes, and so that we are well advised to listen to what other people have had to say — when they have been looking after the truth in a rational, argumentative way — instead of turning a deaf ear on their arguments and proposals.

Thus analytical-philosophy standards are common-sense standards refined and developed. Unless such standards are, more or less, correct, our very starting point was confused or misleading and our whole philosophical enterprise is likely to lead either nowhere or to a sorry end, full of massive error or worse, to shear nonsense. Admittedly, that argument does not show that our enterprise is correct, or that its goals are worth pursuing. We may be in deep and wholesale error. Our philosophical enterprise may be fated. Or some other, nonrational, ways may be opening bright prospects.

However, since justification is perhaps relative, if the common lore out of which we start our philosophical inquiry is thoroughly misguided and wrong, all the odds are that any purported brilliant idea about a new method, a path of insight or intuition rather than reasoning, should be just an additional error. For, what may seem to be an emancipated idea, springing from an unpolluted source of insight, is likely to emerge, upon reflection, as just a continuation of an old procedure in a new guise — a procedure rooted in our pre-theoretical ideas, and so in our common-sense views, at some remove. On the other hand, even though we cannot radically, or fundamentally, justify our ancestors' common lore, which has nurtured our philosophical enterprise, we can find partial justifications, we can continue our quest for justifications. And no other tool seems to come our way or to be open to public assessment and discussion except rational inquiry, i.e. a logical approach. (We are not excluding informal logic, nonclassical logics, nonmonotonic logics, and so on. We say 'logic' taken in a broad sense. But not so broad that it can embrace anything whatsoever, with an astrological logic, a queasiness logic, a logic of emotions or feelings. Boundaries are fuzzy, but they exist, somehow.)

A possible continental rejoinder would be that there may be legitimate alternatives to reason, or to truth; or that there may exist other ways of reasoning, which may turn out to be more conducive to truth, or to whatever is deemed valuable for philosophers to look after; in other words, that logical ways of reasoning, endorsed by analytical philosophers, may be just contingencies, rooted in cultural particularities of the so-called 'Western' world, or of the Greek civilization to which we still belong.

Maybe. Yet, it is hard for those who espouse such views to put them forward and endow them with plausibility unless they resort to that very same ways of reasoning they regard as contingent peculiarities of a certain civilization or of a particular tradition. Are they compelled to such a choice merely out of courtesy in order for us, analytical folk, to understand what they have to say? More probably, a sort of transcendental argument — of which some continentals are fond — may be developed. They have the choice of either depriving their proposals of cogency or plausibility altogether, or else arguing in the customary, logical sense. Insomuch as they fall back on argument, what they are doing is no longer continental thought; it does not sound continental, it does not bear the continental hallmark, it has lost the continental ring.

Our reply is not a knock-down argument. Only a few people nowadays cleave to the old foundationalistic hope of providing a secure, assumptionless, ground for our whole epistemic enterprise, through which our philosophical arguments could become perfectly conclusive, dispelling and refuting errors definitely and forever. Nevertheless, our argument shows that continentals face a very hard and unenviable task if they want to convince people, in a rational way, of the worth of what they are after.

Nor is much evidence in support of the so-called cultural relativity or contingency of reason, or of logical reasoning. Lévy-Bruhl's thesis of primitive peoples' pre-logical mentality can no longer be considered a good argument for such a relativity. Quite apart from the fact that such an anthropological view is not in much favour nowadays, the essential point is that, with the recent development of paraconsistent logics, we know for sure that a system of beliefs containing contradictions can yet be logically defensible, that people espousing such a body of

beliefs can reason in the same way as other people do, with only a few — and perhaps marginal or minor — inference-rules being omitted, such as disjunctive syllogism. In fact there is no hard evidence in support of the Western monopoly of reason at all. More probably than not that monopoly was a colonialist fable, which today has ironically become a purportedly anti-establishment myth. (Purportedly, yes: we are aware of no evidence backing up the claim that analytical philosophy is socially conservative and that, against it, supporters of anti-establishment causes, such as gay movements, feminists, non-Western folk, and so on, have to resort to other ways of thinking, outside the pale of reason, or else to other ways of reasoning outside logic; we ask those who advocate such brash views to convince us, at the very least displaying some sort of statistical-inquiry results to the effect that nonconservative persuasions are more frequent among continentals than among analytical philosophers.)

Let us bring this section to a close by stressing that there is no clear-cut, sharp, crisp boundary between analytical philosophy and nonanalytical thought. Some analytical philosophers are as much relativists and truth-deniers as the most immoderate continentals may be. Far from assuming a well-established rational order out there or a logical pursuit of truth, they spurn truth altogether. They are likely to be considered iconoclasts; probably not many philosophers are prone to accept such proposals or to take them seriously into account except in order to refute them. Yet, their way of arguing is analytical — they try to bear it up with logically well-constructed arguments. On the other hand, there are philosophers who are not usually taken to be analytical but whose writings are close to analytical standards, at least in a broad sense. And there are potentially infinite degrees between purely analytical reasoning and the kind of obscure prose — bereft of arguments in any recognizable sense — which is so characteristic of some outstanding continental writers.

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Section 4.— A Balance between Theoretical and Practical Philosophy

Ours is a general philosophy journal. We intend to keep a balance between issues in theoretical and practical philosophy. We expect most of the articles will deal with metaphysics — including regional ontologies (philosophy of nature, of mind, language, and so on) —, theory of knowledge and similar fields. No philosophical enterprise deserves the name unless it gives pride of place to first philosophy (not a philosophy which is necessarily «first» in a foundational sense — something most philosophers do not believe in nowadays). Yet it is also certain that no philosophical enterprise is worthy of the name if it shrugs on practical matters. We feel committed to applied philosophy understood as a philosophical elucidation of matters of concern for the life of members of our species and other higher animals, particularly as such a life is regulated by publicly established rules or by political decisions. Philosophical elucidation can shed light on common assumptions, expose fallacious arguments, find out ontological implications of relevant proposals or even envisage courses of action rendered possible upon an abandonment or a qualification of certain logical or ontological assumptions.

This is why we welcome submissions on applied-philosophy issues such as: (1) bioethical issues (ranging from assisted suicide and euthanasia to abortion, genetic engineering, inter-species interbreeding, etc); (2) political matters (including paradoxes of self-reference in constitutional law, or conflicting principles constraining legitimacy); (3) juridical concerns (e.g. the nature of culpability,

constraints on licit contracts, whether the law's empire is wishful thinking, etc); (4) environmental issues (what are the rightful claims — if any — of future generations against those now living, or how to reconcile a quality-of-life enhancement with environmental conservation); (4) third-world (and related) issues, such as the right to migration, the duty of the well-to-do to pay compensation for past wrongs (slavery, e.g.), reverse discrimination, meritocracy, the crisis of legitimacy in generalized-corruption situations, whether free-market mechanisms can yield the promised results of widespread prosperity, or the value and justification of existing boundaries.

This last subject can in a way epitomize all our concerns and purposes. We feel that a philosophical elucidation of the single issue of boundaries may be the core both — perhaps — of philosophical investigation, and anyway of philosophical concern today. We invite our potential contributors to submit papers dealing with: what boundaries are; how or where boundaries or demarcation lines can be drawn in a justified way; to what extent — and for what purposes — such boundaries are really or morally binding.

We refer to boundaries in all fields: in the application of words, in the geographical separation of collectivities, in the establishment of areas of inquiry, in laying down historical «periods», etc. We intend to honour what our journal's name has come to mean in the philosophical tradition — a process through which boundaries are little by little eroded, pushed, shifted, until in the end they seem to have vanished into thin air — or to be much less absolute than they used to be. Soritization is not going to solve all of our problems and difficulties at low cost — still less at one fell swoop —, but it can turn out instrumental in the quest for adequate solutions.

Our allegiance to a combination of pure and applied philosophy is compatible with our viewing **SORITES** as a journal whose main audience is the multitude of people educated in academic philosophy as practised by the professional analytical community. Ours is neither an interdisciplinary publication nor a general-readership journal. All papers seriously considered for publication in **SORITES** will be written from a philosophical perspective by authors both acquainted with the philosophical techniques of argumentation and familiar with current debates in analytical philosophy. (We know there is no shortage of journals which follow opposite lines and which may welcome papers by those who want to put forward their ideas or proposals from backgrounds or view-points which do not conform to our guidelines.)

Having said that, we proceed to stress that no impassable frontier is going to enclose **SORITES**. No issue is ruled out once and for all, provided it turns out to be philosophically relevant and is brought up from a philosophical perspective with analytical rigour. Thus take, for instance, a domain which has been claimed to be a preserve of the «continentals», viz. the critical examination of the philosopher's nonphilosophical background. We are aware this is as thorny, formidable issue. Yet, any attempt at coping with it is welcome — provided it is no facile, hollow claim lacking evidential support. We incline to think that what has caused analytical philosophers to keep clear of an issue like that is not a purported ignorance of the philosophizing person, a purely objectivistic concern or the like, but precisely the fact that hitherto rational discussion on those issues has not materialized. (Which means that mustering and displaying of evidence, assessed with publicly available criteria, has not emerged yet.) Whether or not biography can be incorporated into philosophical discussion is a question on which we want to remain open-minded,

ruling nothing out. What we do not accept, in the absence of comparative induction, is that a biographical story explains anything about the philosopher's thought — still less that it either bears out or discredits the philosopher's opinions.

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Section 5.— Editorial Policy

There are many excellent printed publications in analytical philosophy. However the mass of outstanding, superb material which remains unpublished grows faster and faster each year. On the other hand, the computer revolution in communications is only just beginning to change the established or institutionalized patterns of cultural transmission. We tend to think that Gutenberg's revolution in the 14th century was small, almost insignificant, as compared with the telematic revolution at the end of the 20th century.

Paraphrasing a famous claim by Marx, we hereby assert that the means through which cultural exchange is pursued may become its fetters. There is a probable elitist objection to our view, namely that the replacement of printed paper by telematic channels may bring about such a multiplication of available material that our lives will only appear the more pitifully short; or that such an accumulation will lower the standards and let the noise in.

We think similar considerations could militate against Gutenberg's revolution. We must live with the new enhanced technology and learn to be the better-off thanks to it. Our species and our civilization are resilient enough to discharge the task successfully.

On the other hand, the new electronic means of expression are going to provide more opportunities to many authors and many manuscripts. When the ratio between published and unpublished manuscripts is 1/10 or less, rational selection becomes increasingly problematic and doubtful, with prejudice being resorted to by editors and referees, even if they honestly try to be fair. Prejudice may take many forms, and of course we all are prejudiced, to some extent or other.

We hope, though, that **SORITES**, an electronic philosophical journal dedicated to the crossing of boundaries — along with many others which have already arisen or will arise soon —, is going to close the gap, thanks to which prejudice will become less deleterious. In particular, our journal, at the crossroads of Europe and Africa, will try to further philosophical exchange between both the Northern and Southern «blocs» or «banks», warmly welcoming submissions from third-world countries. If we attain success in that, we'll have at least do something good in our soritizating enterprise.

Having said that, we must make it quite clear that our procedures will be those which are standard in the academic community. Every submitted manuscript will — unless the editors consider it unsuitable for publication in **SORITES** for reasons of content, or style, or language — be refereed either by members of the Board of Advisors or by other specialists; as far as possible, each suitable manuscript will be refereed by philosophers not unsympathetic to the paper's philosophical outlook or orientation.

No manuscript can be submitted if it is being considered for publication elsewhere.

Once accepted, papers may not be printed without the previous consent of **SORITES**.

All submitted papers must be written in English. The author's local variety of English (including the spelling) will be respected — be it Indian, Filipino, Australian, American, Western-African, British, Southern-African, Eastern-African, Jamaican, etc. All editorial material will be written in BBC English, which is the journal's «official» dialect.

There is no settled length limit for papers, but we expect our contributors to stand by usual editorial limitations. The editors may reject unreasonably long contributions.

We welcome submissions of in-depth articles as well as discussion notes.

Our «official» word-processor is WordPerfect 5.1, but everybody will have a fair opportunity of contributing to **SORITES** even without WordPerfect at all. Each issue of **SORITES** will be available in more than one format, i.e. at least in an ASCII format over and above the WordPerfect 5.1 format.

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Natural Kinds and Projectible Predicates

NATURAL KINDS AND PROJECTIBLE PREDICATES[1]

Axel Mueller

1.— Introductory Remarks

In this essay I want to approach two — at first sight not immediately connected — themes:

1.) Goodman's Paradox, i.e. a problem usually associated with the justification of induction or the conditions of confirmability of hypotheses, and

2.) some traits of the application of the so-called «natural kind terms» as they have been postulated by proponents of the theory of direct reference, i.e. theses and problems usually associated with the interpretation of possible world discourse and/or metaphysical questions as to «metaphysical realism» and essentialism.

Do these two problem clusters intersect in any sense at all? One intention of the following reflexions consists in an attempt to answer positively to this question. This might not seem to much a dare, as Goodman himself pointed out the connection between counterfactual conditionals, lawlikeness of generalizations and the problem of the characterization of projectible predicates, as well as Putnam always insisted in the «theoreticity» of natural kind terms, that is, understood them in the sense of the predicates which are used with more or less success in confirmation — and induction-impregnated practices. Nevertheless there is little more than hints in the respective direction from either side. So Goodman says that to entrench a «class of objects» and to entrench a predicate is more or less the same[2] and adds, in the part with the title «Survey and speculations»: «Our treatment of projectibility (...) may give us a way of distinguishing 'genuine' from merely 'artificial' kinds (...) and thus enable us to interpret ordinary statements affirming that certain things are or are not of the same kind (...). [S]urely the entrenchment of classes is some measure for their

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[1] Final form of this paper has benefited very much from the minutious reading and discussing of a first draft done by the editor of this journal, Lorenzo Peña, as well as from a discussion group in the Spanish Institute for Advanced Studies (CSIC), Madrid (Spain), formed by Carlos Thiebaut, Cristina Lafont and Lorenzo Peña. Thanks to all of them. Working extensively on this paper, which is a draft of the line of argument of my dissertation, was possible thanks to a grant of the Studienstiftung des deutschen Volkes.

[2] Cf. Goodman, N.: *Fact, Fiction, and Forecast* [in the following FFF], Hassocks

genuineness as kinds; (...) An adequate theory of kinds should in turn throw light on some troublesome questions concerning the simplicity of ideas, laws and theories.» (p.122-3)

Putnam says that to stay with a predicate and to treat two theories with different characterizations of its reference-class as successors, i.e. phases of one and the same global theory, is virtually the same[3]. On the other hand there has been a considerable progress in the theory of reference concerning natural kind terms, which has not yet had its due resonance in confirmation-theory[4]. Two contingent historic facts might have prompted this situation: first there is the unhappy divorce of epistemology and metaphysics and the subsequent dismissal of epistemological concerns promoted by Kripke and the theorists of direct reference mainly interested in ontlogical questions. On the other hand we have the implicit or explicit assumption of the unintelligibility of possible world discourse as «intensional» and the subsequent assumption of insignificance concerning the results of «natural kind term theory» of theorists of science interested in questions of confirmation theory. My impression is that both a priori rebuttals are unjustified. One need not accept the Kripkean essentialistic self-interpretation of reference theory (with natural kinds as real essences which dictate us what ontological commitments to make, assuming the truth of our theories) to accept its pragmatic and normative, as well as its purely linguistic imports[5]. And one need not become an ontological or epistemological sceptic when one accepts the

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[3] p.95: «The entrenchment of a predicate results from the actual projection not merely of that predicate alone but also of all predicates coextensive with it. In a sense, not the *word* itself but *the class it selects* is what becomes entrenched, and to speak of the entrenchment of a predicate is to speak elliptically of the entrenchment of the extension [=reference, A.M.] of that predicate.»

[4] Exceptions to this can be found in the works of J.Leplin concerning his concept of «methodological realism» (see fn47) and S.Blackburn *Reason and Prediction*, Cambridge MA 1973, ch 4, who gives a realist account of Goodman's paradox.

[5] This has been demonstrated by interpretations of this theory given by H.K.Wettstein «Demonstrative Reference and Definite Descriptions» in: *Philosophical Studies* 40 (1981), 241-57, «Has Semantics Rested on A Mistake?», in: *Journal of Philosophy* 83 (1986), 185-209; «Cognitive Significance Without Cognitive Content», in: Almog, J. &al. (eds.): *Themes from Kaplan*, N.Y. 1989, 421-454, «Turning the Tables on Frege or How is it That «Hesperus is Hesperus» is Trivial?», in: Tomberlin, J.E. (ed.): *Philosophical Perspectives* 3: *Philosophy of Mind and Action Theory*, Atascadero (Cal.) 1989, 317-39, and N.U.Salmon («How *Not* to Derive Essentialism From the Theory of Reference», in: *Journal of Philosophy* 76 (1979), S. 703-725, as well as *Reference and Essence*, Princeton 1981 and «Reference and Information Content: Names and Descriptions», in: Gabbay, D./Guenthner, F. (Eds.): *Handbook of Philosophical Logic*, Vol. IV: *Topics in the Philosophy of Language*, Dordrecht 1987.

deepness of the problems of underdetermination raised by the discussions in confirmation theory by philosophers like Goodman and Quine[6].

To avoid these consequences and to keep the respective theories might seem to most of the philosophers of either part tantamount to drop the theory: direct reference without essences and necessary truth is like underdetermination without ontological relativity and incommensurability, as it were.

But there are always other possibilities apart from dogmatism.

There is, for example, a quite modest, pragmatic hypothesis which has been put forward by philosophers like Dagfinn Føllesdal and Keith S. Donnellan since the sixties, and there are Putnam's attempts to combine a critical epistemological attitude with a pragmatically biased modest realism stemming from or localizable in certain reference-theoretic assumptions. My attempt in this paper is to contribute some more programmatic considerations to this program. The basic idea consists in taking the theory of the direct reference of natural kind terms as an answer to the problems raised by the radicalization of underdetermination. In Putnam's case this switch from scepticism as to reference to an argument very much like 'if (1) there is no principled way to reduce the meaning to any epistemologically priviledged basis, (2) meaning is a matter of intratheoretical structure (interrelations of signs) and (3) meaning should determine reference, then non-(3) meaning does not determine reference, thus (4) reference being relatively independent from intratheoretical «meaning», so we have to provide an alternative account of reference' is evident. In this argument, as we see, there is no refusal of underdetrmination: (1) is entirely accepted. Neither is this possibilitated by a new foundationalism: (2) is accepted, thus (4) does not mean that reference is entirely «theory unloaded», i.e. independent of any theory, but there is no one theory which (now or in the future or in a world described by «necessary truths») determines reference. Reference is thus rescued to be the complicated thing it is: as the concept which serves to explain the relation between theory, understanding and the objects described, and is not determined by anything, factual or counterfactual, without reflexion on side of the users of theory. It is, in other words *supposed*. On the other hand, (2) prevents us from becoming Milleans and divorce theoretical terms from our understanding of them and their place in theories: intratheoretical reduction and definition is thus vindicated as a legitimate possibility, so that there are no grounds to suspect that what is being worked out is something like the «furniture of the world». (3) is, after all, quite a modest modification (although it goes right to the heart, one should add).

Taking this as an example, in the following I want to adventure the following ideas:

The conditions for and presuppositions (or commitments) of the adequate use of empirically interpreted predicates made explicit in the theory of the reference of natural kind terms coincide largely with the desiderata for a solution of Goodman's paradox[7]. I assume, in other words, that the referential anomalies resulting from

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[6] The chief example of this attitude seems to be Putnam, although he as well should count as one among the theorists named before.

[7] This is to say, I haste to add, that I neither pretend to give a solution (because there is none) nor to abund in the theory of identity in modal logic.

«intensionalism» detected by Kripke, Donnellan and Putnam are *not only* and *not unseparably* such of the interpretation of formulae of modal logic, and that the Goodmanian anomaly ist *not only* one within the framework of confirmation theory and the theory of valid inductive method. In contrast to that I would propose to see both of the mentioned disciplines as «*contexts of discovery*» of one or more underlying, principal problem(s) for the philosophy of language as such which challenges certain ways of transforming old philosophical problems in problems of the philosophy of language. Thus I think that the metaphysical problems stemming from the discussions in the theory of direct reference are reinterpretable (even if this might be exactly what their proponents do *not* wish to do) as parts of answers or proposals for understanding the (normative-apriori) conditions for the justification and «normal» application of predicates within inductive practices which we always have to buy if we do use them in the «normal» way, i.e. assume inductive validity for our inferences from data. That is: they may be «internalized» and be seen as a description of the realism which guides us as long as we *use* the terms. On the other hand, Goodman's paradox might be seen, as I think, as a critical obstacle to a metaphysical hypostatization of *the* world, i.e. to the reification of something normative which is operative *within* our practices: it shows that we, as soon as we reflect upon these conditions, get to see that they always could be otherwise and that there is no ontological or otherwise *guarantee* for the correction of our conceptual schemes. We have to be realists to pursue the aims of science but we are not damned to live in one specific world and could not be so.

In short: I want to argue for a «deflationist» reading of the theory of direct reference combined with an «inflationist» reading of Quine-type (or, in general: instrumentalist) scepticism concerning the ontological import of theoretical concepts respectively the epistemological importance of the theory of direct reference. I understand this as a part of the elaboration of a concept of «world» or «reality» which helps us understand the rationality incorporated in the methodology of certain enterprises, like science. Thus both modifications could, at least as I hope, contribute to an elucidation of the ontological and epistemological premises which are operative in our use of language with empirical import[8].

These are two defects which I want to be clear about from the beginning; they are due to the general character of the theses I want to put forward: they should be valid, I think, for every account of identity through possible worlds, because they do not concern the concrete structure of
an assumption of sammeness of kind as such but its place and unavoidability in certain practices. I suppose that the most natural reading of the following results from the assumption of a modified Kripke-semantics for possible world like the one proposed by Deutsch in «Semantics for Natural Kind Terms», in: *Canadian Journal of Philosophy* 23/3 (1993), 389-412, and his improvements in «Semantic Analysis of Natural Kind Terms», in: *Topoi* 13 (1994), 25-30. However, as I said, the concern of this paper is less in semantics proper than in pragmatics.

[8] We can find witnesses for this suspicion on both sides. Thus H.K.Wettstein thinks that you simply miss the point of the theory of direct reference if you look for it exclusively in its aptness to formalize metaphysical speculation or in its contributions to the clarification of the interpretation of

The first part of my thesis is that one can obtain the most important results independent from the presupposition of a metaphysical realist interpretation of the modalities because modalities are not all that matters to epistemological matters, as all the world agrees. On the other hand, and this is the second part of the thesis, a pragmatic interpretation of the structural properties assigned by this theory to the use of empirically (or otherwise objectually) interpreted general terms can provide us with a non-naturalistic description of the characteristics of a possibility to use language which is of priviledged importance in contexts where we are primarily interested in learning from experience.

2.— Aspects of the theory of reference for natural kind terms: some remarks on the conditions for a distinction between «normal» general terms and «natural kind terms»

If one views the reference of a descriptive general term as given by a necessary and sufficient condition of its application stated in other terms than the general term in question (i.e., normally a description), there is room for a conflict between the satisfaction conditions associated with the condition for application and the reference of the term interpreted through it. In certain contexts both seem plausibly to be not completely substitutable. Thus if you determine the reference of the term «gold» with a description of the form (1) «something is gold iff it is F, G and H» and affirm (2) «It is possible that gold is not F» (e.g. on aposteriori grounds or in a thought experiment) then you get by substitution the inconsistent result that (3) «It is possible that what is F, G and H is not F». Nevertheless it does not seem that by your modal remark you construe any impossible or grammatically or logically false nor absurd affirmation. This would be trivially the case, of course, if you view (1) as a definition in the strictest sense of the word. In that case eliminability is carried through in virtue of the fact that (1) is an adequate definition (i.e. provides eliminability and non-creativity in the language where it occurs and is held true), and consequently (2) is inadmissible in a language where (1) is true. So avoiding (3) is possible by adopting an aprioristic point of view concerning the descriptively fixed reference which immunizes (1) from revision by hypotheses like (2), confirmed as they might seem. This is, however, an epistemologically quite uninteresting case. The interesting case is the one where you propose a *revision* or *alternative* to affirmations like (1) on whatever grounds, i.e. when you want to (and, strictly: have to) appeal to something like (2) to inspire an investigation as to whether (1) is true or not. This is what a change in status from a definition to a hypothesis seems to consist in, and one necessary step in this course seems to be exactly to *admit* (2), be the

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modal discourse. In «Turning the tables on Frege or How is it that «Hesperus is Hesperus» is trivial» he expresses this view as follows: «If one sees the modal arguments as at the core of the anti-Fregean approach, as I do not, one might conclude that intellectually mediated reference [i.e. the determination of extension by intension, A.M.] is *not* what the anti-Fregean revolution is about» (p.336, my italics), but, as we could add, in the theory of interpretation for modal logic. In
«Cognitive Significance without Cognitive Content» (in: Almog, J./Perry, J./ Wettstein, H. (eds.): *Themes from Kaplan*, N.Y./Oxford 1989, 421-54) he considers to be the «lesson of the anti-Fregean revolution» the insight that «linguistic contact with things —reference, that is— does not presuppose epistemic contact with them» (454).

specification of «gold» what may. Another important thing seems to be that the admission of (2) goes *without*, from the point of view of the possibility of interpretation of the term, causing a complete deviance from former use or the inacceptability of a theoretical system which would inevitably prompted by such a patent contradiction like (3). A criterion for holding on to «former use» is beyond doubt to carry on the reference of a term. So the aprioristic attitude towards assumptions like (1) does not seem adequate for cases like the evaluation of hypotheses and the consideration of alternatives.

In the sixties thinkers like Dagfinn Føllesdal and Saul Kripke (among others) began to view this kind of problem as a symptom for an at least incomplete conception of the reference of descriptive terms and their behaviour in all contexts. They proposed instead to interpret the modal operators as relative to certain fixations of the reference of the non-logical terms of the languages in question. The central idea in these approaches seems to be a radical change in the conception of the status of sentences like (1). To introduce, use and learn some descriptive term usable in the above mentioned contexts (a «genuine singular term»[9] or «rigid designator»[10]) one fixes in a certain manner (operationally, ostensively, contextually or even with the help of a theory) its reference by the use of an implicit or explicit description, but this specific manner of making someone familiar with the reference of a term is neither to be seen as *a priori* successful in all possible circumstances nor necessarily true nor obligatory («analytic» or «true by definition»)[11]. On the contrary, whatever

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9 This is Føllesdal's term who introduced it in his dissertation *Referential Opacity and Modal Logic* (Harvard 1961) and explained its use further in the articles «Quantification into Causal Contexts», in: Cohen/Wartofsky (eds.): *Boston Studies in the Philosophy of Science*, Bd. II, N.Y. 1965, 263-74, reappeared in: Linsky, L. (ed.): *Reference and Modality*, Oxford 1971, 52-62, «Knowledge, Identity and Existence», in: *Theoria* 33 (1967), 1-27, «Interpretation of Quantifiers», in: Rootselaar, B. van/Staal, J.F. (eds.): *Logic, Methodology and Philosophy of Science*, Amsterdam 1968, 271-81, «Quine on modality», in: Davidson, D./Hintikka, J.(eds.): *Words and Objections: Essays in Honour of W.V. Quine*, Dordrecht 1968, 147-57, «Situation Semantics and the 'Slingshot' Argument», in: *Erkenntnis* 19 (1983), 91-8, «Essentialism and Reference», in: Hahn, L.E./Schilpp, P.A. (eds.): *The Philosophy of W.V. Quine*, LaSalle 1985, 97-113.

10 This is, as everybody knows, Saul Kripke's term, who explained it and the premises for its application mainly in «Identity and Necessity» (in: Munitz, M. (ed.): *Identity and Individuation*, N.Y. 1971, S.135-64) and «Naming and Necessity» (mit Addenda) (in: Harman, G./Davidson, D. (eds.): *Semantics of Natural Language*, Dordrecht 1972, S.253-355 bzw. S.764-9).

11 In a certain sense one can see this, at least in Føllesdal's case as a consequent application of Quine's critique of the analytic-synthetic distinction like the one pronounced in «Carnap and Logical Truth» (in: Hahn, L.E./Schilpp, P.A. (eds.): *The Philosophy of Rudolf Carnap*, LaSalle 1963, pp.385-406) where he says about definitions, which he cosiders to be the candidate of whose analysis we can most probably hope to get a notion on analyticity which does not coincide with logical truth: «Definitions (...) can be

means you choose or however you try to introduce a term with a fixed reference to someone or in a specific context, this can only be successful if you manage by this to get the reference *of the term* right, i.e. possibilitate that it be employed furtheron to refer to *relevantly* «the same» objects or, in case there are none of these, to none[12].

The point of «genuine names» is that they neither are implicitly nor imply any specific description to be used correctly. At least they do not have to be interpreted thus, in contrast to «usual» terms. So what has to be done is to find means to draw a distinction between «genuine singular terms» and disguised descriptions to be true to their respective differences in behaviour under certain interpretative circumstances and to avoid inconsistencies. For the satisfaction of the truth conditions of descriptions in different possible worlds coincides most probably, if these worlds differ substantially concerning the intended domain of the term, with a variance of its extension. Now, if «fixed use» coincided with «complee extensional determination», then it should be expected that a term whose reference has only been fixed for a *part* (e.g. «the thing *in the actual world*») of the «absolute» extension (through possible worlds and all times) either would be hopelessly unclear in its use or, if this is not accepted, as uniquely referring only to this factual, *partial extension*

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either legislative or discursive in their inception. But this difference is in practice left unindicated, and wisely; for it is a distinction between particular acts of definition (...) So conceived, conventionality is a passing trait, significant on the moving front of science but useless in classifying the sentences behind the lines. It is a trait of events and not of sentences.» (p.395)

[12] For reasons that, as I hope, will become clear in the following, I depart here to a certain extent from the «orthodoxy» of direct reference theory, because I want to make a more general use of its results without an essentialistic commitment from the outset. This is why I do not refer to «microstructures» or «object-identity» but rather introduce contextually an unspecified notion of «relevant sameness» which is evidently much broader than e.g. Putnam's «same» (sc. «The Meaning of 'Meaning'», in: Putnam, H.: *Philosophical Papers 2. Mind, Language and Reality*, Cambridge MA, 1975, pp.215-71) or most of the other conceptions which have been developed in the framework of this theory (e.g. the writings of Salmon, Deutsch mentioned above). I consider it sufficient for the following to suppose some «sameness-in-use-relation» accepted by the users of singular or general terms in certain practices which are linked to inductive method and hypothetical reasoning. Each of these practices, as well as each discipline, will have its own specification of this relation of the form: «A is the same substance as B iff ...», «A is the same (historical,...) individual as B iff ...» etc., where «...» is probably an interpretation-condition drawing on admissible model-classes ('physically', 'chemically', 'historically' or otherwise admissible). Thus I am not necessarily referring only to «rigid designators» in the classical sense of unqualified identity, but to designators which are to be understood as rigid *within* each admissible model class. In that sense, substitutivity or identity seems to me to be a structure to be *aimed at* in the (a priori) evaluation of admissibility but not to be *ontologically presupposed*.

(which would then be its *full* extension, other intuitively acceptable applications out of this factual extension, if seen as correct, automatically provoking the assumption of a homonym but different term, a lexical variant). Both possibilities seem to be highly inadequate if we look to our actual behaviour when we use e.g. empirically interpreted terms and extend or differentiate their use: there is a lot of discount of differences in belief, as Putnam frequently says, and, above all, no assumption at all of a «change in reference». Føllesdal[13] expresses this change of perspective quite decidedly when he remarks concerning the function of concept-explanations («senses»): «genuine singular terms have a *sense* (...), and (...) they refer partly in virtue of this sense. However, while Frege held that sense determines reference (...) I hold thet reference «determines» sense, *not by itself*, but in an interplay with our theories of the world and our conception of how we gain knowledge and how we are likely to go wrong in our perception and in our reasoning. (...) *The sense of a genuine singular term is designed to insure through the vicissitudes of increased insight and changing scientific theories that the term keeps on referring to what it presently refers to*.» (p.112)

Thus the conclusion was, that «genuine names» should refer in all possible worlds to the same object. As this now has been assumed not to be automatically accomplished by appeal to some (criterially understood) descriptive condition (like the mechanisms envisaged in formulations as «in a purely semantic way» or «by the meaning of the terms»), this demand for «referential transparence» can be seen as an at least partly independent claim in its own right about the behaviour and use of empirically interpreted concepts. Further, as this is obviously the consequence of a general, metatheoretical reflexion on the status and possible function of «meanings», the same is valid as much for singular as for general terms. If there is any justified doubt as to how «referential transparence» is to be understood theoretically, then this cannot only affect a certain kind of terms (although it might be of heuristic importance to isolate the most evident case, as is the case of proper names and indexicals in relation to «intesionalism»). What has happened seems rather to be a change of methodological perspective under the threat of communication-theoretic scepticism prompted by underdetermination-problems. Thus the various attempts to articulate a theory of «direct» (but not immediate, as one should always add to avoid facile misunderstandings and dismissals and the fast search for refuge in some kind of «causal connection between sign and world» as an answer to the question: «but how the devil does a word get a grip on a thing»[14]) reference differentiate not only between «genuine names» and «definite descriptions» (as for the singular expressions) but also (as for the general expressions) between «natural kind terms» and «usual general terms» (or «n-criterion words» where n is the number of criteria you consider to be sufficient to determine the reference[15]) and (as for the intentions

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13 In «Essentialism and Reference», in: Schilpp, P./ Hahn, L. (eds.): *The Philosophy of W.V. Quine*, LaSalle ^{3}1988, pp.97-113.

14 For this kind of shortcut see Devitt «Against Direct Reference».

15 This is a liberal allusion to Putnam's term «one-criterion-words» (cf. «Is Semantics possible?», in: *Philosophical Papers 2. Mind, Language and Reality*, CambridgeMA 1975, pp. 139-152) as to denote the class of general terms having necessary and sufficient conditions for their application or are

for communication) between the «referential» and the «attributive» use of signs. It seems to me that some kind of these distinctions will appear as soon as you try to find out what it is that makes expressions of a determinate grammatical kind behave in a certain way (e.g. are counterfactual-supporting, extendible, have an «open texture» etc.). In the following I will try to trace some pragmatic aspects incorporated in the reflections on «natural kind terms» as opposed to «n-criterion words», like we could call general terms which are supposed to be referentially interpreted by complete necessary and sufficient conditions for their application (whose extension *is*, in other words, once and for all determined by their «intension», which might best be understood as «semantical» or «interpretative rules», i.e. terms whose «meaning» changes or gets lost when you change the conditions).

To get an impression of a territory the best thing is to have a look at its inhabitants. So the question is: What terms can be or are classified usually as «natural kind terms»? And the second question is: what is it that they share to be classified thus? or: What more general differentiation does this distinction aim to reflect?

To the first question: in the various writings investigating «natural kind terms» the most common examples given are concepts of lower biological taxonomy (as «tiger», «cat», «whale»), certain operationally defined magnitudes (i.e. relations, like «meter») fundamental concepts and magnitudes of physical theory («theoretical terms» like «electron», «atom», «impulse») and everyday-language expressions for substances («water», «gold»). Contrasting to that usually it is pointed out that the following do not satisfy the conditions to be «natural kind terms» (i.e., are «non-rigid» or «disguisedly descriptive» or, as I proposed, «n-criterion-words»): conventionally determined family- and property-relations and concepts («father of», «bachelor», «owner of»), concepts definable by contrast to some contingently preexisting classification («vixen» as «female fox» but not: «fox» as «male vixen», if you take the classification as grounded in «fox»), concepts for mathematical relations («square root of», «third derived from»), concepts of higher taxonomical order («mammal», «vertebrate being», «fish») and complex descriptions of chemical substances («H_2O», although this is not always entirely clear, some theoreticians seem to assume implicitly that these are «rigid descriptions», i.e. substance *names* instead of descriptions of chemical theory).

If there should be any order in this (hopelessly incomplete) list, at least it seems to me that it is far from evident. Even rough distinctions like «concepts for relations and states in the social world» vs. «concepts for relations and states in the objective world» or «concepts for more or less observable entities» vs. «concepts for more or less unobservable entities» are only good for a first try to give the extension of natural kind terms. You could add without hesitation disposition predicates, which are doubtlessly not only present in the discourse about the «objective» world (as opposed to the «social» one) and others, for their logic and problems apparently do not differ too much from the supposed logic of natural kind terms[16].

«defined terms» in the strict sense of «definition» mentioned above in the text.

[16] Cf. Goodman, Nelson: *Fact, Fiction, and Forecast*, Hassocks ^{3}1979 [in the following FFF], p.45, fn.9. One could even adventure (see note 1) the

This might seem to ignore all the things that have been said so far in the theory of reference with respect to «underlying structure» etc. What I am alluding to is the alleged theorem of Kripke-like approaches that for a term to be a «natural kind term» there has to be some strong ontological commitment to some not yet specified entity or mechanism or structure which is shared by all individuals falling under the respective predicate (say, *X*). What this is supposed to mean is that there *is* an identity-relation between all of them which *would* make the terms «normal» if only there were a possibility to be *sure* once and for all, *what it is to be an X*. As this is *ex hypothesi* not the case, we have to commit us to its existence, even if unknowable. As further these terms are supposed to be counterfactual-supporting, the phrase expressing this commitment would have to be necessarily true if true at all.

I only want to indicate here some reservations I have that keep me from integrating this without modification in a description of the «direct (but not immediate) reference account of natural kind terms» as such. Apart from the (important) question if this is rather a surprising consequence of unproblematic assumptions about the behaviour of general terms in possible world discourse or an independent axiom with pending plausibility (as I, following Salmon[17] and Deutsch[18]

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hypothesis that a treatment valid for «natural kind words» should be expected to be valid for dispositional predicates as well: both types of expression are supposed to be counterfactual-supporting and -demanding: to explain the application of a dispositional predicate you have to invoke sooner or later a counterfactual condition, which is structurally the same when you demand that a kind-word refer «to the same things in all possible worlds». Both can only be introduced by reference to a part of their supposed total extension and have defeasible application-conditions, i.e. are supposed to function even when not associated with an exhaustive ncessary and sufficient condition for application. The best explanation of their use, i.e. to determine whether a given individual is or is not a such-and-such/has or has not such-and-such disposition is in both cases intimately tied to the best theoretical account available (this has been argued
by W.K. Essler and R.Trapp in «Some Ways of Operationally Introducing Dispositional Predicates with Regard to Scientific and Ordinary Practice», *Synthese* 34 (1977), 371-96 and by Essler in «Some Remarks Concerning Partial Definitions in Empirical Sciences», *Pacific Philosophical Quarterly* 61 (1980), 455-62). I leave matters as confused and provisional as this because a thorough examination would demand its own place. However, see fn24 for some more details and section VI. for some speculations.

17 *Reference and Essence*, Princeton 1981, Appendix II.

18 «Semantics for Natural Kind Terms», *Canadian Journal of Philosophy* 23/ 3 (1993), p.404/405, where he shows that in a proper model-theoretic analysis of natural kind terms (his system NK) «the rule of *necessitation* [that is: $\phi \models \Box\phi$, A.M.] fails» (405). The important consequence this has for the usefulness of an «orthodox» reference-theoretic account (i.e., one making essential use of the notion of «rigidity» to model the behaviour of natural kind terms) of empirical classifications he stresses in «Semantical Analysis of Natural Kind Terms» (in: *Topoi* 13, 25-30) where he concludes: «It seems to

am inclined to think), this transition from truth to necessary truth seems to betray in a certain way the initial intention of such theorizing: namely to give an account how terms refer which do not at all, that is: *neither* factually *nor* counterfactually, have a necessary and sufficient condition for their application, i.e.: are simply underdetermined. To answer to this important question: well, they are determined, we just do not actually know what it is that (causally or however) does this, but suppose this thing, seems to eschew the question instead of answering it. There is undoubtedly something right in this answer, namely, that underdetermination confronts us with the unavoidability to reflect on what we suppose when going on to use the terms *as if* they were *totally* determined when we have determined their reference *somehow*. What seems wrong about the specific answer is the assumption that there has to *exist something which makes them determined terms independently of our decision to treat them as such*. Reflecting on what we do when we use terms as described in reference-theory and what it commits us to does not, from the outset, have necessarily to result in some outright ontological answer. Rather it would seem to me that this would be a surprise. What is to be expected by this kind of investigation is, in my opinion, not so much information about what the world is really like as what status is that we have to give the world as to be able to understand what we do when we are «simply going on to refer to the same with changing criteria of identifying it as such». To put up counterfactually some «ultimate identification» that legitimizes our doing so by telling us: «if some sentence like «a=b» is true, then it is necessarily true and thus this will be how the world is like with respect to a's» does not really solve the question of how we succeed to keep track to one another and most of the referents *before* or *without* that substantial knowledge. As to confuse the case a little more: there certainly *are* a priori conditions that do permit us to do so, but they are, as I hope to make clear in the following, more general or *formal* and less demanding at the same time.

If there has to be drawn, then, some distinction between two ways to use general terms that corresponds to the distinction between the two uses of singular terms, it has to be looked for in metasemantic restrictions to the effect of distinguishing admissible and unadmissible interpretations such that in the case of admissible interpretations referential transparence and extensional determination by necessary and sufficient conditions do not coincide (i.e. where there is, for every model in a correct interpretation for a term *G(x)* some model for every necessary and sufficient condition *A(x)* for its application such that *G(x)* is satisfied by different individuals than *A(x)* in that model, that is, where the sentence «For all x: G(x) $\leftrightarrow$ A(x)» is false). Thus these terms would qualify as special for being underdetermined in the sense that there is no criterially interpretable (or even «analytical») description of their extension which remains under all circumstances coextensional with the extension intended by the application of the term[19]. Such terms then *do not logically*

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me that the semantical concepts of rigidity and nondescriptionality are secondary to that of an important property.» (p.30)

[19] The similarity that can be sensed here between the so-called «model-theoretic argument» given by Putnam and the conditions that give rise to the theory of direct reference is, in my opinion, not casual. It shows that Putnam's argument, as given in *Reason, Truth, and History*, ch.2 and the proof in the appendix to the book, and its various variants, far from making him a

imply any determinate description of the extension for all applications, as judged by empirical adequacy and/or communicative success (although there might be for every application-instance one «contextually correct» description of the extension — but this, and this is the point, is not to be seen as an imprecision as to the relevant part of «meaning», as reference is to remain intact).

How do we fix the reference of such a term and how do we come to the assumption that it is referentially transparent? According to most theorists this is done by some sort of «baptism» or «dubbing» in the following way: you take some representative sample of the reference (in the case of singular case this question is simple, because there is only one individual, thus only one representative sample) which is a subset of the set of individuals falling under the term (say «tiger») and introduce the term by some remark to the effect: «this is a tiger, that is a tiger» and so on. Furtheron the term is (in the vocabulary of the person who has been taught the term or in the vocabulary of the language to which it has been added) supposed to refer to all individuals «like the ones in the sample». It keeps its reference intact either by continuous, unramified use (historical chains) or by thus getting glued to some causal mechanism which consists in something like «If tigers exist at all: whenever there is a tiger or meant a tiger and the word «tiger» employed, then there is a tiger (respectively: some organism with the genetical structure such-and-such) referred to» or by both. But baptism and causal chains are not the only possible interpretations of the pragmatics of successful reference fixing and keeping. Putnam also admits operational specifications (thus theoretical terms get covered as well) and in principle nothing seems to prevent any successful way to fix the reference to be legitimate: as the aim is only to specify something out of a set as paradigmatic, every means, linguistic or not, contextual, theoretical or whatever that accomplishes this, seems sufficient. This also seems to be implied by the fundamental fact wherefrom an alternative theory of reference gets its inspiration: if there is no one description that guarantees the reference a priori, then every one of them that fixes it in fact is correct, and as it does not depend on any description, even non-descriptions (in the given language) can be so. It is simply an empirical question how reference fixing is accomplished in fact, and baptism is just one model of a possible solution for the case of the introduction of a new term into the vocabulary of a given language (or idiolect). The same is true, it seems, of the «contact with the reference» that an individual is supposed to have as to get enabled to apply the term correctly. This can be helpful in the case of some sort of objects, namely the ones which can be perceived directly (or at least, «directly» relative to the language into which the term is to be introduced), but need not be literally the case in general. What is important is that the reference gets sufficient specification in the context of the introduction as to enable a speaker not to confuse cases of future application; and

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«renegate» to realism (as M.Devitt would have it in «Realism and the Renegate Putnam», in *Nous* 17 (1983), pp.291-301) or committing him to transcendent idealism, shows (assuming that his reference theory is the core of his realist point of view) how realism is demanded for by paradoxical conditions *within* our practices when they are described in the traditional, semanticist way: the need for a new approach to reference is *prompted* rather than *risked* by the model-theoretic argument, it seems to me.

this can be accomplished, according to the case in question, in various ways which need not necessarily demand the presence of an individual of the extension[20].

Be that as it may, after a successful introduction a term is «referentially transparent» in the sense that we are, as all users of the term in question, supposed know that *there is a kind of things* that have (according to the best of our knowledge) some common trait and *to every individual of which one refers with the term*, e.g. all tigers. The set of all tigers, however, is not determined by any of the descriptions at our disposition that made us familiar with some of them, and therefore this type of fixing of use is *no consequence of the specific determination of the extension* accomplished by some description. We know, to put it a bit differently, that under different correct determinations of the extension under different circumstances the set of individuals falling under the term might differ, but we suppose that every individual in each of this sets has to be a tiger. As the number and structure of all possible determinations is, in view of the future and alternative states of the world, indeterminate, an effective way to give *the* extension is normally not to be expected. We could call this the *descriptive inexhaustability* of natural kind terms[21]. And it is exactly this information of the descriptive inexhaustability which is essentially part of our knowledge of the «meaning» of such terms[22]. It consists in our expectation that their reference is not covariant with «possible worlds», that is: alternative descriptions of the world in which there are individuals of this kind. In that sense we could explain this as a *normative* trait of the use of such words in the following way: we keep the interpretations of these terms constant through changes when we employ them, i.e. consider them to be referentially transparent, even though we do not (and often cannot) expect to be able to indicate the total extension, the product of the extensions under all circumstances (e.g. by some universal criterion of application), i.e. even though we assume their extensional opacity (relative to the possibilities of our language). The latter implies directly that there is no (semantical or other) fact that can be held uniquely responsible for the justifiedness of our referential expectations and presuppositions: these concepts do have, from the point of view of their *use* a regulated and concerning their reference constant application, but this invariance is (in general) not founded in any invariant condition of constatation of pertinence or characterization of the members of the extension.

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[20] The decisive steps to answer these questions would be some account of the representativity of a sample as much as a general account of what it is and how we know or suppose that some specification is sufficiently exact *in the introductory situation*. But this is far too complicated to be treated in this article.

[21] Thus the alleged «non-descriptivity» of natural kind terms would not be, as is often suggested, a result of some capacity of language to refer without any descriptive context but rather one of the continuous possibility of revision and conceptual change: there are not too few, but too many possible descriptions of the extension as to guarantee by this criterion referential transparency.

[22] This has been argued by Goosens and later by Deutsch (see below).

Kind words as characterized up to now thus seem to be unseparably linked to knowledge-changig practices, for the *central rule for their use* would then be to know exactly this: that they, although introduced and explained by descriptions, are not equivalent with them. The «original concept» which gets introduced in some vocabulary *together with the implicit or explicit information that it is a kind term* is almost empty. In that sense H. Deutsch[23] remarks: «It does not take much to be *that kind of thing*. (...) if we were armed with only the original concept of cat [his example of a kind concept, A.M.], we wouldn't know much about cats. (...) The possibility that cats are really automata is rooted, not in our ignorance, or possible ignorance, of the nature of cats, but in the meaning of the word 'cat' — the original concept of cat.» (p.409)

The problem with the talk about «reference» in connection with that type of general terms is obviously, as «reference» and «satisfaction of a description» do not coincide here *ex hypothesi*, to characterize what it exactly is whose existence is supposed to be able to refer to it. This has always been the decisive question where essentialism lurks, which might be no problem for philosophers who believe in real essences and try to prove their existence by some theoretical construct or other, but it certainly is not uncontroversial. How does it come about?

An important premise for the explanation of reference in this manner seems to consist in the idea that, given that a sufficiently well introduced term is to be considered as part of the background knowledge in a certain situation, one has to suppose the *description-irrelative (i.e. independent) existence* of some «object» (or better: reason) of referential transparence, which has to be the result of some (generally *unknown* and often *supposed to be unknown* and therefore *not completely statable*) general trait common to all individuals that are members of the kind in the case of general terms (as to be able to refer with the general term to each of the individuals that are memebers of the kind). W.K. Goosens[24] dubbed this

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[23] «Semantics for Natural Kind Terms», in: *Canadian Journal of Philosophy* 23/3 (1993), pp.389-412.

[24] «Underlying trait terms», in: Schwartz, S.P. (ed.): *Naming, Necessity, and Natural Kinds*, Ithaca/London 1977, pp.133-54. Quine uses a similar term in connection with his explanation of the functioning and purpose of dispositional predicates («Necessary Truth», in: *The Ways of Paradox*, Cambridge, MA, ©1975, 68-76) and clarifies their close resemblance with natural kind terms in «Natural Kinds» (in: *Ontological Relativity and other essays*, NY 1969, 114-38)). It would be worthwile investigating further Quine's conceptions and to compare them with what has been said in natural kind term reference theory. This is so because, following Quine's arguments one can see without difficulty a parallelism between dispositional predicates and kind terms and the evident importance of both in scientific practice, i.e. their epistemological import. Some indications may suffice to justify this claim: Quine calls (in «Necessary Truth») the counterfactual conditional-discourse underlying the use of dispositional predicates as indispensable for imputing dispositions on a domain and, above all, for the innerscientific practices of prediction and formulation (and interpretation) of hypotheses (p.73, 69), and describes its general epistemological structure as follows: «In general, when we say 'If x were treated thus and so, it would do such and such', we are

characteristic of kind terms as the presupposition of some «underlying trait». The problem was, that possible world-invariant properties or traits seemed to be directly identifiable with «essential properties», i.e. attributions of necessary truth. This is due to the attempt to draw for the distinction between natural kind words and n-criterion words on the distincton between possible world covariant and non-covariant properties. And this, if interpreted *ontologically*, leads fast and neat to talk about essences. Thus it seemed that natural kind terms might be «non-descriptive» concerning contingent properties, but surely had to be descriptive concerning «metaphysically necessary» properties. What stands in question is not, of course, the logical correctness of this conclusion when you accept some sort of Kripkean interpretation of modal logic. What is questionable is from where you want to apply it: if it is applied or interpreted in any absolute sense, then you get to essentialism. But when this conclusion gets situated within the description of the rules underlying our discourse in hypothesis-accepting practices as their interpretation-theoretic structure, then «necessity» and «essences», once gotten in the scope of reflexion, get

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attributing to x some theoretical explanatory trait or cluster of traits.» (ibid., my italics). This attribution has the following status respectively function within a given corpus of knowledge: «the [disposition-, A.M.] term has been a *promissory note* which one might hope eventually to redeem in terms of an explicit account of the working mechanism.» (p.72, my italics) This suggests that the hypothesis to the effect of some «working mechanism» or a «sub-microscopic structure» (ibid.) in the case of chemistry, in general of an «explanatory trait» (ibid.) is less to be interpreted as a serious hypothesis about the furniture of the world in itself than as a provisory, hypothetical and confirmable ontological posit with pending justification: «In the necessity constructions that impute dispositions, the generality lies along some *known or posited explanatory trait*. (...) They turn, still, on generality. But they *turn on theory, too*, precisely because they fix upon explanatory traits for their domains of generality.» (74) The acceptance and the concrete content and structure of these «promissory notes of common traits» is thus shown to vary from epoch to epoch, depending on the accessible «underlying theories» about what is possibly to be counted as a component of such a «common trait»: «What kind of account of a mechanism might pass as explanatory depends somewhat, of course, upon the general situation in science.» (72) This means that the use of dispositional classifications is comparably weak and relative to other, more fundamental or at least
already approved and accepted classifications which in turn are seen to determine ontology. This is so because until there is no lawlike statement (or, according to the discipline) something functionally equivalent to it, the assumption of some such «ontological hypothesis» is nothing more and nothing less than a hypothesis with uncertain justification. Now, such general statements of law are known not to be inductively confirmed in any direct way (cf. e.g. W.K. Essler: *Induktive Logik*, Freiburg 1970, chap. V.4.); they are thus best be seen as belonging to the (contextual) *a priori* part of a theory as a whole which is rather than a consequence, a precondition of the investigation in the structure and content of the world.

an acceptable and even explanatory reformulation (and thus loose, perhaps to the disconcern of some, much of their metaphysical robustness[25]).

Roughly the change is this: invariant properties can be seen as «important» properties, where «importance» depends on the explanatory aims of the respective practices. The relation of some pretheoretical notion of «essence» and «importance» can then be seen in the following way: «important» properties may not coincide with the «real essences», because we might restrict our interest to the cases where some property invariably occurs (e.g. to investigate its connexions with some other), i.e. voluntarily restrict the given domain of things of a kind to things of a kind in events of a type. Then this property would be «important» but not «essential» to things of the kind in general. On the other hand one should expect that «essential» properties (whatever they are supposed to be) should count always as important, when known. And this is the point: it is a commonplace that «essentiality» is something we cannot «get to know» by any standard scientific investigation. Thus, seen from an epistemological angle, the intersection of the class of essential properties and the class of all knowable or investigable properties is the empty class. The most natural reaction to this is to put up some principle like: it should be the case that the properties considered by us as «important» for the description of a kind be its «essential» properties in the sense that our generalization concerning its members be true. This, in turn, can only be argued for inductively. Our belief in the truth of the generalization can only be *confirmed* with reference to a subset of all members of the kind and thus can also be *infirmed* and even be *falsified* with reference to it. Then it might be rational to drop this classification, even though at some later stage additional information or a new explanatory approach account for the reason of this infirmation and the classification can be «revived».

Nevertheless the conviction that members of a kind, if it actually is a kind, must have some *absolute* common trait, has a place in these practices. But it does not follow *logically* alone from some given postulate (that is: *that* is not the interesting point) but is itself a *counterfactual* statement with normative content about the «grammar» or functioning of *all kind terms in general*. Whenever we have reason to suppose of some concept that it is a kind term, then this means that we know that some hypothesis to the effect that there is some trait common to all its members (however they be identified) — some «homogenity» in the domain — is

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[25] This is stressed by Putnam in «Possibility and Necessity» (in: Putnam, H.: *Philosophical Papers 3. Realism and Reason*, pp.46-69) where he remarks: «the 'essence' that science discovers is better thought of as a sort of *paradigm* that other applications of the concept (...) must *resemble* than as a necessary and sufficient condition good in all possible worlds. This should have been apparent already from Quine's criticism of the analytic-synthetic distinction.» (p.64) That is: if you want to design a theory against the underdetermination-problems stemming from this criticism, this theory should not imply theorems to the effect of reproducing the very target of this criticism. So goes only half the way when he affirms in the same context: «saying that 'Water is H_2O', or any such sentece, is 'true in all possible worlds' seems an *oversimplification*» (p.63); it is simply *just as inadequate* as saying that some such sentence is 'analytic' and *subject to the same criticism*. It is, in other words, if theorem of a theory, part of an *inoperative* theory. In case of being an axiom, one should consequently look for ways to avoid it.

valid in the language where this concept is used. We are confronted, then, with a restriction concerning admissible interpretations (and not in the least with «limits of our world») as asked for for *supposedly empirical reasons*: as long as we assume the adequacy of a given classification, of which some kind term is part, only such «worlds» are admissible domains, in which we refer with the term only to individuals which are actually members of the kind and to all of them in the respective world, according to our best theory, i.e. where this homogenity claim is satisfied.

Thus substitutivity of all tokens of a kind word in all contexts is, as in the case of «genuine names», no logical consequence of the determination of application for this general term, but a counterfactual («grammatical», as Wittgenstein might remark) claim concerning te *functioning of kind words*, namely: that they *should* be substitutable in *all* contexts (including the modal ones) and that all ontologically relevant operations (identity, quantification etc.) are valid for them even in case that there is no *analytical* or *absolute a priori* definition for them.

There are, then, certain traits of practices that demand (or at least: whose participants regard) it as a constitutive fact of their possibility that the extension of some terms cannot be completely given or given by a mechanical procedure alone but nevertheless there is a provision for their empirically clear use. Dagfinn Føllesdal[26] has provided, for the case of «genuine names», a list of conditions which prompt this type of interpretation-theoretic entities: «Names are normally introduced for the following three purposes:

(i) When we are interested in *further features* of the object beyond those that were mentioned in the description that was used to draw our attention to the object.

(ii) When we want to follow the object through *changes*.

(iii) When we are aware that some or many of our beliefs concerning the object are *wrong* and we want to correct them.» (S.108)

Contexts of use like the ones described by Føllesdal and the modalities hinted at before could be called, following Goodman, contexts in which we want to «project» predicates and the statements formed with their help. Those are contexts in which the «proceeding from a given set of cases to a wider set» (FFF, p.58) (where «set» can be understood as set of applications) not only is being made in fact but is furthermore part of the *normative expectations* imputed on a competent participant and is seen as (at least retrospectively) *rationally justifiable* and a legitimate proceeding is interpreted as a *learning process*. The paradigmatic cases in question are undoubtedly contexts in which one uses *inductive* procedures. In that sense, the foregoing could be seen as the attempt to describe the projectibility-conditions for the case of natural kind terms, which constitute an especially interesting case because what is intended with the term «natural kind term» seem to be tha classifications in use that lay on the ground of the practice of natural science.

3.— Goodman's paradox

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[26] Cf. «Essentialism and Reference», in: Hahn, E./Schilpp, P.A. (eds.): *The Philosophy of W.V. Quine*, LaSalle [3]1988, pp.97-113.

Goodman's paradox is usually situated, as its inventor did, in the context of questions concerning the justification of inductive reasoning and, more specifically, of confirmation theory.

As I only want to draw the attention to some points where I think the problems that gave rise to reflexions about a new approach in the theory of reference and the problem discovered by Goodman[27] coincide (or at least converge to the same reason), I will not suppose anything very original under the term «induction». When in the following there appear expressions like «inductive practice» or cognates, this is to be understood as a practice guided by some canonical method to relate in a systematic form singular experiences with generalizations and expectations which do not follow deductively from those. Roughly, such a method will permit to consider it to be rational accept some hypothesis or sentence as true if there is a sufficient number of positive instances at disposition. Such a step from a sum of singular true experience-describing sentences in the position of premises to some other sentence held (to degree n, if you like) true[28] is then an «inductively valid inference» and the hypothesis is to be seen as «confirmed» (to degree n, if you like) by the experience at hand. Among these one can decide the two groups of

a) singular predictions of the form

(a) $a_1 \in F, a_2 \in F, ..., a_n \in F \rightarrow a_{n+1} \in F$ and

b) inferences from singular data to general hypothesis of the form

(b) $a_1 \in F, a_2 \in F, ..., a_n \in F \rightarrow \forall x(x \in F)$

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27 This idea goes back to Quine's article «Natural Kinds» (in: *Ontological Relativity and other Essays*, N.Y. 1969, pp.112-38), where he treats dispositional terms, kind terms, counterfactual idiom, similarity grades and simplicity as a problem-cluster, for which he suggests that a clarification of one of the problems would have immediate consequences for the treatment of the others. However, I have the impression that Quine sees this problem-cluster as a sort of residual sphere of «second order» intensional talk which will be superseded as extensionalist approaches get better. This does not seem to be the case, for the approaches of Føllesdal and Kripke to some of the named problems does not make use of intensions in any suspicious way; quite on the contrary, Kripke's model-theoretic semantics of the modalities converts the whole idiom in a perfectly extensionalist language. And it was exactly there where the necessity for a distinction between kind terms and general terms arose. So it seems that the problem remains under extensionalist treatment.

28 This is, of course, not to say that there are grades of truth. There are supposed to be grades of acceptability, measured by some measure-function (usually supposed to be some modification of a probability calculus), but not of truth, for what is to be accepted is the *statement* in question, i.e. true sentence. And this taking to be true of some determinate sentence is considered to be more or less rational, according to the output of the canonical method.

The singular statements on the left represent the data at hand consisting in the results of an investigation of the domain of individuals concerning the property *F* and the clause on the right is the hypothesis.

The question which interested Goodman was whether firstly every two coextensional descriptions of the experience at hand result under the same canon inevitably in identical grades of confirmation and secondly if, therefore, there is something like «objective» learning from experience which functions without more prior knowledge of the concepts used to describe the experience than knowing that they exhaust all data.

Against these two ideas — the idea of an absolute measure function and the idea of the independence of inductive inference from linguistic descriptive means — Goodman construed in «A Query on Confirmation»[29] the following example:

Take a bowl full of emeralds. Until some moment t there have been drawn 99 green balls from the bowl. What would be, pretheoretically speaking, the correct singular prediction about ball 100? According to scheme (a) we would infer (correctly):

$a_1 \in \text{green} \wedge a_2 \in \text{green} \wedge .... \wedge a_{99} \in \text{green} \rightarrow a_{100} \in \text{green}$.

That is: «'$a_{100} \in$ green' is true» would be more probable or better confirmed by the available data than «'$a_{100} \in$ green' is false». Now Goodman construes the following predicate «grue» which is, concerning the available data, coextensional with «green». The definition is:

DGRUE «$\forall x(x \in \text{grue}) \leftrightarrow [(x \in \text{green} \wedge x \in \text{drawn until t}) \vee (x \in \text{blue} \wedge x \in \text{drawn after t})]$».

According to rule (a) one should expect that «'$a_{100} \in$ grue' is true» would be more probable or better confirmed by the available data than «'$a_{100} \in$ grue' is false». This implies, of course no logical contradiction, taking in account the definition given: why should a_{100} not be grue if the premises are $a_1 \in \text{grue} \wedge a_2 \in \text{grue} \wedge .... \wedge a_{99} \in \text{grue}$? A contradiction raises only if we try to infer *simultaneously* both ways, i.e. try to apply «green» and «grue» simultaneously to a_{100}. Nevertheless it is clear that our previous experience with colour-terms and their application conditions do not contain anything like the expectation of sudden changes because of the lapse of time (without changing something else within this lapse of time, like switching on a redlight bulb) and our experience with precious stones does not admit of too much variation in colour without a variation of the sort of stone and our conception of regular drawing-bowls dictates that there be no variation in the data just because of the lapse of time. True as all this may be, it is not sufficient to rule «grue» out as badly defined over «green» as correct and the «grue»-hypothesis over the «green»-hypothesis. The only additional condition you need for the definition of the new predicate to pose a problem to «normal» predicates is the quite

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[29] In: Goodman, Nelson: *Problems and Projects* [in the following PP], Indianapolis 1972, pp.363-6. A precision of this argument resulting from the subsequent discussion with Carnap about this article can be found in «On Infirmities of Confirmation Theory» (PP, pp.367-70). The most famous version of the problem is probably the one in FFF, chapter 3 («The New Riddle of Induction»).

modest premise that there are more objects to be examined than the ones contained in the available data[30].

The problem is general and not artificial, because there is a general rule[31] for the generation of such predicates which does not infringe any accepted rules of reasoning straightforwardly. It is simply a problem about a distinction between predicates apt or not apt for inductive inferences which permit learning from experience. This means that a consideration of the premises that lead to it (and a fortiori, a search for premises that prevent it) will show premises implicitly invoked in the cases that we pretheoretically consider to be satisfactorily solved.

In the following I will not discuss all the numerous proposals for a solution, dissolution or *a priori* rejection of Goodman's «new riddle of induction»[32]. Especially I will not discuss Goodman's own approach to a solution in form of a theory of «entrenchment», because it draws only on *custom and origin* of the predicates. These are, however, facts about behaviour, describible in predicates which, in turn, can be submitted to a «corruption» by the general rule: we cannot know whether «projected» is corrupt or not without some further information about what it is that justifies projection of projectible predicates and unjustifies projection of corrupted ones. The problem is about *validity in general* and not about *the empirical fact of projection of a particular historical period* and can thus — *pace*

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30 Cf. Putnam, H.: *Representation and Reality*, Cambridge MA, 1988, p.13 where he remarks on the occasion of interpreting the changes in the specification of the concept 'electron' within a «story of successive changes of beliefs about the same objects» (namely Bohr's various descriptions of them): «to treat all (...) occurences of 'electron' [within this process, A.M.] as synonymous as is involved in his [Bohr's, A.M.] decision to treat later research programs in the story as *extensions of the earlier ones* (...) plays a central role in theory evaluation. In fact, treating 'electron' as preserving at least its *reference* intact through all this theory change and treating Bohr's 1934 as a genuine *successor* to his 1900 theory is virtually the same decision».

31 This point is, to my knowledge, due to W.Lenzen (*Theorien der Bestätigung wissenschaftlicher Hypothesen*, Stuttgart 1974, p.174ff., esp.183, fn5) That is to say, the new predicates construed by a definition like the one for «grue» («corrupted» we might call them, following W.K. Essler's terminology in «Corrupted Predicates and Empiricism», in: *Erkenntnis* 12 (1978), pp.181-7) do trivially coincide with the «normal» ones in case that the second clause (after the 'or') in DGRUE is false because of a factual truth like

32 This rule is the following (cf. F.v.Kutschera: «Goodman on Induction», in. *Erkenntnis* 12 (1978), pp.189-207): Take the two principles induction (a) and (b) and any predicate F and any set A of objects such that $A=\{a_1, ..., a_n\}$ is the set e.g. of all objects tested for F until t (or, more general, a non-empty real subset of all objects in the supposed universe such that there is an $a_i \notin A$ with $i \neq 1, ...,n$), that is, the available data. Then, this is Goodman's argument, there is a predicate F' such that $F'(a_j) \leftrightarrow F(a_j)$ for $j=1, ...,n$ and $F'(a_k) \leftrightarrow \neg F(a_k)$ for all $k \neq 1, ...,n$. Formally this is $F'x :=_{df} x \in A \wedge Fx \vee \neg x \in A \wedge \neg Fx \vee$. The contradiction arises for a simultaneous application of (a) or (b) for a_{n+1} with respect to F and F' which seems justified, for $Fx=F'x$ for A, which gives $F'a_{n+1}=\neg Fa_{n+1}$ and Fa_{n+1}.

alleged virtuous circles — not be on a way to a solution if this way is absolutely a-normative[33]. Thus Goodman is in my opinion completely right when he writes: «Any argument that the initial choices of projectible predicates are determined by some non-random operation (...) requires showing that these *predicates* are distinguished by some common and independent characteristic (...) that can be correlated with such an operation (...). The *unavailability* of such a characteristic (...) is just what gives rise to the riddle.» (PP, 358, my italics). There is just one condition which sounds a bit like Goodman's attempt but is in effect of totally different sort, namely the following adequacy condition for a solution: any definition or theory of «projectibility» has — in analogy to a theory of truth in semantics — to yield all predicates as projectible that have been judged pretheoretically as sucessfully projected[34]. Nevertheless the change of perspective from a purely syntactic-semantic treatment to a pragmatic one (as no condition as to the properties of the *signs themselves* without consideration of their *use* or *application* helps) that Goodman proposes in remarks like «entrenchment derives from the *use* of language» (FFF, p.95) seems absolutely right. This is the line I want to follow in the following a little bit. I should make clear from the beginning, though, that I consider the theory of entrenchment as one of the possible specifications for a strategy towards an elucidation of what the use of language in contexts where we distinguish valid from invalid inductive inferences, and furthermore as one which is (for the reasons given above) not too promising. Thus the following is not to be seen as a contribution to entrenchment theory, and I fear it will not even be sufficient to indicate a determinate strategy for avoiding Goodman's problem. What follows is rather intended to apport some more evidence to the suspicion that there are common points in the desiderata raised by reference-theoretic and projection-theoretic problems.

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[33] To get an impression of the impact caused by it, may it suffice to recommend the excellent collection of essays on Goodman's paradox provided by D.Stalker: *Grue!* (Chicago/LaSalle 1994), especially the exhaustive annotated bibliography of texts in English on the problem contained in it.

[34] Goodman himself says that in view of this problem the aim has to be to reach a «dichotomy of predicates» (FFF, p.80). The insufficiency for an answer to this question of the resources given to us by past behaviour is stressed by him in his critique to Hume when he says: 'Hume overlooks the fact that some regularities do and others do not establish such habits; that predictions based on some regularities are *valid*, while predictions based on others are not. (...) To say that valid predictions are those based on past regularities, without being able to say *which* regularities, is thus quite pointless. Regularities *are where you find them*, and you can find them *anywhere*.' (FFF, S.82) This obviously applies *mutatis mutandis* to descriptions of «induction-*regularities*» found in our culture (be they or not reached by reflexion: the question in point is whether they have *normative* import or not). What is demanded is a *general procedure to distinguish two types of predicates* in the structure of which one could find some set of interpretation-theoretic presuppositions of valid inductive inferences (as opposed to invalid ones); this is obviously impossible if one is limited to *particular and contingent* descriptions of the set of all valid inductions.

4.— Aspects of the interpretative theory underlying projectible predicates: Some remarks on the conditions of the distinction between projectible and non-projectible predicates

The reason why I do not think that my observations in this part will constitute a solution (or even only the nucleus of such) to Goodman's paradox is precisely that I am inclined to think that there is no outright solution from an absolute vantage point. *Absolutely* seen, the paradox is, due to the fact that it is generated by a perfectly admissible general rule, unavoidable for any empirically interpreted predicate. Predicates or classifications can only be justified *from within a practice* and the same predicates can always, *in a reflexive metalevel* of the language used in this practice, be «corrupted». It would not even be correct to exclude the rule leading to the corruption and the premises necessary for its derivation *a priori* as bad in general: there are corrupted domains where *only* corrupted predicates (and their respective confirmation-methods) are adequate, moreover there should not lurk any intractable problems if we *know* how the corruption has taken place: in that case we could e.g. simply modify our definition and make it conditional on this mechanism (which would be a kind of errorlevel-fixing)[35]

So what seems to be called for is, in my opinion, a view of how this activity of «corruption of predicates» is restricted, when necesary, and what the world and the language is supposed to be like to do this justifiedly. These presuppositions are, thus, presuppositions of inductive rationality. I will try, then, rather than to solve, to *reconstruct* Goodman's riddle in such a manner that we get a glimpse of why we do not always have to struggle with the paradoxical consequences it can have and what the ontological and epistemic commitments might be like that we have to make to do so.

Now, what does it mean to change the perspective towards a point of view that throws some light on the *pragmatic* conditions of interpretative procedures in inductive practices that help us understand the process of learning from experience as a rational one? First, it means indicating oversimplifications: «The fact is that whenever we set about determining the validity of a given projection from a given base, we have and use a good deal of other relevant knowledge. *I am not speaking of additional evidence statements*, but rather of the record of past predictions actually made and their outcome.» (FFF, p.85)

For the reasons given in the last part, Goodman's last remark is not very convincing as it stands. A possible treatment of the difficulties raised by the problem cannot be expected to work if it is limited to the invoking of the *fact* of background knowledge, and *mutatis mutandis* neither by any structured description of some background knowledge. It has to start not only from *use* but from a representation of the *rules for use* of the predicates, to indicate further some rule contained in all sets of rules for projectible but not contained in the ones for the unprojectible predicates (or something like that). For this to be efficient it is, again, insufficient to put up some classification of *de facto* successful words and their understanding (or «meaning»). The aim has to be to indicate the (ontological, cognitive and epistemological) structures implied by these rules that *explain success.*

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[35] Cf. Hacking, I.: «On Kripke's and Goodman's Uses of 'Grue'», in: *Philosophy* 63 (1993), 269-95.

Having put things like this, the question seems to be: what are the *normative conditions invoked by and taken for granted in the application of predicates such that they do not prevent learning from experience*? These conditions would be, in turn, part of the background knowledge, but *not* (as Goodman correctly indicated) some substantial knowledge about the content of the domains. They would be of a much more general sort, like the «knowledge» that there is some domain and that there is a language and that both have to be related to each other in an interpretation that assigns objects to signs (constitutes reference) etc, and that the *intended domain* for application of a certain predicate is of a certain *structure*. They would constitute something like a cognitive matrix that is inevitable for the correct mastering of predicates in certain practices. We have already seen that on the purely syntactic or semantic level there is no difference between «grue» and «green»: you can define «green» in a vocabulary of «grue» plus an individual constant and vice versa. Both are *definitorily symmetric* and furthermore they are, with respect to the given data (in *A*) *eliminable*, i.e. in the description of all individuals in the data you can always substitute the definiens of the respective describing predicate *salva veritate*[36]. Both predicates become *asymmetric* in the case of a_{n+1}: a_{n+1} is grue iff not green and vice versa. Now, this could be described as a process of becoming extensionally opaque although there would be, *with respect to the given data* the possibility of an identical necessary and sufficient conditon for use. If we take *A*, as plausible, as the set of our possible paradigms for introductions of the predicate, then there is always a manner to go on with the application of the predicate in question which resembles «grue». *On logical grounds* there is no means to prevent it. It seems, then, that we are confronted with the same phenomenon as in the second part. An indication of some

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[36] A case where it might be reasonable to keep or construct a corrupted predicate could be, for example, the case of some «objective» change that would, however, for its particularity, not call for a change of theory, but nevertheles for a modification in the homogeneity-supposition. Imagine for example human population of a certain specific genetic structure inhabiting an area with active volcanoes. One day one of them erupts and this eruption causes testable changes in the genetic material of the children of the members of the population that survived the catastrophe. In that case it would be irrational to expect the predicate of genetic theory, say «to have characteristics F, G, and H in the genes», which was coextensional with «being a (geno-)typical inhabitant of
area V» (and even theoretically more exact) before the eruption to be projectible afterwards. That means that it would be irrational, *knowing the «rule» of corruption*, to go on using the genetic predicate of before the eruption because it used to be perfectly projectible; rather one should take the «rule of corruption» in account and add it to the background knowledge (in the example this would be exactly a «Goodmanian» disjunction of the type: «someone is (geno-)typically V-ish iff he/she either has characteristics F, G, and H in the genes and was born before the eruption or has characteristics K, L, M and was born after the eruption»). One could even go on with this example and think of the possibility that this variation is not of dominant character and thus disappears, say, after the seventh generation so that the first predicate gets fully re-applyable (when all members of the variant-population have died). For the time between these two events it would, nevertheless, be inappriopiate.

necessary and sufficient condition (i.e.: identical examination procedures) based on *A* does not lead, as we go on applying the predicate, to the same results with the same objects: both predicates get «ramified», such that after t (or after a_n) the test-conditions to be satisfied to count as «green» or «grue» are mutually incompatible. If we assume that the examination method does not change and the objects after a_n give the same results as before, then all attributions of «grue» become false, i.e. «grue» has not been specified sufficiently in *A* to refer to grue things, whereas «green» has been sufficiently specified: it keeps its reference, whereas «grue» has still to gain a representative class to get its reference fixed: *A* was paradigmatic for the introduction of «green» but wasn't representative for the introduction of «grue». As both are interdefinable, this is always true for the respective definiendum and definiens: in the definiens there is an essential occurence of an individual which causes the non-representativity of the assumed sample, i.e. to be a sample for the predicate in the definiendum requires always *more* concrete information on the individuals of the *extended* domain and their properties. Now, our assumption that with «green» we can go on as before, i.e. that *every* token, independently if uttered before or after a_n, is substitutable salva veritate for every other, depends *a fortiori* on a different presumption (or «information»), namely that the universe is, with respect to «green», homogeneous enough that *A* can be seen as representative and thus serve as the class to introduce «green» in such a way that it becomes *definite* or *non-ambiguous*: it refers, if it refers in *A* to objects of a sort, then *always* to objects of *this* sort. «Grue», on the other hand, gets assigned as reference objects that have, relative to relevantly the same test-procedures (e.g. colour-analyses), *different* structure: there are some *F*'s and some non-*F*'s in its extension. To apply it with the help of these test-procedures, we have consequently not only to know the result of the test, but furthermore *which individual it is we apply the predicate to* to be able to determine the truth value of the respective sentence.

So we could say that relative to a remaining application-background, one of the predicate refers to individuals with relevantly the same structure whereas the other does not. If we assume now that Goodman is right in that «green» is projectible and «grue» is not, this can be expressed as saying that we assume for «green» that there is something which all green things have in common in a certain respect (e.g. colour), whereas there is nothing like that in the same respect within the same cardinality of a sample of grue things[37] (for they could have something different in common). If we suppose this of our universe of things, homogeneity in a certain respect of more abstract order within a system of classification (like: light green is a sort of green is a sort of colour is a sort of optical thing property is a sort of thing

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[37] This follows from the definitions given above: If we take *A* as e.g. the class of all experiential sentences up to now and assume that there is no more individual than those contained in *A*, then *F*x=*F'*x (see fn30). This is quite obvious, because the individual expression needed to define either «grue» on the basis of «green» or «green» on the basis of «grue» does, in that case, not refer. Thus the condition «not drawn until t» is trivially true because there is no more thing to be drawn and the condition in the second part of the disjunction of the definiens is, because of the (in classical logic) trivial falseness of «drawn after t», also false. Therefore the definition of «grue» (i.e., in general, of *F'*) is satisfied, in this case, by all things that are green (i.e. *F*).

property is a sort of property), then «green» is referentially transparent relative to our colour theory whereas «grue» is not, for an examination of colour *alone* is not sufficient to determine whether a given thing is grue or not and thus our aplication of the predicate correct or not. Nevertheless we *do* have a necessary and sufficient condition for the application of «grue» on the basis of our colour theory and the harmless assumption of more things than the examined ones in our universe (which is, in turn, nothing more than a specification of the interpretation-possibilitating premise of a non-empty universe), viz. the definition given in the previous section.

What does all this amount to? It amounts to saying that having necessary and sufficient conditions for the application of a predicate does not mean to have projectible predicates. It is neither necessary nor sufficient for this. The presumption of the projectibility of a given predicate depends instead heavily on uniformity assumptions contained in underlying theories about the structure of things, i.e. assumptions about the world *understood as such from within a certain practice* (this is important, because our background theory could be corrupted and its background theory etc.[38]: *realism* in that sense is *always* an intrinsically internal presupposition for the *use* of predicates and, if hypostatized as *absolute,* immediately subject to corruption-counterexamples[39]). To put this point a bit differently: there is no description of any part of the extension (i.e. class of things to which a given predicate applies) which *guarantees continuity of reference* and thereby the projectibility of allegedly projectible predicates. Non-projectible predicates, on the other hand, are only applicable if we give a description of one determinate part of the extension.

The important thing is that nevertheless the projectibility or continuity of reference is *essential* for the possibility to learn of new things that they are like the known things and to learn, starting from their common features, about their differences, in short: to learn from experience. To make predicates projectible, we have thus to suppose a) that there are things that have the property we assign to them independently of our knowledge of them (that there are more than the ones we know, for otherwise there would be neither any interest for a property nor a possibility for the «grue»-predicates to come up, and thus no reason for scepticism about the referential efficiency of our predicates) and b) that there is something common to all things we apply the predicate correctly to. This is, however, a

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[38] For example we could imagine (in the case of Goodman's 'grue') that there is only a finite number of additional things in our universe which does not exceed the number of green things -n; then the average homogeneity of the class of all grue things could never reach the average homogeneity of the class of green things because n does not become sufficiently little to be neglected. Otherwise the set of all grue things becomes (in the limit) almost indistinguishable from the simple (inductive) complement-class to green (*pace* the n green things in it, but if n is very much lesser than the total of all grue things in the limit, then there is an almost zero-possibility to get something green out of the class of grue things).

[39] This has been argued against certain attempts to dismiss Goodman's problem on grounds of «natural» categorization-systems which provide us with a conflict-blocking overhypothesis by J. Ullian in «More on 'Grue' and Grue», in: *The Philosophical Review* 70 (1961), pp. 386-9.

consideration from *within* a certain practice: «green» is, once introduced, referentially transparent and «grue» is not because we base our expectations on some available background theory which we do not question in this same moment: it is our «theory of how the world is» and contains some assumption to the effect that colour attributions are independent of time points and the selection of individual colour-porters as such: colours are «in the world» (of our not problematized background-classification), whereas the selection of individuals to be examined and its temporal structure is not (because it is never, according to virtually all theories of regular confirmation-methods). In that sense we could talk of a *realist* assumption of projection-practices, where the realism enters in form of the presupposition that there is something common to the things in general (the world) independently of how we introduced the term and afterwards introduce it: every representative class *within* our available data is such that it is *sufficient* to exemplify the property in question *for all* subsets of the application.

This is, however, obviously not a *substantial knowledge about the things in themselves* but a presupposition about the structure of the universe of discourse. And *this* is in turn not a presupposition linked to the predicates as such but to their *application* and *use* and the rationality assumptions related to them: if we want to learn from experience, we need projectible predicates, and if predicates are to be projectible, we have to presuppose the mentioned structure of the world. Without our interest in learning from experience, the interest for projectibility would have, in my opinion, no reason. This is so because complete ignorance (that is, the supposition that there are no more objects to be examined) blocks non-projectibility and complete knowledge (that is: including the ramification-conditions in the definitions of non-projectible predicates) makes it harmless. And the interest to learn from experience and its necessary conditions, that we neither know nothing nor everything about all things in the respect in question, named by some predicate, is, obviously, a presupposition of our rationality.

The knowledge needed for the use of projectible predicates thus cannot consist in a knowledge of *the* (factual or counterfactual) extension,, but only about the supposed common structure of all individuals that are members of the extension*s* in the plural that we successively determine under varying epistemic circumstances. This knowledge is, concerning every single predicate, a knowledge of the homogeneity to be supposed for the things in the universe in that respect, concerning *all* projectible predicates it is the «knowledge» *that* their domain has a non-corrupted (or known corrupted) homogeneity, in other words, that the predicate can be correctly applied to every individual object of it *independently of the fact of how and when it has been registered or identified*. The behaviour to use such a predicate *as* fixed, as *applicable to a determinate sort of things* is, then, to be seen as a rationality assummption concerning the use of a determinate sort of general terms.

It implies *epistemologically* that one has to suppose certain things *about the domain* as soon as one uses a predicate and presumes it to be projectible. This, in turn, means that the supposition is *independent* of the determination of the respective extensions *in* a certain epistemic situation, as long as the domain is supposed not to change. This *relative independence*[40] of determination of extension in an epistemic

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[40] This is in fact the same as what Putnam says in his «model theoretic argument», in *Reason, Truth and History*, Cambridge MA 1981, ch.2. For a

situation and total extension referrable to with a projectible predicate (see the condition on p.37) can be seen as the nucleus of the presuppositions we have concerning *all* projectible predicates. It can be understood as a sort of general rule for use for predicates employed in practices oriented by the presumption of the possibility of learning from experience. As such it is (as Goodman saw) part of the background knowledge (the suppression of which gave rise to the riddle), and additionally as a *normative presupposition* about the efficiency (with respect to the aim of learning) of a given classification.

This nuclear presupposition has two components, which are of metasemantic and cognittively reflexive character (i.e. show that to be able to use projectible predicates, one has to suppose a minimum of distance to one's home langage), a quasi-ontological and a meta-epistemical part.

On the one hand there is the quasi-ontological assumption that the things falling under a concept have a common trait (are, relative to some background categorization, to be the same if numerically different) which «justifies» the classification independently of the concrete method of identification of individuals *as* falling under the concept. It is metasemantic in the following sense: assuming this, we simply count only domains (or models) as admissible that satisfy this assumption, make it, in other words *come out true*. It is, like in the case of genuine names or natural kind terms, an implicit restriction of admissible interpretations[41].

On the other hand there is the meta-epistemic assumption that the substantial, epistemically operative knowledge associated with the predicates that permits their identification *as* members of the extension is subject to continuous change (and does, therefore, never amount to a necessary and sufficient condition to determine *the* extension — because corruption is always possible, be it by us, by our errors or our world), whereas *this alone* does not bring about substantial changes in the domain.

The first part accounts for the preconditions of a referentially determinate use (which is thus construed as a supposition and so no direct negation of underdetermination but rather a strategy to cope with it) and the second part accounts for the openness and variability of the determination whether a predicate has been «correctly applied».

5.—. Some similarities

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fine and very clarifying account of the structure of the argument see Hallett, M.: «Putnam and the Skolem Paradox», in: Clark, P./Hale, B. (eds.): *Readiing Putnam*, Oxford 1994, pp.66-97.

41 It is never total, though: we can, with a change in «colour theory», most probably expect that our former predicates for colours in general will all be corrupted with the condition «Something is of colour x iff of colour x until the new theory was accepted or of colour y afterwards» or something like that. To suppose projectibility in a *continuous* sense we should then have to wait for a «unified colour theory». But in this section I am talking about the presuppositions we make from *within* an induction-based practice, and this is *essential* for the acceptability of realist assumptions, I think.

After having looked at the two problem-clusters of projectibility and kind terms, I want to stress some of the similarities that seem to be central to the use of general terms in either case.

The first and most striking similarity is the fact that gives rise to the problems: underdetermination of reference by interpretation. To take the problems discovered by Goodman and the natural kind theorists seriously is to accept from the outset that empirically interpreted predicates are not equivalent or coextensional to some one description of their extension where they do not occur. Nevertheless in both cases the presumption of referential transparence is central to the practices that would get in trouble were these problems operative. A theory of what we do when we suppose it, i.e. what background assumption falls if we should *discover* that some terms naively used as if referentially transparent are not really so (cases in medicine abund, but even a rise in differentiation in measure theory or an unnoticed extension of paradigms can prompt such a discovery): namely the assumption that the things falling under a kind concept are not really of a kind, put in terms of the practices: that the kind term is none and thus our generalizations might partly or generally be mistaken, such a theory is therefore an intrinsically pragmatic theory and no enterprise in metaphysical ontology.

This is the second similarity that seems remarkable to me: in both cases the assumption of referential transparence in absence of complete knowledge of the extension has the status of a *rationality assumption*. In that sense we could agree for both cases with what Føllesdal formulates for the first case thus: «Sameness of reference is *never* guaranteed.» (loc.cit., 110) The assumption of referential transparence or continuity is in that sense *not strictly epistemic or normative* and unfundamentable in the sense of not being logically or otherwise deducible. Accordingly Føllesdal goes on saying: «I look upon rigidity as an *ideal*, (...) that *prescribes* the way we use language *to speak about the world*. (...) All our talk about change, about causation, ethics and knowledge and belief (...) *presupposes that we can keep our singular terms referring to the same objects*. To the extent that we fail, *these notions* become incoherent.» (ibid., 111) Nothing to add except the stress on the fact that learning from experience is one case of «change, knowledge and belief» and that therefore, if Føllesdal's conjecture is right (and valid for general terms, as I hope to have argued), also this concept gets mysterious if we do not provide an adequate account of reference that explains the cases where we do *not* (to our knowledge) fail.

The analogy between Goodman's problem and the problems Witgenstein treats in the *Philosophical Investigations* (addition, pillar) that has been stressed by Kripke[42] and both to the problems that prompted the reflexions on the foundations of

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42 This is, as mentioned before, a hint to the *normative character* of a possible reconstruction why we do not always get confused by Goodmanian predicates. The elucidation asked for is, as far as I can see, how to make clear *why* corrupted predicates *must not* occur in certain practices, and not a general answer to the sceptic, that is, a discovery of something that is the case that *makes them not occur in fact*. An assumption raised by such a reflexion is of *intrinsic normative* character because for the reasons given it is neither plausible nor even desirable to exclude Goodmanian predicates *a priori*. The only answer to the problem raised by corrupted predicates asked

reference (more general: interpretation-) theory in form of accounts of 'direct' reference suggest that the operative assumptions, even though they have ontological and epistemological import, are interpretable as formal conditions of a certain manner of use of general terms (perhaps the «referential» use?). They do not constitute an *empirical* knowledge of certain facts or properties of the world: to presuppose projectibility or fixedness of reference through representative samples is not really to have learned something *about the world* for to be able to do *that* we have already to have projectible predicates. It means rather to have learned something about the relation between language and world, to have learned to differentiate by way of reflexion between language and the world described by it. This is the third similarity I see: that in both cases we get aware of some *reflexive capacity*, namely the capacity not always to confuse the result of given identification procedures (and «operational definitions») with *the* reference, the linguistic categories with confirmable structures in the world etc.

The fourth similarity one can extract from what has been said so far, if it is not completely erroneous, is a strikingly Kantian consequence (which, however, was already foreseen by Goodman[43]). It concerns the epistemological status of the assumptions having to be taken for granted if we assume projectibility and/or counterfactual substitutivity or fixedness of reference. Both assumptions are *a priori* relative to a practice of application of the respective predicates but nevertheless only

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for is one from within the field where they actually cause trouble. If that is right, then the only thing we have to make clear is what exactly we do *if* we exclude them and what are the assumptions we have to make to be able to do so. This is the structure of the answer to the question how it is that we do not always stumble over corrupted predicates and consequently err in our inductive behaviour. These assumptions may have ontological import of a general sort (like the differentiation between sign, interpretation and object) and,, in virtue of that structure, exclude certain interpretative strategies as unapt to serve this aim (render certain interpretation theories wrong for an account of this behaviour and the contribution of language to the success of general behaviour), but one must not forget that this does not «prove» them to be «true». They are part of a rationality strategy *seen from inside*. From a participant perspective in the mentioned practices we certainly assume the existence and independence of our objects of investigation from the outcomes of the investigation, that is, we are and have to be «internal realists». However, this does not in the least mean that the ontology supposed in these practices has to be seen as any more priviledged than are these practices themselves in our conception of ourselves. This is to my mind the reason for the steady insistence on «explanatory relevance» of a common trait, for this is a case where the privilege of being worth to be pursued — explaining, that is — is almost too evident to be stressed. Especially it does not justify a claim to the effect that this is *the* world and the normativity integral to these assumptions has not to be misunderstood in the sense that, biewed from the outside (possibilitated by e.g. an alternative account of the domain) we have to hold on stubbornly to some set of categories.

[43] Kripke, S.A.: *Wittgenstein on Rules and Private Language*, Cambridge MA 1982, esp. p.20.

to be motivated, specialized for each single predicate and to be tested *a posteriori*. So the two assumptions, conceived of as two aspects of a capacity to distinguish world and language (for each language, but not, of course for all languages), are *synthetic a priori*, where the «*a priori*» is, obviously sort of *contextual*. Kripke calls the respective assumption for modally stable terms, as is well known, «*a posteriori* necessary». This can be given the following, «deflationist» reading: by this selection he stresses the sort of non-analytic but strict validity that we impute on the rules for applying these predicates: these rules serve as a standard for admissible interpretations (and thus are, from a purely semantic point of view, rendering a concept of «necessity») as long as we consider the generalizations articulated in them of explanatory force or whatever worth, and this evaluating, reflexive activity is not to be accomplished by logical truth or analyticity. Kind words exist in this view thanks to the experiences we make with things in the world and only assuming them to be such we can procede to an investigation of the objects in the domain that *must not* substantially change *because of the results of the investigation (the changing descriptions) itself*. The experiences which prompt the generation and are involved in the introduction of kind terms are made with arbitrary objects or «contingently» given «samples», where their *being samples of a kind* is, again, an *a priori* assumption concerning the homogeneity. Thus the set of introduction-paradigms is «*a priori* contingent». In both formulations we can thus see substitutes for the *synthetic aprioricity* of the presupposistions that are inevitable for the generation of kind words. Of course, Kripke would probably charge this treatment of undue *epistemologization of metaphysical categories*; but if we do not pronounce any opinion as to their place in theories (which is the same as relating epistemology and (meta-)semantics), these categories become quite pointless. In sum, I think that one can say that all of Kripke's metaphysical conclusions are only insofar essential to an explanation of the behaviour of expressions that are flexible enough to cope with changes in our knowledge without being unduly flexible in their reference as they can be reconstructed as indications of *normative* conditions of the use and interpretation of predicates within inductive practices. One of the best expressions for this way to connect the heavily charged notion of «necessity» with the *reflexive attitude* needed for this manner of use can be found in Donnellan's early article «Necessity and Criteria»[44]: «Whether [a determinate statement, e.g. one that relates a property considered as important for the generation of a kind, an «underlying trait» and a given predicate, A.M.] *is*, as we *intend* it, a necessary truth or contingent, is *indeterminate*. *It is indeterminate because the* ***decision*** *as to which it is would depend on our being able to say now what we should say about certain hypothetical cases*. (...) Necessity (...) might be thought of as an *ideal rigidity in our* ***judgments*** *about what to say concerning hypothetical cases*.» (S.658)

6.— Speculations on the relations between projectible and natural kind terms

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[44] Cf. FFF, p. 96: «Somewhat like Kant, we are saying that inductive validity depends not only on what is presented but also upon how it is organized; but the organization we point to is effected by the use of language and is not attributed to anything inevitable or immutable in the nature of human cognition.»

The question of the exact relation of projectible to natural kind terms remains open. An answer to it will depend on the specific account in which the behaviour of both types of predicates and related ones (e.g. dispositional predicates) will be described. I will only adventure one hypothesis in this respect. In general the conditions for being treated as projectible should coincide more or less with some versions of what Føllesdal said about the motivations for introducing names. It seems probable that what is intended with the classification of some general terms as natural kind terms is somewhat more specific than what is intended by a qualification of predicates as projectible. Inbetween I would expect the dispositional predicates. Thus the relations would be: not all projectible predicates are apt to constitute natural kinds, but for a given predicate to be a natural kind term it is inevitable that it be projectible. Projectibility would then be a necessary but not sufficient condition for being a natural kind term. The methodological priority which is often given to the latter is probably the consequence of the fact that they underlie our most common classificatory practices. On the other hand one can, as I have tried to make clear, learn something about what projectibility consists in through an analysis of the rationality-presuppositions involved in the use of these so common terms.

It does not seem all too far fetched to suspect that terms that are treated as projectible are natural kind terms iff they occur as fundamental concepts in theories of natural science (as opposed to social science and others).

The class of dispositional predicates seems also to be more general than the class intended by the term «natural kind terms», but it is an open question if all natural kind terms are to be analyzed as disposition terms. However it seems conceivable to me that this is so in view of the fact that what is done in natural kind term theory is to establish a relation between underlying, unknown and known, superficial properties of objects, which is exactly what one does when imputing a disposition to some object. Dispositions are, however, less firmly linked to theories about facts of the objective world and preferably excluded by them as *explicit* dispositional predicates. In that case, natural kind terms would be the accepted correlates of disposition predicates for natural science. This might sound a bit strange at first sight, but the decisive point that has always been made to differentiate natural kind terms from n-criterion words is clearly that in the case of natural kind terms there is, in addition to some manifest community, an (causal, microstructural or whatever) *explanation* how this community is brought about, although this is something we (can) have only *indirect* knowledge of as long as this explanatory trait is only accessible by the analysis of some *testable* manifest traits and the reaction of the things that have them. On the other hand dispositional predicates also have some traits that resemble names, as has been claimed for natural kind terms: they are descriptionally inexhaustible and help us to generate sets of things, all of whose members we refer to by calling them e.g. «intelligent», «soluble», «being one meter large». Thus the structure of application is *implicitly* dispositional, I am inclined to suspect (both types are, to remind of an almost forgotten attempt to treat this kind of questions, introduced by some sort of «bilateral reduction sentences» and afterwards used *as if* they were «normal» predicates although it is known that they are not defined and they are kept as reference-constant through changing operational conditions to determine membership (this is the *advantage* of not being defined but being, nevertheless, accepted as referring to some explanatory relevant grouping of things)).

Dispositional predicates do also have to be projectible, but in contrast to the completely general supposition of an existing homogeneity there is, in the case of dispositional predicates, an explicitly stated criterion for the decision whether it is justified or not. This might be expected to be found in all projectible predicates, thus perhaps both classes coincide under the condition that the projectible predicates are to be interpreted empirically. But these remarks are, I want to stress, by now merely speculative.

7.— Summary and Conclusion

The specificity of natural kind terms seems to be that they are our means to constitute domains of investigation. We could call them synthetic categories. In what sense they are dependent on purely formal, essentially synthetic but nevertheless contextually a priori presuppositions can be seen when they are viewed as a special case of projectible predicates.

I think that the behaviour of natural kind terms and our behaviour using them show in an exemplary way a specific formation of ontological and epistemological background convictions concerning the relation of language, our use of it, and reality. That the description of the rules for their use consists simultaneously in a description of the rules for predicates apt to be used in inductive procedures implies that a part of these convictions concerns deeply our relation to past experiences and expectations about future experiences with reality. The presuppositions for the use for predicates usable in induction are obviously at the same time the ones needed for the possibility of structured learning processes. So the reflexion on the conditions for the use of natural kind terms, which have, as we saw ontological and epistemological import, can apport (some of) the philosophical assumptions taken for granted in the talk of «learning from experience» or, to put it differently, what the assumptions are one is committed to when adopting a cognitivist attitude towards our experience with the world. As soon as an agent supposes to learn from experience, he has to accept some version of the presuppositions (or more) indicated above; they are part of the general background knowledge that makes possible that we deal in an ordered way with past experience and access to some such way to evaluate new ones.

The question of how it is possible or better: what it is to apply a kind word in a determinate way is answered by the theory of reference with the seeming triviality that this is the case iff we always refer with it to the same: all individuals of a kind. The mentioned question reminds undoubtedly of Wittgenstein's incessant questions on following a rule. Now, taking this reminder in account, one could say that the exciting «discovery» in the course of the work on a theory of the interpretation of natural kind terms was exactly to show that the *specific theory* of rule-following for that case has inevitably to be a theory of reference and is not possibly substitutable by any account based on meaning that consists in the attribution of some set of deterministically conceived, substantial rules that function according to the example of analytical or logical truths. This has been resumed by Putnam in the mouthshell that in the case of the interpretation of this sort of terms «reference does all the work»[45]. The strictness of the validity or «necessity» of the rules for the interpretation of natural kind terms is not exactly analogous to logical truth; it is not primarily due to our relation to the intersubjective undisputability of logical truth but rather to our relation to experiences in the objective world and our conviction

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45 In: *Journal of Philosophy* 59 (1962), S.647-58.

articulated in it that the world is independent from the beliefs we maintain *de facto* (although it is not, of course, independent from experience and language in general: rather every use of language articulating our experience presupposes necessarily some object of experience). This discovery of a «non-analytic necessity», as Deutsch[46] puts it, is, from my point of view, the most important result of the so called theory of «direct» reference and is, thanks to its general character reconstructible and obtainable without most of the fundamentalistic metaphysical convictions associated with a good deal of the work done in this area[47].

In sum, the (semantical, epistemological, pragmatic,, ontological) differentiations between sign and signified, reality and construction, reference and transmission of what is meant and, above all, our capacity to draw them, seem to be unseparably linked to the cognitive inventory that we invoke when we talk of «learning from experience», «the independence of confirmation instances» and the like.

Thus any theory that blurs these differentiations is incompatible with a claim to the effect of the possibility of learning, improving theories etc. A deterministic theory of reference that tries to reduce the reference of the terms to a mechanism between the factual substantial knowledge associated with the term (its meaning or one determinate description of the extension) and objects that satisfy this knowledge is incapable of describing adequatly the behaviour of the participants in practices who assume them to serve the aim of learning. To attribute them a capacity to learn and criticise *de facto* existing beliefs and a cognitive attitude towards hypotheses is incompatible with describing their interpretative behaviour with a deterministic theory of language.

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46 Cf. Putnam, Hilary: *Representation and Reality* (Cambridge MA 1988), S.46.

47 «semantics for Natural Kind Terms»

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The «Right» Approach

THE «RIGHT» APPROACH

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Arguments for social change are very often based on references to human rights, but I want to maintain that there is a problem with talking about rights, especially human rights. And I want to suggest that the purpose of improving society might be better served if we were to talk less about rights. I do not want to deny that people have rights or to propose that people be relieved of their rights. I simply believe that we can make quicker progress toward what we want to achieve if we conduct our discussions in a vocabulary other than that of rights. What vocabulary that might be, I shall try to indicate shortly.

First, however, let me describe the problem that I see with discussions conducted in terms of human rights. It has become extremely common for those wishing to advocate improvements in society to do so in a way that involves references to human rights, or basic human rights, or absolute rights of human beings. If, for example, I want to advocate a change in laws restricting what can be said in the press, I can refer to the public's right to know what is going on. If I want to argue for programs designed to reduce malnutrition, I can do so by invoking the basic right to a minimally acceptable diet. And if I want to support improvements in the treatment of employees by their employers, I can base my position on a reference to a fundamental right of each person to be respected by others. The outline of such appeals to human rights is fairly familiar. The existence of certain rights is asserted, and it is argued that because these rights are there, the rules and regulations of society must be altered in a certain way — in order to conform with the rights. It is as though we were pointing out rocky outcroppings on the map of a territory to be settled and saying, «Here, because these outcroppings are placed as they are, we shall have to run our roads like *this* and lay our fields out like *that*.» Like the rocks, the rights are there; and the problem is to arrange human society in harmony with them.

So far so good. But now comes the problem. At the practical level, we do not always agree on what rights there are; and when we do agree about the existence of certain human rights, we do not always agree about their relative importance. It is as though — to continue the earlier simile — we had different maps of the new territory. We are not in complete agreement either as to how many outcroppings there are or as to how large they are. Examples are all too easy to find. Some humans are convinced that one of the sexes has a fundamental right to rule over the other. Others are just as sure that no such right exists. Some maintain that women have the right to have an abortion; others deny it. Some hold that adults have a right to find sexual pleasure with willing partners of either sex; others disagree. We may agree that there are both inheritance rights and a right to a fair share of the earth's resources, but we may disagree on which is to take precedence. We may agree that indigenous populations have a right to their traditional way of life and that settlers have a right to theirs — without being able to agree as to which is more important.

And cases such as these do not even tax the imagination. We are used to people making claims of rights within certain boundaries of custom and tradition. But what if those boundaries are surpassed? What if someone claims that we have no right to use animals for food? Or that the *other* sex is really the one that has the right to rule? The problem presented by such cases is that all too often people accustomed to thinking in terms of rights will not know what to *say* to such claims — except perhaps, «You're just wrong.»

Put more generally, the trouble with practical discussions involving references to human rights is that they cannot have recourse to any generally accepted method for the rational resolution of disagreements. The rights are asserted to exist and to have a particular degree of importance, but there is no agreement on what might count as proof that such and such a right does or does not exist — or that it does or does not have a certain level of importance.

If we really were discussing rocky outcroppings in a new territory and found that our maps were at variance with each other, there would be no such problem. We would all know how to go about settling our disagreement: the area would simply have to be surveyed. Now, might it not be that disagreements in practical discussions conducted in terms of rights could be resolved in an analogous manner? Just as we could call in expert surveyors to settle the disagreement about the outcroppings, could we not settle disagreements over rights by calling in experts in the appropriate field — presumably social philosophy? In fact, we *should* be able to do so. The problems that beset discussions of rights at the practical level *ought* to be susceptible of resolution through work at the theoretical level. Unfortunately, that does seem to be feasible. What we find at the theoretical level is more disagreement — although now it is disagreement over the kinds of things that rights are and the ways in which their existence and relative importance might be established. There is no shortage of theories, to be sure; but there *is* a shortage of agreement on the essential points. The expert surveyors, as it were, are not even in agreement on what it is for something to *be* a rocky outcropping — or what it is for someone to talk about one.[1]

At this point I should be careful to emphasize that I am not denying the possibility of a correct theoretical analysis of rights or talk about rights. I definitely believe that one is possible. I am not ready to defend Jeremy Bentham's denial of existence to all but legal rights.

> There are no other than legal rights; — no natural rights — no rights of man, anterior or superior to those created by the laws. The assertion of such rights, absurd in logic, is pernicious in morals.[2]

Nor am I willing to concur with Alasdair MacIntyre's caustic characterization of belief in natural or human rights as «one with belief in witches and in unicorns.»[3] All that I *do* wish to deny here is the likelihood of any rights theory being agreed

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1 For a survey of theories about rights, see Morton E. Winston's anthology, *The Philosophy of Human Rights* (Belmont, California: Wadsworth, 1989).

2 *Pannomial Fragments* in *The Works of Jeremy Bentham*, vol. 3 (New York: Russell and Russell, 1962) 221.

3 *After Virtue: A Study in Moral Theory* (Notre Dame, Indiana: University of Notre Dame Press, 1981) 67.

upon by social philosophers at any time in the near future. Accordingly, I shall not argue here for what I take to be the correct theory of rights.[4] To do so would only be to add to the theoretical disagreement, and I can see no present practical value in doing that. Perhaps I am being overly pessimistic, but the history of theoretical disagreements in rights theory does not inspire much optimism.

The nature of the trouble I see with basing advocacy of social change on a reference to rights should now be clear. When differences arise over the existence and importance of rights, there simply is no means at our disposal of resolving them in a rational manner. And inasmuch as the need for social change in many areas is absolutely imperative, I submit that we would be well-advised to find a basis for advocacy that is more readily amenable to rational agreement. It *may* not be a case of Rome burning while the theorists theorize — and then again, it may be even *worse* than that.

The next question then is whether it is possible to discuss the improvement of society in a vocabulary that does not include rights. Can we deliberate about changes in the social order without referring to rights as the bases for the changes advocated? I submit that we can — and that this should involve no great difficulty, since it has been done before.

The classical Greek philosophers, if you will remember, were not given to framing their theories of the ideal polis in terms of rights. It is not, of course, that they could not speak in those terms. Plato, for example, certainly seems to be using the concept of rights when he describes, at *Republic* 549, the kind of father likely to produce a timocratic son...

> a brave father, who dwells in an ill-governed city, of which he declines the honors and offices, and will not go to law, or exert himself in any way, but is ready to waive his rights in order that he may escape trouble.[5]

The point, though, is that the classical Greek social theorists did *not* tend to phrase their own political ideas in terms of rights. And in fact we today have little difficulty in explaining their theories on the improvement of society without invoking that concept. They tended rather to think about political matters in terms of an end in view. Aristotle thinks of the polis as having the particular purpose of enabling people to achieve eudaemonia — and proceeds to reason out how things ought to be ordered with that end in mind. And Plato identifies «our aim in founding the State» as «not the disproportionate happiness of any one class, but the greatest happiness of the whole... .»[6]

Perhaps it would not be wise to dwell on the Greeks, for many of us today might want to reject certain of their specific suggestions about the arrangement of

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[4] I do think that there is a correct theory, and I discuss it in my own courses. I just do not feel that arguing for it is the best way to promote the solution of important social problems.

[5] Trans. B. Jowett, *The Dialogues of Plato*, vol. 1 (New York: Random House, 1920), 807.

[6] *Republic* 420. Op. cit. 683.

society.[7] There is, however, no need to suppose that their method of approaching the problem leads inexorably to their particular conclusions. We might even be able to argue against certain of their proposals on the grounds that these can now be seen *not* to be conducive at all to the end in question. But be that as it may, the possibility clearly exists that we can conduct our own discussions about improving society as they did — with reference to some end in view that is not specified with reference to rights.

If we could agree upon such an end, then we would be able to reason empirically about how to obtain it. The question of whether or not a particular change in the arrangement of things in society would be conducive to that end would be a factual question of the sort we know how to handle. With a certain amount of determination and a lot of trial and error, we could find out whether a suggested change would be an improvement or not.

The major problem here, of course, lies with the specification of the end. Is it possible — if we cannot agree on basic human rights — that we *can* find some description of society which we can all accept as what we would like to see? If there are many different lists and rankings of human rights, are there not likely to be just as many different conceptions of the kind of society toward which we are working? I believe that, in fact, most of us do already share such a conception of the end in view. We may have widely divergent notions about the specific steps essential to reach it, but I think we agree — at a sufficiently high level of abstraction — on what we are trying to attain.

Suppose, for example, that we learn in some way of the existence of a small planet inhabited by intelligent beings somewhere in the far reaches of the galaxy. Suppose we learn further that the inhabitants of Planet X have arranged things in their society in such a way that they are able to lead extremely satisfying lives. The present generation there rates their society as a smashing success, and there is every reason to believe that succeeding generations will be equally satisfied. Suppose now that we know nothing else about this society — nothing whatsoever about the particular nature of their social arrangements — their customs, laws, and regulations. All we know is that because of whatever arrangements they have, they are heartily satisfied with their existence.

The interesting question now is whether knowing this and nothing more we might consider going to Planet X to help improve things. Improve things? I submit that the fact that most of us will find this question odd is a strong indication that most of us do in fact agree on a basic description of the end in view for society. Simply put, most of us would be quite happy with the realization on earth of the sort of society just described for Planet X. Those of us who want to see existing societies on our own planet improved do in general want to see them changed in the direction of the hypothetical society on Planet X. But could not a question be raised about the moral advisability of steering by reference to Planet X? If we seek ways to approximate a society whose members lead satisfying lives, and we do so without reference to rights, can we be sure of being on the right track morally? This question may well strike certain sorts of consequentialists as more than odd, since some of

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[7] A classic example is Karl Popper. V. his *The Open Society and Its Enemies*, vol. 1, *The Spell of Plato* (London: George Routledge & Sons, 1945).

them may wish to maintain, for example, that any course which leads to the greatest number of happy lives in the future is by definition the moral course. But deontologists may be less quick to dismiss the question. Some of them, even without phrasing their concerns in terms of rights, may wish to object that steered in such a way, our course might well veer into immorality. In the course of establishing social arrangements under which the members of society could have satisfying lives, might we not permit or even require certain immoral actions?

I do not wish to dismiss this question as pointless, because I do not wish to reject all deontological moral theories outright. Nor do I want to claim that the end-in-view approach to social improvements will automatically avoid immorality. What I do want to maintain is that this approach is in fact more likely than the «right» approach to lead to arrangements which, while satisfying the requirements of morality, will enable the members of society to lead satisfying lives. A full explanation of how I conceive the harmonization of moral requirements with the realization of the end in view would necessitate a basic discussion of the nature of morality and so cannot be attempted here.

The prima-facie attractiveness of the hypothetical society on Planet X is significant. It indicates that we already *have* an end with respect to which proposals for social improvements can be empirically evaluated. If a change is proposed in some existing social practice, the question to ask is whether or not the institution of that change would constitute a step forward toward a situation in which all members of the society in question would live satisfying lives. And that is the sort of a question which can be answered by trial and error if nothing else. The answer to such a question does not have to await the resolution of theoretical philosophical questions which may or may not be achieved in another hundred years.

Moreover — and this may be even more important — if reasoning about improvements in society is conducted with reference to such a generally accepted end, the results of field research become applicable. And field research is one of the things at which humans are rather adept. If we want to achieve a society in which people live highly satisfying lives, we can certainly obtain valuable knowledge by studying the correlation of satisfaction and dissatisfaction with existing social arrangements. If some small society somewhere already has better arrangements with respect to the end in view, we ought to find out what they are.

What *sort* of field research might be profitable? Obviously it could involve direct questioning of populations about how satisfied with life they are.[8] Comparative data on this question for different societies around the globe could be quite instructive. The question could be asked in various ways: «Would you leave this society if you had a good chance?» «Do you hope your children will lead the kind of life you have?» «Would you advise someone to settle in this society?» Other kinds of data that could prove useful include comparative information on suicide rates, stress-related physical and mental problems, and certain types of crimes.

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[8] What I have in mind is something like the «satisfaction» polls done periodically by the Gallup organization. V., for example, George Gallup, Jr., *The Gallup Poll: Public Opinion 1993* (Wilmington, Delaware: Scholarly Resources Inc., 1994) 223-224.

To be sure, caution would have to be exercised in the analysis of data resulting from any such research. In particular, in cases in which different groups within a single society showed significantly different levels of satisfaction, special study would be required to determine whether the satisfaction of some might not be causally related to the dissatisfaction of others. If we are interested in discovering social arrangements which will enable all members of society to have satisfying existences, then presumably we are *not* interested in arrangements that produce satisfaction for some in a way that has to produce dissatisfaction for others—«marvels for the rich but...privation for the worker» for example.[9]

In time, rights theorists may reach agreement on the nature of rights, and the «right» approach to the resolution of crucial social problems may become more productive. Until then, I advocate an end-in-view approach because I am convinced that it is a swifter and surer way of resolving problems that cannot wait to be remedied.

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[9] Karl Marx, *Economic and Philosophical Manuscripts* in T.B. Bottomore, trans. and ed., *Karl Marx: Early Writings* (New York, McGraw-Hill, 1964) 124.

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Meaning Realism and the Rejection of Analyticity

MEANING REALISM AND THE REJECTION OF ANALYTICITY

Manuel Liz

1.— Introducing Some Terms of Art

My aim in this paper is to argue that there are ways to maintain a non-holist meaning realism even though one does not accept any analytic/synthetic distinction (hereafter **A/S** distinction). In order to characterize with precision the kind of meaning realism that is going to be defended and the kind of analyticity that is going to be rejected, we will introduce in this section some helpful terms of art. They will be used through all our discussion. They are inspired in Boghossian (1993), but there are some important differences.

1.1.— Minimal Meaning Realism and Meaning Irrealism.

Let us begin with a minimal characterization of meaning realism. Being minimal, this characterization will serve us to make clear what is entailed by different sorts of irrealisms with respect to meaning, and it will be also useful in order to define minimal realisms and irrealisms concerning semantical properties others than meaning.

Minimal Meaning Realism: It is constituted by the acceptance of two very simple theses, namely

1. the thesis that there exist in fact semantical properties such that particular cases of «to mean that ...» would refer to, and
2. the thesis that these meaning properties can be instantiated in our world.

Beyond these two simple but fundamental theses of minimal meaning realism, let us consider another related thesis:

3. the thesis that some of these meaning properties really are instantiated in our world.

It is clear the different force of these three theses. The third thesis is stronger than the second one, and the second thesis is stronger than the first one. The third thesis entails the second one, and the second thesis entails the first one, but converse

relations of entailment would not be true. Now, in relation to the denial of each one of these three theses, we can define the following relevant positions:

> **Meaning Nihilism:** It consists in the denial of the first thesis and, as a consequence, it implies the denial of both the second one and the third one.
>
> **Meaning Eliminativism:** It consists in the denial of the second thesis and, as a consequence, it implies the denial of the third one too.
>
> **Meaning Absenteeism:** It consists in the denial of the third thesis.

Although the third thesis does not properly belong to minimal meaning realism, it entails its two theses. Because of that, in order to maintain a minimal meaning realism it would be enough not to be a meaning absenteeist and to subscribe that third thesis. However, a rejection of the third thesis is compatible with the acceptance of the two theses of minimal meaning realism. One can be at the same time a minimal meaning realist and a meaning absenteeist. Really, the meaning realism we are characterizing really is minimal.

With the help of the new terms and concepts we have just introduced, we can now define meaning irrealism as follows:

> **Meaning Irrealism:** It consists in being meaning nihilist or meaning eliminativist.

With respect to any supposed property other than meaning, we could also define a minimal realism, a nihilism, an eliminativism, an absenteeism, and an irrealism in a very similar way. Specially, that would be possible for other semantical properties like analyticity, synonymy, and so on.

In general, with respect to any supposed property X, we could define a **Minimal X-Realism** as constituted by the following two theses: 1) the thesis that there exists in fact a property refered by «X», and 2) the thesis that that property can be instantiated in our world. **X-Nihilism** would be the denial of the first thesis of that minimal X-realism, **X-Eliminativism** would be the denial of the second one, **X-Absenteeism** would be the denial of the thesis that property X really is instantiated in our world, and **X-Irrealism** would consist in being X-eliminativist or X-nihilist. To be X-nihilist would entail to be X-eliminativist, and to be X-eliminativist would entail to be X-absenteeist, but not the other way around. Let us follow saying that:

> **To maintain a Factualist Thesis about property X** is to maintain the first thesis of a minimal X-realism.
>
> **To maintain a Non-Factualist Thesis about property X** is to maintain an X-nihilism.
>
> **To maintain an Error Thesis about property X** is to maintain an X-eliminativism.
>
> **To maintain a Non-Error Thesis about property X** is to maintain the second thesis of a minimal X-realism.

We usually maintain a non-factualist thesis about supposed properties such as to be a squared circle or to be the last natural number. We usually assume that these expressions do not describe any property at all. On the other hand, even if we are factualists about properties such as to be able to go back in time or to have a

temperature below absolute zero, we usually maintain an error thesis about them. We assume that these properties cannot be instantiated in our world. Finally, with respect to other properties such as to be 200 years old (for human beings), we usually are absenteeists. In fact, we accept that these properties exist and we guess that they have not been instantiated in our world, but we are neutral concerning whether they can be instantiated or not in it. Theses 1, 2, 3, and the rest of our definitions try to preserve these intuitions.

1.2.— Absenteeism and Realism Without Determination.

With the help of the above mentioned concepts and distinctions, we have introduced the thesis of absenteeism. In contrast with Boghossian (1993)'s approach, that thesis will be very relevant for us in order to interprete the position of Quine (1951) and Putnam (1966). As it was indicated, there are important differences between meaning absenteeism and meaning irrealism. Now, these differences can be generalized saying that to be an X-absenteeist would not be the same thing as to be an X-irrealist. To be an X-irrealist necessarilly entails to be an X-absenteeist, but not the other way around. In order to be an X-realist it is enough not to be an X-absenteeist, but it is not enough to be an X-absenteeist in order to be an X-irrealist.

Related with any minimal X-realism, there would be another position that is worthy of attention. It could be characterized as maintaining some sort of X-realism without determination in the following sense:

X-Realism Without Determination: It is constituted by the acceptance of two thesis with respect to the supposed property X, namely

1. the tesis that there exists in fact a property refered by «X», and
2. the thesis that there is no cogent procedure to determine whether something has X or not.

We need to say something about the notion of cogent procedures of determination. Cogent procedures of determination would not be effective procedures. Cogent procedures of determination can be defeated. Cogent procedures of determination can be proposed and rejected, they can be orientated in a more or less empirical way, and they can be more or less accurate within certain limits. Unlike effective procedures, cogent procedures of determination sometimes can produce wrong results. But, no procedure of determination would count as a cogent procedure unless 1) it is assumed its truth conduciveness with respect to the problem in question and 2) it is assumed that that truth conduciveness can be explained as a matter of natural, conceptual, or conventional laws.

If there are cogent procedures to determine whether a property is or not instantiated, there are facts of the matter able to decide that question. Determinate properties would be properties for which there are facts of the matter concerning whether they are instantiated or not. «To have certain electrical charge», «to be made of wood», «to be in Spain», and «to be one of the members of the Spanish Parliament» are examples of descriptions that refer to determinate properties in that sense. There exist cogent procedures to determine whether something has them or not. They are truth conducive procedures, and its truth conduciveness can be explained with the help of natural, conceptual, or conventional laws. On the other hand, «to be the next winner in a horse race», «to be the more important scientific

discovery in the history of humanity» or «to be events that occur simultaneously in time» would be examples of descriptions that refer to non-determinate properties. There is no cogent procedures to determine whether something has or not these properties.

A meaning realism without determination would explicitly accept the above first thesis of minimal meaning realism according to which there exist in fact semantical properties such that particular cases of «to mean that ...» would refer to. But, a meaning realist without determination rejects the existence of cogent procedures to determine whether something has or not any of these meaning properties. Such a meaning realism without determination would be a meaning realism without any way to determine whether meaning properties are or not instantiated. That very peculiar kind of meaning realism is important because it offers a possible way to make compatible some rejections of the **A/S** distinction with certain meaning realisms of a holist kind. In this sense, sometimes it has been sugested that the Quinean rejection of the **A/S** distinction would only entail a meaning irrealism concerning isolated statements, but not a meaning irrealism concerning something like the meaning of whole scientific theories of the world. One of the most crucial references for this interpretation is the following:

> My present suggestion is that it is nonsense, and the root of much nonsense, to speak of a linguistic component and a factual component in the truth of any individual statement. Taken collectively, science has its double dependence upon language and experience; but this duality is not significantly traceable into the statements of science taken one by one.
>
> The idea of defining a symbol in use was, as remarked, an advance over the impossible term-by-term empiricism of Locke and Hume. The statement, rather than the term, came with Frege to be recognized as the unit accountable to an empiricist critique. But what I am now urging is that even in taking the statements as unit we have drawn our grid too finely. The unit of empirical significance is the whole of science. (Quine, 1951)

By itself, the nonsense of distinguishing factual from linguistic components in the meaning of individual statements goes against the «Quinean» **A/S** distinction without entailing any meaning holism. In section 6, we will arrive to a position close to the first part of the above quote of Quine (first paragraph) trying not to be engaged in its second part (second paragraph). According to the holist interpretation of this passage there could be at least a meaning realism compatible with the «Quinean» rejection of the **A/S** distinction; namely, a meaning realism concerning the meaning of the whole of science. Even though it is not possible to determine whether something has or not that meaning, it must exist. That was, for instance, the main point of Acero (1993) in his commentaries to Boghossian (1993)'s arguments against the compatibility of meaning realism with the Quinean rejection of the **A/S** distinction.

However, the sort of meaning holism that is assumed in that interpretation only is a meaning realism without determination, and this is a very weak thesis. As we have said, it would make impossible to have cogent procedures for determination of meanings. And without being able to determine whether something has or not the property of having certain meaning, it is difficult to see how such a meaning realism could be engaged with the thesis that some meaning properties really are instantiated in our world. Anyway, it is also difficult to see how it could be engaged with the second thesis of a minimal meaning realism according to which meaning properties really can be instantiated in our world. It is plausible to argue that to maintain a non-error thesis about meaning requires to have cogent procedures to determine whether, with respect to meaning properties, the modal property of being-able-to-be-instantiated-in-our-world is or not instantiated itself in our world. And it is plausible to argue that if we have these cogent procedures, then we also have cogent procedures to determine whether something has or not those meaning properties.

Because of that, it is difficult to imagine any minimal meaning realism not being a meaning realism with determination of meaning properties. Meaning realism without determination would not be a minimal meaning realism. The same would be true for whatever X-realism without determination. In general, it is plausible to argue that any minimal X-realism must be an X-realism with determination, and that no X-realism without determination would be a minimal X-realism.

Meaning realism without determination is not a meaning nihilism. As we have said, such a meaning realism accepts the first thesis of meaning realism. There is a difference between a meaning realism without determination and a meaning nihilism that denies the existence of meaning properties. However, that difference is a very tiny one. It only consists in the acceptance by the former, but not by the second, of the second order existential statement that there exist at least one property such that a particular case of «to mean that ...» would refer to. That would be the only difference. A meaning realist without determination even cannot have any cogent procedure to answer any particular case of the question «What «to mean that ...» means?».

Boghossian (1993) argues that one cannot be a minimal meaning realist rejecting at the same time the **A/S** distinction. Against that, we will defend in this paper the compatibilist view that it is possible to do both things. Certainly, we could assume an irreductible meaning holism maintaining this way a meaning realism without determination able to be compatible with certain rejections of the **A/S** distinction. However, as we have just said, that would be very weak. Also we could be meaning realists maintaining a mere absenteeism concerning the **A/S** distinction. But, mere absenteeism would not entail any irrealism about analyticity. The compatibilist view we want to defend involves both a minimal meaning realism and an irrealism about analyticity.

The structure of the paper is as follows. We begin in Section 2 noting that «analyticity» must be understood above all as a philosophical technical term, i.e., as a theoretical term introduced in order to explain certain phenomena. Section 3 offers a crude objection to the **A/S** distinction, an objection based on a direct and simple argument against the possibility of having an adequate definition of analyticity. Without such a definition, analyticity becomes a non-determinate property or, simply, a property that does not exist. The important thing is that our argument supports a non-factualist thesis, i.e., a nihilism, about analyticity that does not depend on any sort of meaning irrealism. After that, in section 4 we closely examine Boghossian (1993)'s argument against the compatibility of any minimal meaning realism with nihilism about analyticity. The general conclusion will be that even a minimal meaning realism that accepts that if the meanings of some statements are fixed then so too are their truth properties is compatible with a nihilism concerning the **A/S** distinction. Section 5 follows a different route. Apart from the reasons examined in preceding sections to be irrealists about analyticity, there would be also some normative reasons against it. The sort of normative reasons that together with an absenteeism one can find in Quine (1951) or Putnam (1966). As we have said, mere absenteeism by itself does not entail any irrealism about analyticity, but with the help of these normative reasons it does. The interpretation that with respect to analyticity we will offer of both authors also would be compatible with a minimal meaning realism. Finally, section 6 is about the semantical property that sentences like «all bachelor are unmarried» are supposed to have when we say that they are trivial cases of analyticity. (Note: for the sake of simplicity, we will only consider

two truth values, truth and falsity, and we will not make any relevant distinction among sentences, statements, and propositions)

2.— Analyticity as a Philosophical Technical Term

First of all, it must be noted that analyticity is not an univocal notion. The reason of that is very simple: «analyticity» is above all a philosophical technical term. Strictly, our theories about analyticity are not theories about it. They are theories about certain other phenomena, and analyticity is not among these phenomena but among the things that are intended to explain them.

Grice and Strawson (1956), and Putnam (1966), among others, held another opposite view. For them, the **A/S** distinction is a semantical phenomenon that does in fact exist, and the only real problem is about its nature. They are commited with a minimal realism about the **A/S** distinction maintaining that

> where there is agreement on the use of the expressions involved with respect to an open class, there must necessarily be some kind of distinction present.

That is just the perspective that in this section I want to critizice. It seems to me radically misguided for several reasons. Firstly, what does «agreement on use» mean here? Surely, not only mere coincidence in the results of a classification (analytic statements versus non-analytic ones). Two or more classifications can lead to the same result, they can be extensionally the same, even if they are guided by quite different sets of criteria and theoretical commitments. «Agreement on use» requires something more. In our case, it would require agreement on some philosophical beliefs with respect to the **A/S** distinction itself. But, in this last sense, it is clear that there is no such agreement and that, therefore, the existence of a real **A/S** distinction can be questioned.

Really, there is some agreement. Although it is only a certain agreement on the target class of phenomena that could achieve an unified explanation through analyticity. But, very often it has happen in the history of science and philosophy that the error was just in thinking that a given class of phenomena were needed of an unified explanation. So, we must consider «analyticity» as a philosophical technical term, and we must not see any class of, let us call them, trivial cases of analyticity as proving anything about the existence of an **A/S** distinction well stablished in our languages.

2.1.— «Non-Quinean» Notions of Analitycity.

The perspective we have adopted has very important consequences. Suppose, as Boghossian (1993) does, that you think of analyticity as being something like «truth by virtue of meanings». Then, surelly, specially if you wish to use that idea to explain where logical truth comes from, you will be led to the need of distinguishing two different concepts of analyticity. Using Boghossian's terminology, we can say that you will be in the need of distinguishing between «pure» analyticity and «impure» analyticity. Unlike impure analyticity, pure analyticity must have no dependence on logic. With respect to pure analyticity, facts about meaning must be sufficient for the truth, without any contribution from either empirical or logical facts. Only this way you could use analyticity to explain logical truth.

At this point, Boghossian maintains that the concept of pure analyticity only can make sense if there is some modality distinct from the logical that may be used to

define the dependence of truth values on meaning that it aims to articulate. In the case of pure analyticity, Boghossian says, «by virtue of» must become some sort of metaphysical necessitation, or something like that. Really, if we work with the, let us call it «Quinean», notion of analyticity according to which

> a statement is analytic iff it is true by virtue of meanings and independently of facts,

the conclusion reached by Boghossian is compelling. As we haved said, in the case of pure analyticity, the «facts» in question must also include logical facts, and «by virtue of» must become some sort of irreducible metaphysical necessitation between meanings and truth values.

However, being «analyticity», as it is, a philosophical technical term, there are other ways to look at the phenomena that it intends to explain. The «Quinean» notion is not the only possible notion of analyticity. In relation to our actual languages, there are other very different notions of analyticity. And with respect to some of them we do not need the appeal to any sort of metaphysical necessitation between meanings and truth values. That would be so simply because there are different notions of analyticity that do not make any primary reference to things like «meaning» or «truth». Let us think, for instance, on these other notions of analyticity:

> A statement made in a language is analytic iff it is one which all speakers of that language accept and for which they cannot give any reason apart from the one consisting in the fact that they are speaking that language.

> A statement made in a language is analytic iff it is one which any speaker of that language can never give up without leaving to speak that language.

It is true that the above notions are not only semantical notions of analyticity. «To be a speaker of a language», «To accept a statement», «to give reasons», «to give up a statement», etc., have important pragmatical components. But, why must analyticity be only a semantical notion? These other notions of analyticity could be so general and powerful as the «Quinean» one can be. Moreover, the modal qualifications present in these definitions offer an alternative to the metaphysical necessitation that Boghossian is calling for.

I am not endorsing any of these, let us call them, «speaker-based» notions of analyticity. I only want to note that there are other «non-Quinean» ways to understand it. More, an analogous distinction between pure and impure analyticity could be drawn from these «speaker-based» notions, one that would not require any appeal to an irreducible methaphysical necessitation. Consider, for brevity, only the last one. Besides the lack of precision of the concepts here involved, we could define impure and pure analyticity with respect to it as follow:

> Impure analytic statements made in a language are the ones which a speaker can never give up unless the speaker gives up some of the logical statements of that language.

> Pure analytic statements made in a language are the ones which a speaker can never give up even though the speaker gives up all the logical statements of that language.

With respect to synthetic statements we would have the following:

> Synthetic statements made in a language could be defined as the ones which a speaker can ever give up without giving up any of the logical statements of that language.

Once the language were fixed, pure analyticity would be fixed too. And, once the logical statements of a language were fixed in a way or another, a distinction between analytic (both pure and impure) statements and synthetic ones would appear. The important thing is that, with these concepts at hand, we do not need any irreducible metaphysical necessitation that in the case of pure analyticity connects meaning with truth. It is true that we need some modal qualification of the possibilities and impossibilities mentioned in the above definitions. In fact, they are suppose to have some sort of pragmatical modal force. But we do not need to metaphysically connect meaning with truth because we do not have here any primary reference to meaning or truth.

What, then, about the concepts of analytic truth and logical truth? Simply the following. We could define analytic truth in a language as the class of all analytic statements (pure and impure) of that language and, given certain logic, we could define logical truth in a language as the class of all analytic statements of that language that are not pure analytic statements. From that point of view, pure analyticity could not be directly used to explain how logical truth ultimately comes from meaning. But, pure analyticity in the above sense still could be able to restrict the class of possible logical structures that are allowed for any given language. Both analytic truth and logical truth would be something derivated from the use of a language, not something derivated from meaning. Let us note that if we adopt that last perspective about analyticity, analytic truths in the language we are speaking would be unrevisable. Some of them in an absolute sense, and some of them in a sense relative to the logical structure imposed over the language. However, not every true statement would be analytic. Many true statements could be given up by the speaker of the language without any logical change being, in this way, synthetic. Notice also that we are not making any claim about the rationality of these revisions. All that is in the game is the pragmatical modal fact that nobody would speak certain languages if some very special statements that can be made in these languages are given up.

As we have said, we do not claim to endorse these alternative «speaker-based» notions of analyticity. We are only stressing the fact that in relation to our actual languages there is not only one way to look at the phenomena linked to the philosophical technical term «analyticity». Really, there are a lot of possible characterizations of analyticity others than the «Quinean» one.

3.— A Nihilist Argument Against Analyticity

Now, we can ask «Why to accept one of these possible notions over all the other ones in order to adequately define analyticity?» «Why to accept, for instance, the ‘Quinean’ notion of analyticity instead of some of the ‘speaker-based’ notions?» «Which one, if any, of the multiple notions of analyticity could lead to the adequate definition of analyticity?» These and other similar questions would finally lead to another one that we are going to confront in this section, namely «What are the conditions that the adequate definition of analyticity ought to satisfy?» The analysis

of some of these conditions will offer us a very direct argument against the **A/S** distinction, one that supporting a non-factualist thesis about analyticity will not depend on any meaning irrealism.

3.1.— The Adequate Definition of Analyticity.

So, let us concentrate on what would be required by the adequate definition of analyticity. Any plausible candidate to define analyticity would have to adopt the following general form

(D) s is analytic iff B(s)

where «s» stands for any statement, and «B» refers to particular properties others than analyticity which must be haved by s. The adequate definition of analyticity also has to be a true statement able to cover trivial cases of supposed analyticities like

(t) «All bachelors are unmarried».

If some particular definition is the adequate definition of analyticity, then it must be a true statement such that statements like t are analytic statements in the sense defined by it. There is nothing odd up to this point.

The problem comes when we decide that a particular definition of analyticity is in fact the adequate one. The adequate definition of analyticity must be a true statement able to cover trivial cases of supposed analyticities like t. But, it must be not only that. It must also reflect some simple features haved by these trivial cases of analyticity. As we are going to see, the adequacy of a definition of analyticity in these conditions would entail the analyticity of the definition itself just in the defined sense. And, to put it in a nutshell, the problem is that it is very difficult, if not impossible, to obtain any statement of that kind.

In order to make clear what the problem is, let us call **analyticity$_1$** to the property of being analytic just in the intended sense offered by certain particular definition of analyticity, and let us call **analyticity$_2$** to the property of being analytic in the sense in which trivial statements like t are supposed to be. Now, let be Di any particular definition of analyticity. If Di is the adequate definition of analyticity, it would have to introduce analyticity$_1$ through a true statement like

Thesis 1 (Di): s is analytic$_1$ iff B(s).

Because the theoretical character of analyticity$_1$, it is not necessary that to be analytic$_1$ entails to be analytic$_2$. But, if Di is the adequate definition of analyticity, to be analytic in a trivial sense must entail to be analytic in that defined sense. In other words, the following thesis would hold:

Thesis 2: IF s is analytic$_2$, THEN s is analytic$_1$.

As we have said, the adequacy of a definition of analyticity would also require to maintain some simple features haved by trivial cases of analyticity, i.e., by analyticity$_2$. Particularly, we claim that it would require to accept at least the following three thesis:

Thesis 3: IF s is analytic$_1$, s' is analytic$_1$, and s iff s', THEN (s iff s') is analytic$_1$.

Thesis 4: IF s is analytic$_1$, THEN (s is analytic$_1$) is analytic$_1$.

Thesis 5: IF B(s) and s is analytic$_1$ iff B(s), THEN B(s) is analytic$_1$.

First thesis would consist in the clausure of analyticity$_1$ under logical equivalence maintained among analytic$_1$ statements. Second thesis says that to state that something is analytic$_1$ is itself an analytic$_1$ statement. Third thesis says that to state that something has the properties something has if and only if it is an analytic$_1$ statement is itself an analytic$_1$ statement.

Theses 3, 4, and 5 come from the field of analyticity$_2$. We assume that an analogue of these three theses hold for analyticity$_2$. Really, it is not easy to prove conclusively this point. Analyticity$_2$ is a very fuzzy matter. However, it seems plausible to presume that these three theses reflect in fact important features haved by analyticity$_2$. Let explain this. With respect to thesis 3, «s iff s'» would be a statement enough simple to guarantee that, being true, if it is not analytic in a trivial sense, it is because s or s' are not analytic in that sense. If «s iff s'» is true, then the trivial sense in which s and s' are analytic transmites that analyticity to «s iff s'» itself. Thesis 4 would hold no more than if a statement is analytic in a trivial sense, then to state that also must count as a trivial case of analyticity. It is important to note that thesis 4 is previous to any consideration concerning whether analyticity$_2$ and, consequently, analyticity$_1$ must be exclusively understood as semantical properties and not, for instance, as properties derived from the use of a language. In the field of analyticity$_2$, it is quite unproblematic to assume that, as a matter of fact, if a language contains the predicate «is analytic», then that predicate ought to be also applied to statements saying themselves that something is analytic. In general, if it is trivial to say something, it must be also trivial to say that it is trivial.

The philosophical theory of analyticity can try to reject thesis 4, perhaps by means of a hierarchy of analyticities relativized to different levels of language. But, in that case, it would have to reject also thesis 2. Not every analytic$_2$ statement made in a language would be analytic$_1$. This is very important, because far from offering a better explanation of the phenomenon of analyticity such as it is supposed to be present in our languages, that revisionist move would suggest (against Grice, Strawson, and Putnam) that analyticity is a theoretical concept designed to explain certain other phenomena, and that perhaps these phenomena could be better explained without any appeal to analyticity. In other words, to accept both theses 2 and 4 is the best thing a theory of analyticity could do in order to achieve an adequate definition of analyticity.

To make clear thesis 5 in relation to analyticity$_2$, we would need to distinguish between, on the one hand, to say that a statement is analytic in a trivial sense and, on the other hand, to say that it has the sort of properties that would make it just a trivial case of analyticity, whatever these properties may be. That distinction is not explicitly present in the context of the analytic$_2$. But the important point is that if it were present in a way or another, then for any statement having those properties it would have to be a trivial case of analyticity to state that it has them. It would be difficult to understand how any statement could be analytic in a trivial sense because the having of some sort of properties without being analytic in that trivial sense to state that it has just these properties. In sum, the adequate definition of analyticity would need to assume these features maintaining the above theses 3, 4, and 5.

3.2.— The Problem of the Analyticity of the Adequate Definition of Analyticity.

Now, let us offer an argument in order to show that, in these conditions, if Di is the adequate definition of analyticity, it would have to be itself analytic$_1$.

1- Suppose any analytic$_2$ statement like t.

2- t is analytic$_1$. [from step 1 and thesis 2]

3- It is analytic$_1$ to state that t is analytic$_1$. [from step 2 and thesis 4]

4- B(t). [from step 2 and thesis 1]

5- B(t) is analytic$_1$. [from step 4, thesis 1, and thesis 5]

6- IF it is analytic$_1$ to state that t is analytic$_1$, B(t) is analytic$_1$, and t is analytic$_1$ iff B(t), THEN it is analytic$_1$ to state that t is analytic$_1$ iff B(t). [an instance of thesis 3]

7- It is analytic$_1$ to state that t is analytic$_1$ iff B(t). [Modus Ponens from steps 3, 5, and 6]

8- In consequence, Di is analytic$_1$. [universal quantification over step 7]

From the above argument, we finally obtain the following important thesis:

Thesis 6: If Di is a true statement making an equivalence between analyticity$_1$ and some set of properties B, and such equivalence is able to cover analyticity$_2$ preserving the features we have indicated through theses 2-5, then Di must be itself analytic$_1$.

The adequate definition of analyticity would have to satisfy thesis 6. This is the final requirement. And it is a very important requirement because it entails a crucial problem if we like to accept the existence of a property that, in these conditions, can be called «analyticity». The problem is that it is not easy to offer any adequate definition able to satisfy thesis 6 and that, being «analyticity» a philosophical technical term, without any such definition we must consider the supposed property of analyticity as a non determinate property or, simply, as a property that does not exist.

Of course, neither a «Quinean» definition of analyticity nor any of the «speakers-based» definitions would be able to be themselves analytic$_1$ in the sense expressed in thesis 6. It is plausible to argue that the only way to satisfy that requirement would be through a definition making equivalent analyticity$_1$ and analyticity$_2$, and such that the equivalence were itself analytic$_2$. If analyticity were defined as the property of being a trivial case of analyticity, and that definition were itself a trivial case of analyticity, then the definition of analyticity would really be analytic in the sense defined by such definition. But, the analyticity that is required for the definition of analyticity itself cannot merely be a trivial one if to be analytic in some defined sense, i.e, to be analytic$_1$, is a theoretical property depending on our assumptions and theories. While «analyticity» follows being a philosophical technical term refering to a concept so strongly dependent on our assumptions and theories, the definition of analyticity could not be analytic$_2$.

As a matter of fact, statements like Di, unlike t, never are trivial cases of analyticity. Philosophers interested in analyticity try to define it just because the

intended definition of analyticity is not a trivial case of analyticity. From this point of view, we could say, against Putnam (1966), that analytic statements cannot be only trivial ones. Being «analyticity» a philosophical technical term, if there are analytic statements at all, there must be some non-trivial cases of them. At least, there must be one non-trivial case of analyticity, namely, the definition of analytic itself.

So, with respect to any definition of analyticity guided by our philosophical assumptions and theories, we must think of it as being analytic just in the defined sense, i.e., as being analytic$_1$, and we must think of it as not being a trivial case of analyticity, i.e., as not being analytic$_2$. And the crucial problem is that we do not have any definition of analyticity able to satisfy both conditions. All of that would entail to be very skeptical about whatever notion of analyticity and to maintain a non-factualism, it is to be nihilist, with respect to the property of analyticity itself. Or, at least, it would entail to refuse analyticity as a determinate property. The lack of any adequate definition of analyticity beyond the trivial cases of supposed analyticities lead us to maintain a realism without determination about it or, simply, to maintain that there is no such a property. But, in spite of that skepticism about analyticity, we would not be committed with any skepticism about meaning. We can follow accepting the determination of meanings and to be minimal meaning realists.

4.— Boghossian's Argument for the Incompatibility of Minimal Meaning Realism with Nihilism about Pure Analyticity

Boghossian (1993) displays a crucial argument for the incompatibility of minimal meaning realism with nihilism concerning pure analyticity. His argument can be easily generalized to affect any meaning realism able to accept, at least as a consequence, that if the meaning of a statement if fixed, then there is a fact of the matter as to whether the truth values of the statement are fixed too. Really, any minimal meaning realism with determination of meanings that also accepts truth properties as determinate properties would have to endorse that thesis. Even a minimal meaning realism that accepts a meaning absenteeism would have to endorse it.

Boghossian's argument is developed on the assumption of a «Quinean» notion of analyticity as «truth by virtue of meanings» considered as a determinate property. Boghossian tries to show that any minimal meaning realism would be incompatible with a non-factualist rejection of pure analyticity in the «Quinean» sense. Although the use of pure analyticity would be here dispensable, we prefer not to modify the original format of his argument in that respect. The argument in question is the following. According to Boghossian, the non-factualist rejection of pure analyticity would quite directly entail that

(1) for any statement, there are not facts of the matter as to whether the statement is true by virtue of its meaning.

And (1) would entails that

(2) there are not facts of the matter as to whether the statement is such that, if its meaning properties are fixed, then so too are its truth properties.

But, if meaning properties and truth properties are determinate properties, as we have assumed, there must be these last facts of the matter able to decide whether the truth values of the statement are or not fixed if its meaning properties are fixed. That is, our minimal meaning realism implies that (2) is false. And if our minimal meaning realism is correct, and (2) is false, then (1) must be false too. If we accept a minimal meaning realism, then we cannot accept the non-factualist rejection of pure analyticity. In any case, Boghossian concludes, both minimal meaning realism and the non-factualist rejection of pure analyticity, i.e., a nihilism about it, are incompatible.

It must be noted that Boghossian's argument is compatible with an absenteeism about analyticity. Even if there were facts of the matter as to whether a statement is true by virtue of its meaning, it could happen that, up to now, no statement were true by virtue of its meaning. The argument really has a strong «prima facie» plausibility. But, in spite of that «prima facie» plausibility, I think that it is possible to resist it. To put it in a nutshell, it is possible to resist Boghossian's argument because it could be false that (1) entails (2)! Let us put it in other words. «(1) entails (2)» is logically equivalent to «(not-2) entails (not-1)», and to say this is to say that

(V) IF (not-2) there are facts of the matter as to whether the statement is such that, if its meaning properties are fixed, then so too are its truth properties, THEN (not-1) there are facts of the matter as to whether the statement is true by virtue of its meaning.

Conditional (V) really has a very naive appearance. If «to be true/false by virtue of the meaning» were simply the same than «to be true/false if the meaning is fixed», then (V) would be a logically valid conditional. The realist acceptance of meaning and truth as determinate properties would logically imply the factualist acceptance of pure analyticity. That is the core idea of the argument.

But, the innocence of conditional (V) is only a superficial one. As we are going to argue, it is possible to break the entailment and to defend the possible falsity of (V) under some interpretation. The important point is that if (V) can be false in some coherent interpretation, then one could be a minimal meaning realist with determination even though one does not believes in analyticity.

4.1.— A Short Study on «Facts of the Matter» and «by Virtue of».

To begin with, let consider the set of properties for which there are «facts of the matter» as to whether something have them or not. Conditional (V) has to do with the thesis of the closure of that set under the composition of properties through the relation «by virtue of». That closure implies the truth of the following general conditional (V*) from which (V) can be interpreted as a particular case:

(V*) (for arbitrary properties F and G) IF there are facts of the matter that decide whether x being F is G, THEN there are facts of the matter that decide whether x is G by virtue of being F.

But, it is clear to me that properties are not in general closed in that sense, and that conditional (V*) is not always true. Let be, for instance, the properties «to be a house» and «to be green». There are «facts of the matter» as to whether something being a house is green. But, it would be very odd to say that there are «facts of the matter» as to whether something is green by virtue of being a house. At first look,

one is tempted to say that yes, that there are such ‘facts of the matter», and that these «facts of the matter» say us that nothing is green by virtue of being a house. But, that would be misleading.

In Lanzarote (one of the Canary Islands) all houses are green. It is traditional in that island to paint houses in green, even there is a law forbidding to paint houses in colours others than green. With respect to Lanzarote, it is possible to say that some things really are green by virtue of being houses. In Lanzarote, one could argue, there are social practices and legal rules able to give an unified sense to the claim that some buildings are green by virtue of being houses. These social practices and legal rules are able to establish one so special link L between the properties of being a house in Lanzarote and being green that the following would hold:

There is a link L between to be a house in Lanzarote and to be green such that, for all x, (if Lx then (if x is a house in Lanzarote, then x is green)).

In other words, the property of being green by virtue of being a house in Lanzarote is a determinate one. There could be cogent procedures based on the social practices and legal rules that are able to stablish that link L. And according to these cogent procedures, one could decide whether in Lanzarote some things are green by virtue of being houses. In consequence, there are «facts of the matter» to decide whether something is or not green by virtue of being a house in Lanzarote.

It is very important to adequately distinguish the last thesis from the thesis consisting in that there are «facts of the matter» to decide whether houses in Lanzarote are green by virtue of the above mentioned social practices and legal rules. Of course, there would be also cogent procedures and «facts of the matter» to decide whether houses in Lanzarote are green by virtue of certain social practices and legal rules. In that case, these cogent procedures would be based on other more basic nomicities. What we are defending is just that both properties would be determinate ones and that this is the way we understand «by virtue of».

But if this is so, then the supposed «facts of the matter» by which nothing is green by virtue of being a house ought to include the relevant and important condition «not being in Lanzarote». And the problem would be that, by the same token, other conditions ought to be included. But, how to know them? There would be no way to determine them completely in an unified way, and «facts of the matter» that do not admit any clear determination are not «facts of the matter». In spite of appearances, it can be argued that there are not «facts of the matter» as to whether in general something is green by virtue of being a house. There are «facts of the matter» as to whether something is or not a house, there are are «facts of the matter» as to whether something is or not green, and there are «facts of the matter» as to whether something being a house is or not green. There are also «facts of the matter» as to whether in Lanzarote something is or not green by virtue of being a house. Of course, there can be «facts of the matter» as to whether in other times, places, or circumstances something is or not green by virtue of being a house. And there are «facts of the matter» to decide whether houses in Lanzarote are green by virtue of certain social practices and legal rules. But, it is possible to argue that in spite of so many «facts of the matter», there is no «facts of the matter» as to whether in general something is or not green by virtue of being a house. Conditional (V*) is not valid.

But, even if conditional (V*) is not necessarily true, conditional (V) has a very different scope and it could be always true. Is it the same with respect to properties like to-have-certain-meaning and to-have-certain-truth-value than with respect to properties like to be a house and to be green? We can try the same strategy with respect to the first pair of properties than with respect to the second one. In order to show that Boghossian argument fails, and that (1) does not entail (2), we only need to prove that (V) can be false under some coherent interpretation! All we need to do is to make a coherent interpretation of (V) according to which (V) is false!

Our minimal meaning realism accepts that, being truth properties and meaning properties determinate properties, there are always «facts of the matter» to decide whether the truth properties of a statement are fixed being fixed its meaning properties. Moreover, our minimal meaning realism even could accept that sometimes these «facts of the matter» are able to decide the truth for some statements! And the problem is: Why must that be enough in order to maintain a factualism about the property of analyticity considered as a determinate property, and some sort of **A/S** distinction? Factualism about analyticity considered as a determinate property would state that there are always «facts of the matter» able to decide whether a statement is true by virtue of its meaning. A non-error thesis about such analyticity would state that there can be at least one case in which these «facts of the matter» decide the truth. The non-error thesis would entail factualism. Now, our problem with factualism is that if it is possible that (not-2) does not entail (not-1), then a door closed for analyticity is opened for meaning realism.

Beside «facts of the matter» able to decide whether a statement is such that, if its meaning properties are fixed, then so too are its truth properties, that is (not-2), what factualism about analyticity, that is (not-1), would need are «facts of the matter» that decide whether the statement has or not the property of «being true by virtue of its meaning». But, to say this would be to say that factualism about analyticity needs «facts of the matter» to decide whether, having the statement the determinate meaning it has, it bears or not the special link (let us call it SL) between its meaning and its truth value that «by virtue of» intends to refers to.

It would be a very especial link because it would not be merely the link (let us call it ML) that there could be between meaning and truth values just when the truth of a statement is fixed if its meaning is fixed too. We could say that what «facts of the matter» in (not-2) try to detect is this last link, that is a ML, whereas what «facts of the matter» in (not-1) try to detect is the first one, that is a SL. But, more precisely, what is the difference between a SL and a ML? Let us speak in general about links L among meanings M and truth values V of statements s. Now, (not-2) can be simply interpreted as follows:

(not-2') For all statement s, there is a link L between its meaning M and its truth value V such that (if Ls then (if Ms is fixed, then too is Vs)).

On the other hand, it would be possible to consider «Vs by virtue of Ms» as a mere paraphrase of «if Ms is fixed, then too is Vs». In this way, we could interprete also (not-1) as saying that

(not-1') For all statement s, there is a link L between its meaning M and its truth value V such that (if Ls then (if Ms is fixed, then too is Vs)).

In that case, (not-2') would really entail (not-1'), and hence above conditional (V) would hold. However, as in the case of things being green by virtue of being houses, there is another possible, and perhaps more demanding, interpretation of the expression «by virtue of» that appears without any restriction in (not-1). According to that point of view, (not-1) ought to be interpreted not as (not-1') but as

(not-1'') There is a link L between the meaning M of a statement and its truth value V such that, for all statement s, (if Ls then (if Ms is fixed, then too is Vs)).

Here, «Vs by virtue of Ms» is not simply a paraphrase of «if Ms is fixed, then too is Vs». As it is showed in the order of quantifiers, «by virtue of» would require the existence of an unified and very strong link between meanings M and truth values V. Now, it is clear the crucial difference between a SL and a ML. The existence of a SL would imply the existence of a ML, but the existence of a ML would not imply the existence of a SL. In other words, just because «There is a link L such that for all statement s (...)» entails, but it is not entailed by, «For all statement s there is a link L such that (...)», the minimal meaning realism from which (not-2) is a consequence would not entail any factualist thesis about analyticity considered as a determinate property, that is (not-1), when such factualist thesis is interpretated as (not-1'').

If there is the link mentioned in (not-1''), really it would be a very especial link. That link would be the property a statement has when its meaning and its truth value are so especially related that the statement has the truth value it has «by virtue of» having that meaning. And this property is not simply the property a statement has when its truth value is fixed once its meaning too is. Lots of statements can have that second property, in a limit case one different property by each statement, without having the first one.

In other words, there is a point in which the links among meanings and truth values could be so heterogeneous that we cannot give any determinate sense to the expression «to be true/false by virtue of the meaning alone». Faced with this situation, we would have to deal with two main problems. One of them would be the kind of existence that something like a SL could have. Could it be, for instance, a merely disjunctive existence? Really, I do not know. Anyway, the second problem is more important. If that supposed SL exists at all, there cannot be any «facts of the matter» able to help us to detect it. As in the case of things being green by virtue of been houses, the mere links of (not-2'), or (not-1'), would not be able to do the work because we would need to determine all of them. And there are too many. Simply, the supposed property of being such SL is a non-determinate one. In the case it exist as a very complex disjunctive property, it cannot be but a non-determinate property.

In consequence, there is a coherent interpretation of conditional (V) according to which (V) could be false. Therefore, some sort of minimal meaning realism with determination of meanings in combination with considering truth values also as determinate properties could be true without being true that, for any statement, there are «facts of the matter» as to whether it is true by virtue of its meaning. Moreover, the truth values of some statements could be fixed if their meanings are fixed too. In the limit case, there could be so many different routes between meanings and truth values as different statements of that kind can be made in a language. So, a minimal meaning realist could say, for instance, that having «bachelor» and «unmarried» the meanings they have, «all bachelors are unmarried» is a true statement. A minimal

meaning realist could say that without being married with analyticity. Given the way the truth is fixed in the case of «all bachelors are unmarried» once its meaning is fixed, that truth even could have a very strong modal force. But, this is another story.

5.— Normative Rejections of the A/S Distinction

We have examined some problems concerning both the definition of analyticity and the appeal to the general notion of «truth by virtue of the meaning» in order to reject the compatibility of minimal meaning realism with nihilism about analyticity. With respect to the first topic, I have maintained that, being «analyticity», as it is, a philosophical technical term, the adequate definition of analyticity would have to be itself analytic not being trivial (more preciselly, that it would have to be analytic$_1$ without being analytic$_2$), and that we do not have any idea about which definition of analyticity could have these characteristics. With respect to the second topic, I have suggested an interpretation of «truth by virtue of the meaning» according to which one could be a minimal meaning realist with determination of meanings and truth values without believing in analyticity. That interpretation of «truth by virtue of the meaning» would call for a so special property of statements, a special link SL between their meanings and their truth values, that it is not easy to imagine how we could appeal to such a property, if it exists at all, in relation to our actual languages.

Nevertheless, even if we cannot imagine how both an adequate definition of analyticity and a SL between meanings and truth values can be possible in relation to our actual languages, that does not mean that we cannot imagine other situations in which these things were available. The following is a propousal to imagine one such situation. After showing that in such a situation it would be possible to maintain a minimal realism about analyticity, we will argue that there are important normative reasons against trying to transform that imagined situation in something real. In other words, what we will argue in this section is that, even if it were conceivable (in a wide sense of «conceivable») some sort of minimal realism concerning analyticity, there would be also normative reasons to maintain an irrealism about it.

5.1.— Imagining Analyticity Step by Step.

STEP 1: In section 3, we have maintained that the adequate definition of analyticity would have to admit theses 1-5 and, therefore, to be itself analytic in the defined sense, i.e., it would have to be analytic$_1$. Moreover, if the definition of analyticity depends on our assumptions and theories, it would have to be analytic$_1$ without being analytic$_2$. The nihilism defended in that section was based on the difficulty to satisfy that claim. However, as it was indicated there, we could imagine an adequate definition of analyticity according to which analyticity$_1$ and analyticity$_2$ were equivalent and that equivalence were itself analytic$_2$. That would require for analyticity to fail to be a philosophically loaded concept and to be defined in a trivial analytic way. Now, we can try to construe a definition of analyticity able to satisfy these conditions. Let be, for instance, this explicit definition of analitycity:

(EDA) An analytic statement =Def One in which it is stated 1) an explicit definition or 2) a logical consequence of explicit definitions or

3) the analyticity of an explicit definition or 4) the analyticity of an analytic statement.

Again, if it were required, we could distinguish out of EDA some kind of pure analyticity from an impure analyticity, and so on. The important thing is that EDA itself is an explicit definition and that, therefore, EDA is analytic$_1$. EDA would be able to meet also theses 1-5 of section 3.

STEP 2: Could EDA be adopted itself as a trivial case of analyticity? Well, the trivial character of a statement is only a psychological/epistemic question relative to a subject or group. And it can change if that subject or group changes. What is non trivial for a subject or group at some time can be trivial for other subject or group at the same time, or it can become trivial for the same subject or group at another time. According to that, EDA could be adopted by some subject or group as a trivial case of analyticity, even it could become trivial for ourselves.

STEP 3: With respect to the truth of EDA, we can assume that explicit definitions are always true. Assuming also that to be an explicit definition is part of the meaning of some statements, there would always be some «facts of the matter» as to whether the truth values are fixed once meanings are fixed too. In the case of explicit definitions, that assumption would fix the truth once the meaning is fixed and we notice that we are faced with an explicit definition.

STEP 4: Now, if we want to use the notion of «truth by virtue of the meaning», the only problem would be the one coming from the difference between mere links and a special link. But, this would be only a problem refered to the way things are made. The route from mere links ML to a special link SL only depends on the kinds of links there are between meanings and truth values. The point is that we could imagine a language used in such a way that the links determining the truth values once the meanings are fixed are always of a kind easily interpretable as the extension of a single natural property. This could be so out of our decisions and conventions or by the force, let us say, of the nature of things. Moreover, we could imagine a set of possible worlds in which, given certain decisions and conventions, or given the properties and relations present in these worlds, all actual and possible languages are of that kind. Being in any of the worlds of that set, we could have a special link SL, and not only mere links ML. The existence of such a SL would have even some sort of modal force with respect to that set of worlds. It would be in some way necessary. Really, we would have something like a «caeteris paribus» analyticity restricted to that set of worlds, and that set of worlds could be extremely broad.

5.2.— Some Classical Normative Reasons Against Analyticity.

We have been arguing that is not necessary the existence of the supposed property called «analyticity». According to our analyses of both the adequacy of the definition of analyticity and the property that could support it in a determinate way when the notion of «truth by virtue of the meaning» is introduced, analyticity would not be something necessary. But, in the situation we have just described, we would really have some sort of «caeteris paribus» analyticity with all the features required. Now, the important question is: Why not to have that specific «caeteris paribus» analyticity?

I think that there is a negative answer to that question, a negative answer based on normative reasons. But, before to see these reasons, let us consider another related question also with a negative answer based on normative reasons. The question is: Why not to have in general analytic statements?

The classical normative reasons against analyticity stress that it would not benefit the progress of knowledge, scientific progress in particular. Furthermore, in some cases analyticity would paralize knowledge and scientific development. In Quine (1951), for instance, there is a very important normative component in his rejection of the existence of analytic statements others than explicit definitions. It is not only a question of fact, but a normative question. Quine not only argues that there are not, in fact, analytic statements others than explicit definitions. He thinks that there ought not to be any analytic statement but the ones being explicit definitions. Quine maintains a certain absenteeism with respect to analyticity, but it is an absenteeism embeded in certain normative thesis. At this point, it is important to realize that he does not maintain any error thesis about analyticity involving the strong modal notion of necessity. He does not maintain that analyticity cannot be instantiated. And he does not maintain either, at least in the context of Quine (1951), any non-factualist thesis nor a factualist thesis with respect to it. Instead, he argue for some Normative Thesis like:

(NT) If there exists a property expressed by «is analytic», then it has never been instantiated by statements not being explicitly definitional, and that is what ought to happen because that property ought not to be instantiated by statements not being explicitly definitional (consequently, all tokens of the statement «s is analytic», where s is not an explicit definition, have been false up to now; and all tokens of the statement «s is analytic», where s is not an explicit definition, always ought to be false).

NT does not suppose the existence of the property of analyticity, and it does not suppose either that that property is necessarily uninstantiated. Really, NT is more than a thesis about the falsity up to now of all tokens of the statement «s is analytic». It involves also a modal notion. But, it involves only a normative one. NT says that the property of analyticity has never been, and ought not to be, instantiated.

The situation of absenteeism with respect to analyticity is such that we do not know which one of the following exclusive theses is true:

a) Analyticity has never been instantiated and ought not to be instantiated, but it could be instantiated and it is a genuine property.

b) Analyticity has never been instantiated, it ought not to be instantiated, and it could not be instantiated, but it is a genuine property.

c) Analyticity has never been instantiated, it ought not to be instantiated, and it could not be instantiated; moreover, it is not a genuine property.

Theses a), b), and c) really have much more content than NT. But, I cannot find in Quine (1951) any compromise with any of them. All that is in Quine (1951) is

NT. Because of that, his position does not entail any error thesis and it does not entail any non-error thesis either, and it is compatible with both a non-factualist thesis and a factualist thesis about analyticity. With respect to the analyticity not introduced by means of explicit definitions, I think that we must interpret him simply as being neutral about these things. In other words, Quine (1951) is not a minimal realist concerning analyticity, but he is not an irrealist either. He simply is an absenteeist that endorses a normative thesis like NT.

It seems to me that Putnam also could be interpreted as maintaining a similar view. Putnam (1966) maintained that outside the field of explicit estipulations, both in formal and natural languages, if «to be analytic» really refers to a genuine property, that property has never been instantiated and it ought not to be instantiated. Up to a certain extent, both Quine and Putnam think of analyticity as entailing unrevisability, and their worries are about the unrevisable character that analytic statements must have. It would block our knowledge, our scientific knowledge in particular, to declare as analytic, and therefore as immune from revision, any statement other than explicit definitions. Only truths estipulated by means of explicit definitions ought to be analytic and immune from revision.

Now, let us go back to our previous question «Why not to have in general analytic statements?». Analitical statements, we can read in Putnam (1966), could provide the advantage of brevity, intelligibility, capability of prediction of some linguistic uses, and so on. But, explicit definitions in formal and natural languages can do all that important economic work. What is more important, once it is assumed that analytic statements must be unrevisable, no other kind of analyticity would have these benefits without the danger of being an obstacle to the progress of knowledge.

We cannot confuse the thesis that analyticity entails unrevisability with the converse thesis. Actually, unrevisability does not entail analyticity. We could have, for instance, some sort of «a-priory» knowledge able to be unrevisable without being based on analyticity. More, all true statements ought to be considered unrevisable. And we cannot confuse either unrevisability with our knowledge of it. We can always be wrong about the unrevisable character of any statement. But, even accepting these things, analyticity would entail unrevisability. Hence, no revisable statement could count as analytic.

Nevertheless, the relevant point concerning analyticity and revisability lays in other place. In order to show that there is no analytic statement other than explicit definitions because all other supposed analytic statements could become revisable, it is necessary that the revisable character of these supposed analytic statements cannot be merelly interpretable as a change of the meanings involved. That it is always plausible such a «change-of-the-meaning» interpretation was one of the main theses of Grice and Strawson (1956) against Quine (1951). On the other side, Putnam (1966) was one of the main opponent to that «change-of-the-meaning» thesis. It would be convenient to have a look at the arguments. Roughly, the argument for the revisable character of any supposed analytic statement, other than explicit definitions, was as follows. Suppose that

(1) «All things being A are B»

is considered an analytic statement not introduced by means of an explicit definition. Now, suppose that we become to have good reasons to maintain that

(2.1) «All things being C are A, and only things being A are C», and

(2.2) «Some things being C are not B»

are true statements. Suppose also that, in a heuristic and theoretical sense, being C becomes more relevant that to be B. In that situation, the argument says, the good epistemic policy would be to reconsider the supposed analytic character of (1) and to modify its truth value. So, we could say that (2.1) and (2.2) in that situation suggest that

(3) «There are things that are A without being B»,

and that, in consequence, (1) cannot be a true analytic statement.

Against that argument, the «change-of-the-meaning» interpretation would reply that the revisability and, hence, the rejection of the analytic character of (1) is only apparent. The predicate «being A» appearing in (1) has not the same meaning that it has in (3). To admit (2.1) and (2.2) does not entail the falsity of (1), but a change of meaning in the predicate «being A». If (1) really is a true analytic statement, (3) cannot refute it. (1) and (3) would be simply talking about different things.

Curiously, that «change-of-the-meaning» interpretation of revisability favours conventionalism. Its defense of analyticity leads to conventionalist views of knowledge and science. In our case, the consequence would be that the choice between (1) and (3) could not be guided by our beliefs about «being A». No improvement in our beliefs about «being A» would entail to be in a better epistemic position in order to decide between (1) and (3). It is only a question of choice of a particular meaning (or language, or conceptual scheme) instead of another one.

The move of Putnam (1966) in order to avoid that «change-of-the-meaning» interpretation is worth of attention. Putnam maintains that when a statement like (1) is not introduced by means of an explicit definition, as it is by assumption, the predicate «being A» is always, as a matter of fact, a law-cluster concept. The meaning of law-cluster concepts is constitued by a cluster of laws. Supposed analytic statements not introduced by means of explicit definitions are no more than one of these laws in the meanings of the law-cluster concepts involved. A statement like (1) itself would be one of these laws for the meaning of «being A». The important point is that any of these laws can be abandoned without destroying the identity of the law-cluster concept. Just in the same way, says Putnam, as a man can be irrational from birth, or can have a growth of feathers all over his body, without ceasing to be a man. So, the meaning of «being A» would not have changed enough from (1) to (3) to affect what we are talking about. (1) simply is one of the various laws that constitute the law-cluster concept «being A», and we know that any of these laws can be abandoned preserving «being A» the meaning it has.

5.3.— Why not to Adop EDA and Give Reality to the Situation in Which EDA Is an Adequate Definition of Analyticity?

We have remembered a very important story about why not to have analyticity in general outside the field of the analyticity introduced by means of explicit definitions. Now, it is time to come back to the problem of why not to have even the kind of specific analitycity offered by explicit definitions. In other words, why not to adopt EDA and give reality to the situation described through STEPS 1-4? Why not to transform our imagined situation in something real, achieving the economical benefits of analyticity?

Let put aside the problem of going from mere links to a special link. Any definition of analyticity that wants to use the notion of «truth by virtue of the meaning» will have to deal with that problem. Really, it is a crucial problem, but it only depends on the way things are made, or can be made. To my view, it is very unplausible that it can exist a special link out of those mere links, even if we do our best efforts to get it (see again STEP 4). If I am right, even if we are not nihilists concerning the property of analyticity itself, and even if we accept that there exists a genuine property such that «analyticity» refers to, we would have to be eliminativists. But this is another question. The point I want to emphatize now is that, besides the problem posed by that special link, there would be normative reasons against the adoption of EDA as a trivial case of analyticity, and therefore as an adequate definition of analyticity. These reasons are related with the story we are just remembered, and with the robust character of the meaning of law-cluster concepts.

In short, the predicate «to be analytic» that we find in EDA also ought to be considered as refering to a law-cluster concept, to a concept with a meaning that is not exclusively determined by EDA itself. Thus, we would have to refuse that EDA ought to be really understood as an explicit definition, with the consequence that EDA would not be analytic$_1$ because it does not satisfy the definition of analyticity proposed.

The answer to the question «Why not to have an explicit definition of «to be analytic» such as the one offered by EDA in the situation above described?» would be, therefore, the same than for Quine and Putnam was the answer to the question «Why not to have analyticity in general?». We ought to refuse the situation in which EDA could be an adequate definition of analyticity because that situation would block our knowledge. To consider that analyticity is not a law-cluster concept and that we can explicitly define «to be analytic» would block our knowledge of what the supposed property called «analyticity» can be. Curiously, it could even be an obstacle to the discovery that in fact there is no such a property.

In consequence, with independence of the problem whether there can be or not special links between meanings and truth values, and not only mere links, we ought not to be in the situation described by STEPS 1-4. Apart from the reasons examined in preceding sections in order to be irrealist about analyticity, there would also be normative reasons against it.

6.— Trivial Cases of Analyticity and How to Interpret Them

We have been maintaining a nihilism concerning analyticity. Also, we have argued that our nihilism in compatible with a minimal meaning realism, even with one that accepts that sometimes truth values are fixed once meanings are. The existence of a special link among meanings and truth values is very doubtful. We have also noted that even if it is possible to imagine situations in which an adequate definition of analyticity is possible, there would be normative reasons to resist being in them. Furthermore, even if we were in such situations, we would follow having problems with respect to the possible existence in our languages of a special link able to suport a notion of analyticity as «truth by virtue of the meaning». So, even if we fall to be nihilists about analyticity, we would have to be eliminativists.

To put it in a nutshell, for someone who loves analyticity, the only available options are to consider analyticity as a non-determinate property, maintaining this way a realism without determination about it, or simply to be absenteeist. And the important thing is that none of these options supports any minimal realism with respect to it.

However, we cannot forget that there are trivial cases of supposed analyticities. It is time to say something about them. They are the linguistic phenomena over which theories about analyticity are proposed, and our skepticism about analyticity has to adopt a position about them. «All bachelors are unmarried», for instance, is very often adopted as one of these trivial cases of supposed analyticity in our natural languages. Really, lexical definitions and explicit definitions made in scientific and not scientific contexts offer us lots of cases of such supposed analyticities.

We cannot explicitly define analyticity saying that analytic statements are no more that explicit definitions (and logical consequences of explicit definitions, and so on). We have just argued that that kind of definition would not be an adequate one. But, this would be compatible with the fact that some statements with the structure of explicit definitions could be, in some sense, analytic. At least, they could be analytic just in the sense of being analytic$_2$. Really, if there is analyticity at all, statements with the structure of explicit definitions are the most plausible candidate for analyticity.

The problem is that there are very few, if any, pure lexical or explicit definitions able to institute an analyticity that cannot be revisable. That is, definitions where the definiendum is not a law-cluster concept. There are few, if any, cases of trivial analyticities that cannot be defeated. So, the same problem we have seen with respect to the explicit definition of analyticity emerges with respect to an overwhelming majority of cases of supposed pure lexical or explicit definitions that have the appearance of trivial cases of analyticity.

6.1.— «All Bachelors Are Unmarried».

Take, for instance, the statement «all bachelors are unmarried». Putnam (1966) maintained that some statements of our natural languages, statements like that one, really are analytic. Among the reasons to maintain that there was a crucial one: «bachelor» is not a law-cluster concept. It is not a law-cluster concept, says Putnam (1966:59), because there are not, and there will not be, exceptionless laws containing the term «bachelor». So, we could consider «all bachelors are unmarried» as very close to an explicit definition of «bachelor» and, therefore, as a trivial case of analyticity.

Putnam would be right if we understand «law» only in some narrow sense. But that narrow sense in no way is the only sense that could be relevant here. That «bachelor» is not, nor will be, a law-cluster concept is true only if the class of laws we are thinking about does not contain nomicities like legal laws, social rules, and so on. Certainly, these nomicities are not physical or natural ones, but they can be so exceptionless as natural laws are, and they are a really very important part of the way we understand terms like «bachelor».

Suppose, for instance, a society in which there is a fundamental, even exceptionless, legal law saying that bachelors and only bachelors are exempt from

pay certain marriage-tax that is obligatory for married people. The law is very basic or fundamental in the sense that it has achived a great importance in that society, and it maintains strong relationhips with a lot of other legal laws, social rules, etc. Suppose, as a matter of fact, that in such a society marriage has gradually adopted a great plurality of, civil and religious, forms so that at the present time it is not easy, for instance, to distinguishing married people from unmarried people that live together. There are also marriage ceremonies which are absolutely private, and so on. In general, it becomes very dificult to tell married from unmarried people. More, some people being in fact married try to keep out of sight their condition of being married in order to not paying the tax. Suppose that it is possible to do that in a lot of ways, so that not to pay the marriage-tax becomes the more relevant criterium to be bachelor, perhaps the only really operative criterium. Faced with this situation, we have two known options:

1- To say that some bachelors according to the law are not really bachelors or, in other words, that there is a change of meaning in the term «bachelor».

2- To say that some bachelors are in fact married.

With respect to the «change-of-the-meaning» option, it must be noted that not to pay the marriage-tax is a really important element of the concept of being a bachelor, an element that has not changed through the changes carried in the ways of being married. Because of that, it is plaussible to argue that «bachelor» has not changed its meaning. If not to pay the marriage-tax has become the more relevant criterium to be a bachelor and that criterium was always present in the society, then first option has not much sense. Second option is the more plausible one. But, second option entails that the statement «all bachelors are unmarried» would be false in that situation and, therefore, that it cannot be considered as an analytic statement.

The moral is that even statements like «all bachelors are unmarried» have a kind of analyticity$_2$, i.e., a kind of trivial analyticity, that could be revised. The importance of all those legal laws, social rules, and so on, is here decisive in order to reconsider that certain concepts really are law-cluster ones, and that every one of the supposed analytic$_2$ truths in wich they appear really could be given up.

But, this is not all. With relative independence of the above argument, there is another way to see meaning that also goes against the supposed trivial analyticity of «all bachelors are unmarried». If we consider seriously the fact that there can be concepts with a meaning determined by certain prototypes and some associated similarity conditions, it would be necessary to modify, or enlarge, our cluster analysis so that some concepts are not, or not only, law-cluster ones, but also prototype-cluster concepts. It is plausible to think that, for instance, our concepts of «chair», «window», «book», «mother», etc., are prototype-cluster concepts. And, perhaps, concepts like «bachelor» also really are prototype-cluster ones. Now, the important point is that even if we say of some of the prototypes Pi of a prototype-cluster concept A that «Pi is A», that statement could be false. It would be false if that prototype Pi comes to fail being a prototype for the concept A. For prototype-cluster concepts, particular prototypes could change without a change of meaning. If A is a prototype-cluster concept, then even if Pi is one of these prototypes it cannot be analytic the statement «Pi is A».

There is an important consequence of the above remarks. If most of our concepts are law-cluster or prototype-cluster ones, theories of analyticity of the sort provided by Kats (1972), based on the classical «kantian» idea that analyticity consists in some kind of redundant predication, would not work either. When law-cluster or prototype-cluster concepts are involved, redundant predication could be always false.

Among the multiple cases of supposed analyticities, the cases in which new symbols are introduced in a language through some intended explicit definitions occupy an important place. This happens very often in scientific and legal contexts. It is a common place, for instance, in the formal languages of mathematics and logic to make and use explicit definitions. The problem is «Are they really pure explicit definition?» «Are, for instance, the usual explicit definitions of logic pure explicit definition?» I think that these questions lack any definite answer. Not because, and this is here the crucial point, we do not have the relevant knowledges to decide these questions, but because there are not cogent procedures to determine in general whether something is or not a pure explicit definition. It depends on how each particular mathematical or logical simbol is related with mathematical and logical concepts, and on how these concepts get their meanings. To be a pure explicit definition is not a determinate property. That problem is even more evident when we go from mathematics or logic to the above mentioned legal contexts. Are the explicit definitions that one can find in legal codes really pure explicit definitions? There is no definite answer. In any case, the field of pure explicit definitions would be very narrow. Moreover, it is important to emphasize that even if there are cases of pure explicit definitions, and even if some statements really have the sort of analyticity that they are supposed to have when we say that they are analytic$_2$, we would have only mere links between their meaning and their truth values, not a special link. The supposed analyticity of the trivial cases of analyticity is only a trivial analyticity. It is not the kind of analyticity that philosophers were looking for. It consist only in the fact that, for some statements, their truth is fixed once their meanings are.

6.2.— Against Linguistic Arbitrariness.

Language is always conceptually motivated and engaged with reality. There are very few cases, if any, in which we come to use a new term only as the result of a pure explicit definition, without any other conceptual or factual contribution but the one that already is present in the old terms. There are always conceptual reasons to use the words, sentences, and languages in the way we use them. These conceptual reasons have to do with the rest of our beliefs and knowledges about the language we are speaking and about the world. Also, there are always externalist components of meaning that make very difficult to explain how it can exist a property exhibiting the classical features attributed to analyticity (for instance, unrevisability), and how we could know and detect analyticity through a knowledge of the meaning of the involved expressions. There are few cases, if any, in which different terms do not involve different sets of statements, theories, criteria of attribution, prototypes, or different externalist components. (With respect to what an externalist account of meaning would imply for the truth conditions of «s is analytic», see the brief but interesting paper of Pretri, 1992).

In spite of that, different terms can have the same meaning, and it can have sense to speak of synonymy and translation if meaning is not reducible to any of

these particular statements, theories, criteria of attribution, prototypes or externalist components. The background of meaning is very plural and heterogeneous. This the reason why it is so difficult to obtain an unified adequate definition of analyticity from the particular philosophical senses proposed for analyticity. Neither the «Quinean» notion of analyticity as «truth by virtue of the meaning», nor any of the various «speaker-based» notions of analyticity, nor an analyticity understood as no more than explicit definition, nor a «kantian» notion of analitycity as redundant predication, etc., would be able to support such an unified adequate definition of analyticity with the intended modal force and generality.

Any supposed analytic$_2$ statement is no more that an element of the background of the meaning of the terms involved, an element that very often can change without a change of these meanings. Because meaning has that plural and heterogeneous character, it is so questionable the existence of a special link able to institute the analyticity that some philosophers are looking for out of the mere links that, in fact, can exist among meanings and truth values in the cases where the truth is fixed once meanings are. The error was just in thinking that a given class of phenomena, linguistic phenomena where the truth values of some statements are fixed being fixed their meanings, would require an unified explanation in terms of a theoretical property called analyticity.

That approach has important consequences concerning synonymy (and, therefore, translation). From a classical perspective that is adopted by Quine, the notion of analyticity depends on the notion of synonymy. In other words, as a minimum, synonymy entails analyticity. If we follow adopting that perspective, our rejection of analyticity would entail a rejection of synonymy too. Nevertheless, there is a sense of synonymy according to which it would be still possible to have synonymy without having analyticity. It would be possible if all cases of «x means that ...» and «x has the same meaning that y» are always understood as statements that can be defeated. The situation would be one in which even if it is assumed the synonymy of terms T and T', that synonymy would not entail the philosophically intended analyticity of «All and only things being T are T'». Assuming for T and T' the meanings they are supposed to have, we could consider fixed the truth of that statement, even it could be considered analyticly$_2$ true. But, if that statement can be in fact false, it cannot be analyticly$_1$ true. And it can be false if determination of meanings and determination of the sameness of meaning is always made through cogent procedures of determination that can be defeated.

All supposed analytic statements must be placed in the background of meaning. We have seen that, against the «change-of-the-meaning» option, meaning cannot be completely identified with that background. Very often, the background can change without any change in the meaning. There would be only a kind of supposed analytic statements for which changes in their truth values would entail direct changes in the meaning; namelly, statements stablishing among several terms that kind of defeasible synonymy. Only in these cases the «change-of-the-meaning» strategy seems to be directly applied. We can accept that. However, it must be noted that to recognize that role for some supposed analytic statements does not entail to accept the kind of synonymy able to make analyticity possible. These statements would be only trivial cases of analyticity, i.e, analytic$_2$ statements. So, it would be possible to have a very useful concept of defeasible synonymy without being engaged in analyticity.

As we said above, conventionalism likes analyticity and the «change-of-the-meaning» strategy to protect analytic statements. The choice among different analytic statements would be only a matter of stipulation, it would be never a rational business. Conventionalism is a kind of relativism. But, there are also other kinds of widespread relativisms that have their roots in a «change-of-the-meaning» strategy without being conventionalists. Nowadays, these relativisms are very popular, specially in the so called «continental philosophy». According to them, whatever change in our beliefs could be reintepreted as a change of the meaning. There is not progress in knowledge, but proliferation of meanings. Here, like in conventionalism, the choice among different ways to speak is never a rational business. But, unlike conventionalism, these relativisms do not see that proliferation of meanings as a matter of stipulation. Proliferation of meanings is the effect of other causes for which we do not have any epistemic responsability. With respect to epistemic subjects, meanings are out of control. Mere stipulation is impossible, and so it is impossible analyticity too.

In that sense, conventionalism would be a relativism with analyticity. The difference between conventionalism and other relativisms without analyticity would be only one of emphasis on the control an epistemic subject can exert on the meanings its words and statements can have. The points of view we have defended go against both conventionalism and these other relativisms. The rejection of analyticity and the rejection of the «change-of-the-meaning» strategy that we have defended in the context of a non-holist minimal meaning realism with determination accept the first part of the quote of Quine (1951) made in section 1 without being engaged in its second part. And that compatibilist view would entail to be in a better position against these conventionalist and relativist moves. In a word, the rejection of analyticity is no more than the rejection of linguistic arbitrariness.

With analyticity there is something trivial and something non-trivial. About what is trivial, there is no much to discuss. Almost all of us do accept it. Once it is assumed that «bachelor» and «unmarried» have the meanings they are supposed to have in our actual languages, all of us do accept that «bachelors are unmarried» is a true statement. This is the trivial side of analyticity. But, analyticity is not that. These things are the phenomena that analyticity in the intended philosophical sense would have to to explain. That is the non-trivial side of analyticity. But it is a wrong side. The error was just in thinking that a given class of linguistic phenomena would require an unified explanation in terms of something called «analyticity».

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Epistemic Values in Science

EPISTEMIC VALUES IN SCIENCE

Valeriano Iranzo[1]

The aims of scientific activity change, in the same way as theories and methods change too as time goes by. In some periods, scientific research tended to show the perfection of Nature and, as a result, the infinite power and intelligence of the Creator. Certainly, nowadays these are not widely embraced goals in the scientific community. It is not only that aims change; there are axiological disputes in science as well. Scientific disagreements are not solely theoretical or methodological. Progress in science consists not only in developing new theories that are better in fulfilling epistemic values than earlier ones but in getting a deeper understanding of those values. But, is there any principle to guide axiological choices in science? Does the task of assessing the legitimacy of goals make any sense? In *Science and Values*[2], Larry Laudan puts forward several criteria to settle questions concerning the aims of science. According to him, scientists agree that the aims pursued are not arbitrarily fixed. In other words, the resolution of a discrepancy over aims is based on resaon, and the scientific community has come to terms on this judgement. Surely, Laudan is right. Neither aims of science are matter of subjective taste — that is I mean we could offer some argument against a revival of medieval scientific aims — nor professional scientists regard them as purely subjective preferences.

Laudan adopts a naturalistic stance, assuming that there is no cleavage between theories and methods, on one side, and goals, on the other. His reticulated model of science, developed in *SV*, emphasizes the interconnection among theories, methodological norms, and goals. Research about scientific aims has to take into account results in other levels because theories and methodological norms are basic to determine the legitimacy of the aim at issue.

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[2] L. Laudan, *Science and Values,* Berkeley, Los Angeles: Univ. of Calif. Press, 1984 (hereafter *SV*).

Laudan points out that he is concerned with *epistemic* aims or values. Thus, his task focuses on a naturalistic account of epistemological normativity in science, setting aside the muddy question of ethical normativity. From now on, I will use the words «aim», «goal», and «value» in an epistemological sense. The distinctive feature of epistemic goals — «explanatory power», «predictive accuracy», «truth», ... — is their close relation to the goodness of our beliefs. No doubt, scientific practice is not isolated and it is externally controlled by social goals. The politicians' decisions that determine the research policies and the technological applications are embedded in ideological and moral values. But, on the other hand, I think that the increasing control over citizens or the use of military power over other countries, and the eradication of infectious diseases — for instance — are not epistemic aims, although they might be aims actually pursued by scientific research (perhaps through the previous achievement of epistemic goals — think only of predictive accuracy concerning human behavior).

According to Laudan there are two main reasons on which to reject an aim: because it does not fit with current theories and practice, or because it is utopian, namely, because it is not realizable. I will call the former, the principle of coherence (PC), and the latter, the principle of realizability (PR). Let us begin with PC.

1. Coherence

To illustrate PC, Laudan offers two examples extracted from the historical record. The first is the shift, at the end of XVIII century and the beginning of XIX, from inductivism which refused to postulate unobservable entities to theories purporting the discovery of nature's deep structure. Against the inductivistic mainstream, Laudan refers to Hartley, Lesage and Boscovich, who were criticized by putting forward theories committed with inobservable entities. They had to develop a specific methodology (hypothetico-deductive), although its incompatibility with the aims widely acknowledged by the scientific community of that time somehow keeped them apart from it. However, confronted with a difficult choice, Hartley, Lesage and Boscovich did not modify their theoretical preferences. The empiricist qualms went by the board as they persisted in trying to understand the visible physical realm through an invisible one. Later, Herschel and Whewell claimed that «the axiology of empiricism was fundamentally at odds with the axiology implicit in scientists' theory preferences» (*SV* 59), and they gave strong and definite support to the postulation of unobservable entities.

Laudan offers another example to illustrate the feedback between theories, methods and aims. Now the aim in question is intelligibility, a goal strongly favoured by the cartesian way of doing science. From this point of view a good explanation involves some kind of reduction of the less intelligible to the more intelligible. Cartesian objections to newtonian physics estemmed from the notion of «action at a distance», a notion hardly intelligible for natural philosophers influenced by Descartes. Of course, the heart of the matter is the criterion of intelligibility, but by the 1740s — Laudan continues — Cartesians could not even convincingly show that the notion of action by contact (the only sort of action in a full universe such as that considered by Cartesians) was more intelligible than the notion of action at a distance. At this stage it became more reasonable to relinquish intelligibility as a desirable aim for science, since none of the physical theories had been entirely

successful in eliminating all suspicious notions, notwithstanding serious efforts in that direction.

Both examples tend to show that the process of goal revision roughly consists in «an examination of what our best (or, here, all our available) theories seem capable of achieving» (*SV* 61). Notice that although PC denounces situations where there is a gap between explicitly deffended aims and current scientific practice, it does not force us to abandon an aim. It is highly desirable to increase the degree of conceptual coherence but changing aims is not the only choice. We can also modify theories and methodological rules keeping aims fixed, as Laudan himself acknowledges.[3] Nevertheless, there is no general way of knowing what to do in these situations. A reasonable choice has to take into account all contextual information that could be relevant and, surely, members of the scientific community are the best qualified to accomplish the task.

PC seems to be a reasonable condition. Something is wrong when our best theories do not have the properties we regard as legitimate aims of science. But, despite the fact that PC rightly stresses the feedback between theories and methods, on one side, and aims, on the other, it is too «soft». To claim that what we can achieve with our best theories and methodological rules occasionally can lead us to revise science's epistemological aims is most definitely a rather imprecise statement. In fact, it would probably even be accepted by those who do not get on with a naturalistic standpoint. However, taking for granted that we cannot fix in advance how the revision has to be carried out, we could reformulate PC as involving the claim that *any goal whatsoever* may be revised when the results — e.g., theories and methodological rules — clash with it, despite repeated attempts in that direction. PC thus reformulated is no more precise than before but, at least, it is fully in line with naturalism, since it is not yet possible to set up a trascendent goal for science, a goal unaffected by the workings of the two other levels.

2. Realizability

According to Laudan, a necessary condition for a rational

— or legitimate — aim is its achievability: «... the rational adoption of a goal or an aim requires the prior specification of grounds for belief that the goal state can possibly be achieved.» (*SV* 51) This is PR in its general formulation.

It is worth stating that we cannot infer the utopian character of an aim, properly speaking, from the fact that no theory is successful in achieving it. In that case there

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[3] *SV* 59-60. Methodological rules for Laudan are hypothetical imperatives which relate a strategy to a goal according to this pattern: 'If one´s goal is y, then one ought to do x.´ The accuracy of a methodological rule consists in the degree of success it has showed in attaining the goal at issue. Therefore they have to be tested against the historical record. See his «Progress or Rationality? The Prospects for Normative Naturalism», *Amer. Philos. Quarterly* 24 (1987): 19-31. A thorough discussion of this view can be found in G. Doppelt («The Naturalist Conception of Methodological Standards in Science: A Critique», *Philosophy of Science* 57 (1990): 1-19). Laudan´s reply is in «Normative Naturalism», id., 44-59.

would be no difference between PC and PR. That fact is a necessary condition, not a sufficient one, to consider an aim as illegitimate. There is an outstanding difference as regards the resulting policies from PC and PR, since lack of conceptual coherence turns on the red lights and warns us that something is wrong — although PC does not tell us where the shortcoming is —, while irrealizability of an aim discards it inmediately as a legitimate one.

Laudan thinks that PR is uncontroversial. We usually regard as irrational those actions aiming for unachievable aims as inmortality or perpetual motion machines; in the same way, if we have good reasons to think that a goal is beyond our faculties, then the most rational course of action is to discard it. In other words, an aim or a value is utopian when «we have no grounds for believing that it can be actualized or «operationalized», that is, we do not have the foggiest notion how to take any actions or adopt any strategies which would be apt to bring about the realization of the goal state in question.» (*SV* 51) In words alien to Laudan's viewpoint we could say that there is no cleavage between an instrumental rationality and a teleological one working separately, rather, instrumental rationality assesses the realizability of aims, and the realizability of aims determines, in its turn, the rationality of the aims. Axiological controversies are on a par with factual or methodological ones, consequently the same mechanisms are involved in settling any scientific disputes.

Laudan distinguishes three kinds of utopianism (demonstrable, semantic and epistemic). I will analyze them separately.

Demonstrable utopianism arises when we infer the impossibility to achieve the aim at issue from logical or physical laws. Laudan's instance is infallible knowledge. Physical laws are unrestricted generalizations but testability is radically limited to observational claims we have access to. Because of this, we can not be sure that our knowledge is infallible, at most we could say that up to now this piece of knowledge has not failed, but this is not enough for infallibility in its full sense. Infallibility could have been a goal for science during long periods of history but now there is a wide agreement about fallibilism, the opposite view. It claims that scientific knowledge is provisional, revisable. We can back it up not only by means of logical arguments, as Laudan does, but with information from neurophisiology or comparative biology. These sciences underline the crucial role played by the sensorial receptors and the nervous system of a species in shaping reality. Research in these fields casts serious doubts on the access to a rough reality independent of the knower, and stresses the changing character of the latter, subject to evolution processes that profoundly alter his appropiation of reality. Hence it is really a philosophical platitude — both in science and in philosophy of science — that infallibility is not a reasonable aim, at least in an absolute sense. The moral that can be drawn from Laudan's example is that we can infer grounds for or against a goal from the theories and methods we accept at some stage of scientific development.

Semantic utopianism arises when the aims are not unambiguously characterized: «If someone purports to suscribe to an aim, but can neither describe it in the abstract nor identify it in concrete examples, there is no objective way to ascertain when that aim has been realized and when it has not.» (*SV* 52) Laudan thinks simplicity and elegance are not legitimate scientific aims in this sense. According to him, most advocates of these goals have no clear ideas about what these aims consist of, they

offer neither a coherent abstract definition nor good examples that supposedly instantiate it.

Unfortunately, Laudan is not clear enough about his intended sense of «clear». His remarks about semantic utopianism raise different issues. Firstly, the necessity of giving an accurate content to aims. It does not seem appropiate to use a goal as an emotive word, on pain of turning axiological debates into a confrontation of disguised subjective preferences. I agree with Laudan that those who subscribe to simplicity, for example, as a reasonable aim have a serious problem if it does not refer to an objective property of theories and is only a way to emotionally reinforce the acceptance of a theory — they are defending a goal devoid of content. However, I think his picture could do justice to elegance but not to simplicity. Far from it, the problem with simplicity is that there is no agreement among its advocates because they have different interpretations about it.

When applied to scientific theories simplicity may demand a reduction in kinds of postulated entities, laws' parameters, basic principles, mathematical calculations, ... We are not bound to understand simplicity in the same way when working on different scientific subdomains, so perhaps there is no such a general property as simplicity, a property that all scientific theories possess in more or less degree. Besides, why do scientists prefer simpler theories? It seems that to equate simplicity to convenience is not enough. If simplicity is an epistemological value, in its full sense, it must be connected with more interesting epistemological properties as predictive accuracy, explanatory power, ... For Popper simplicity is related to falsability; Quine prefers linking it with high probability; and E. Sober is skeptical about the possibility of stating a general argument to justify our preference for the simpler hypothesis when confronted with two having the same score at observational accuracy.[4]

Consequently, there is no general agreement among philosophers neither on how to define simplicity nor on how to justify it. In any case, a suitable account of scientific reasoning has to include simplicity insofar as the scientific judgment is under its influence. And, in relation to what we are mainly concerned with, the really important point is not whether everybody is talking about the same, but whether whatever each one of them is talking about may be a legitimate goal for science. Then, we have to isolate the reciprocal irreducible definitions ($simplicity_1$, $simplicity_2$, ... $simplicity_n$) and treat them as different goals, instead of rejecting simplicity straightforwardly as Laudan does. Then, they have to be assesed making use of the coherence principle and the realizability principle. Therefore, the mere coexistence of different interpretations of 'simplicity' is not a reason enough to exclude simplicity from the realm of legitimate aims.

These comments reveal Laudan's careless use of words like «goal», «aim» and «value». He treats them all as synonymous but it must be emphasized that, roughly

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4 K. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1959), chs. VII y *VIII; W.V. Quine, «Simple Theories of a Complex World», in his *The Ways of Paradox and Other Essays* (New York: Random House, 1966), 242-46; E. Sober, *Reconstructing the Past: Parsimony, Evolution and Inference*, (Cambridge, MA: MIT Press, 1988). For a naturalistic approach that links simplicity with more basic epistemic values see B. Ellis, *Truth and Objectivity* (Oxford: Basil Blackwell, 1990), ch. 8.

speaking, a value is a worthy property and a goal — or an aim — is what we pursue by our actions. We regard scientific theories as good or bad insofar as they possess worthy properties. But not every worthy property should be properly considered as a goal. It sounds extremely odd to say that scientists look for simplicity, or compatibility with the body of accepted knowledge, although they are all worthy epistemological properties, i.e., epistemological values. They are rather means for other goals such as explanatory power, predictive accuracy, and, why not, truth. On this view, their legitimacy would be assessed not only by PC and PR, but through their historical success as reliable indicators of more interesting epistemic values — the real goals of scientific theorization mentioned earlier — as well. Nonetheless, Laudan does not distinguish between the epistemic values pursued by themselves — the actual goals of science — from the epistemic values which are means for ulterior ones. All this does not rule out the possibility that the very epistemic values may be turned into means for non-epistemic values (see above p. 1).

Lastly, epistemic utopianism, which is much more fully characterized by Laudan than the other species of utopianism. This version arises because there is no criterion to determine when the value is satisfied, despite having a clear definition and no demonstration that it is utopian. Truth is the only example Laudan offers at this point although in *Science and Values* he devotes an entire chapter to discussing the issue. The charge of epistemic utopianism is roughly stated in the next quotation:

> Suppose, (...), someone claims to have the goal of building up a body of true theories. Moreover, let us suppose that he offers a coherent and straightforward characterization of what he means by a theory «being true» — perhaps in the classic tarskian semantics of correspondence. Under such circumstances his goal is not open to the charge of semantic confusion. But suppose, as we further explore this person's goal structure, it emerges that, although we can define what it means for a theory to be true, he has no idea whatever how to determine whether any theory actually has the property of being true. Under such circumstances, such a value could evidently not be operationalized. ... In the absence of a criterion for detecting when a goal has been realized, or is coming closer to realization, the goal cannot be rationally propounded even if the goal itself is both clearly defined and otherwise highly desirable. (*SV* 53)

In fact, the chapter on truth goes beyond the delegitimation of an aim to turn into a refusal of realism. It must be conceded that if we had not have the remotest idea as to how to approach truth, we would have a conclusive reason to abandon it as a legitimate aim. But achievability may be relative and, radical inaccesibility excluded, the axiological status of truth depends on additional factors. In the same way as equality or freedom are legitimate aims in political theory or in morals, even though in practice it seems impossible to realize these values completely, truth may be a legitimate goal for science, although we know we will never develop a true — in an absolute sense — account of the world.

Therefore, I shall try to show — contrary to Laudan — that truth is a goal for science and that, being a genuine aim as it is, its rationality depends not only on how far it may be achieved, but on the explanatory role it plays in a fair account of science as well. Following his strategy, I shall deal with this issue separately.

3. Truth

Laudan distinguishes three varieties of realism. Semantic realism — «to claim that all theories are either true or false and that some theories — we know not which — are true» is presupposed by epistemic realism — to claim that one can know if theories are true or false by means of certain kinds of empirical support. The third modality is intentional realism: «the view that theories are generally intended by their proponents to assert the existence of entities corresponding to the terms in those theories.» (*SV* 105) Laudan is not interested in denying that theoretical claims have a determinate truth value. His is not a complaint against bivalence. And he is not interested in casting doubt on the *intentions* of scientists either: they usually propound theories as true claims about the world. Nonetheless, this realistic attitude is not Laudan's genuine target (the more interesting question is, of course, if those theories are really true). Laudan's concern is epistemological realism. In very brief compass, theories may be true or false, but we have good reasons to despair of ascertaining it. He does not discard the possibility that truth be a worthy property which scientific theories do possess, the problem is that we are unable to detect it. Besides, realism has a remarkable normative component; in fact, it is a doctrine about «what the aims or values of science ought to be.» (*SV* 106) According to realism, the main aim of science is «to find ever true theories about the natural world.» (*id.*) Laudan attacks the notion of truth because of its undetectability and, consequently, he eschewes it as a legitimate goal.

Before discussing Laudan's objections, it may be worth noting some well — known remarks. Although research in the history of science shows us a non linear process, it is undeniable that recent theories, at least in mature sciences have a higher degree of empirical adequacy than their predecessors. And we must notice that improving predictive eficacy is closely related to improving instrumental success and technology. It is not difficult to find theories in present day science which encompass an impressive amount of empirical phenomena, much more than ancient generations of scientists would have ever imagined. Antirealists like Kuhn, van Fraasen and Laudan have no doubt about the high rate of empirical adequacy in science but they all warn us about seriously considering the ontological commitments of theories, especially the theoretical ones. And, as reference and truth are linked — given that to devise a true theory with referentially empty central terms would be a rather complicated task —, suspicion over reference (theoretical entities) leads to suspicion over truth (theoretical claims).

Instead of stopping at the empirical level and remaining agnostic about the upper floors, realist-minded philosophers think that some theories are true, from which it follows that their theoretical claims are also true and that the referents of their theoretical terms do exist. The argument many realists (Boyd, Putnam, McMullin, Leplin, Newton-Smith, ...) make use to fill the gap between the empirical and the theoretical levels is based on the explanatory role of truth. For most of them, there is no better ground to affirm the existence of theoretical entities and, consequently, the truth — at least the approximate truth — of theories than their empirical success. Insofar as the success of later theories increases, we have a compelling reason to affirm their truth and the existence of the theoretical entities posited by them. Otherwise it would certainly be striking that the world behave as if these entities existed, whithout really being there. This argument is a version of a model of reasoning called «inference to the best explanation» (IBE) that recalls Peircean abductive inference and has the following form:

O (an account of a fact),

E_1 is the best explanation of O (among the set of available and rival explanations E_1, E_2, ... E_n),

Therefore, E_1 is highly probable.

If we apply the model to the case we are concerned with, O will be an account of the success of a theory and E_1 will state that if a theory is successful, then it is true. A stronger — and more general — version of E_1 could be that the growing success of theories is due to their truth.[5]

Laudan claims, as do most anti-realists, that truth does not possess the explanatory power in which realists believe. If I have understood him, his rejection of truth is derived from two different contentions. The first has to do with devising truth as a property gradually instantiated; the second arises from the historical record and disputes the alleged connection between success and truth.

(a) **Truth and Closeness to truth**.

Even though Laudan does not accept a link from success to truth, as we will see later, he acknowledges that the converse entailment «if a theory is true, then it will be successful» is self-evident. (*SV* 117) The point is that there is no current scientific theory that could properly be considered fully true because realists are forced to weaken the notion of truth in response to Putnam's pessimistic meta-induction.[6] Since a great deal of past scientific theories have been falsified, we can't be sure that theories accepted now will survive every future test. It is quite likely that they will be eventually discarded. According to this, we would never be entitled to ascribe truth to a particular theory because those theories accepted now will be replaced by better ones in the future, just as they in turn replaced earlier theories. Indeed, in all probability every theory we have now is false.

Against this skeptical argument a minor change may be performed upon IBE so as to infer the approximate truth of successful theories. At most, all we have is more o less closeness to truth but not truth itself. Certainly, to adscribe complete truth to

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5 A classical paper on IBE is G. Harman, «The Inference to the Best Explanation», *Philosophical Review* 74 (1965): 88-95. A much more recent essay that underwrites the importance of this kind of inference in human knowledge is P. Lipton, *Inference to the Best Explanation* (London: Routledge, 1991). IBE connecting success with truth is fully developed in J. Leplin (ed.), *Scientific Realism* (Berkeley, CA: Univ. of Calif. Press, 1984).

6 H. Putnam, *Meaning and the Moral Sciences* (London: Routledge & Kegan Paul, 1978) p. 25. In «Structural Realism: The Best of Both Worlds?», *Dialectica* 43 (1989): 99-124, J. Worrall maintains that pessimistic induction was clearly stated by Poincaré.

a scientific theory would prevent from revising it and that would not be the game we are playing — science — but a very different one. In practice, «true» is not an absolute parameter since there is nothing unsound in talking about more or less true, and scientific realists often prefer using expressions as «partial truth», «proximity to truth», «verisimilitude», «truth-content», and so on in order to avoid the commitment with ascriptions of truth in an absolute sense. There are theories closer to truth than other ones, and in mature sciences we have good grounds to consider later theories closer to truth than former ones. Therefore, truth is gradually instantiated. Even though we have no instances of a true theory in an absolute sense, the growing success of later theories enables us to consider them closer to truth than preceding ones. This is what scientific progress mainly consists in, from a realistic stance.

However, the notion realists employ to forego pessimistic meta-induction (approximate or partial truth) is unacceptable to Laudan. First, it has to be showed that a semantically adequate characterization of it is available; secondly, realists have not argued convincingly that approximately true theories are successful predictors; thirdly, an epistemical criterion for adscriptions of approximate truth is needed (*SV* 120).

The first requirement is difficult to fulfill because Laudan does not give us any clue about what a «semantically adequate characterization» would consist in. I suppose Laudan is not demanding a mathematical account of approximate truth. Do we need a technical definition like the Tarskian one? Or, is it enough with a notion that allow us to make comparative judgments between rival theories?

Measurement of closeness to truth is an awkward task. Some realists have tried to define closeness to truth in terms of truth-content. Nevertheless, this approach has to face great difficulties — notice, for instance, that scientific theories have infinite observational consequences — and there is a generalized skepticism among philosophers of science about the possibility of working out the relative truth content of two theories. Popper himself acknowledges the limits of analysis about verisimilitude.[7] Yet, he thinks that the lack of aplicability of verisimilitude is not a sufficient reason to discard the notion of truth. He reminds us that deducibility is not as clear a notion as some would like. Although a general procedure to decide in concrete examples if a formula is deducible from the axioms of a logical calculus — and very often there is no time to work out the infinite number of valid deductions — can not be offered, this fact does not lay aside notions as deducibility and formal validity. This is just what happens with closeness to truth.[8] To deffend the

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7 «Some people have assumed that my aim was something like exactness or precision; or even applicability: that I hoped to find a numerical function which can be applied to theories and which tells us, in numerical terms, what their verisimilitude is (or at least their truth content; or perhaps their degree of corroboration). In fact, nothing can be further removed from my aims. I do not think that degrees of verisimilitude, or a measure of truth content, or falsity content (or, say, degree of corroboration, or even logical probability) can ever be numerically determined, except in certain limited cases (such as 0 and 1).» K.R. Popper, *Objective Knowledge: An Evolutionary Approach* (Oxford: Oxford U.P. 1972), p. 58.

8 K.R. Popper, op. cit., ch. 9.

explanatory role of truth — and, thus, its legitimacy as a goal for science — it is not necessary to have a very exact notion of it. «Closeness» is a misguiding word here because it invites to measure the distance to the last stage; but talking about closeness — or approximation — is only a way of acknowledging that even our current best theories might eventually be rejected.

Here we may take into account recent developments which try to reconcile the relentless historical replacement of theories with the realist intuition that progress — in mature sciences at least — consists in a growing precision as to the identification of what there is. Causal theories of reference have to face great difficulties and it is dubious that scientific realism could ground upon them. Perhaps a «metaphorical» theory of reference like the ones developed by R. Boyd and E. McMullin would be more promising. Both of them appeal to the notion of metaphor and set up a more loose connection between theory and world than a pure causal theory whithout abandoning the realist viewpoint.[9] I shall not pursue the point here, but if it can be shown that recent theories — through a refinement of an initial metaphor, for instance — are better at identifying reference than earlier ones this would give support to our intuitive jugdments about approximate truth. In fact, scientists talk about true/false theories/hypothesis, at least for now, and it does not seem to create a perennial confusion among them. Judgments attributing truth or falsity are revisable but that is a different matter. In the selection of rival theories we may make errors but this does not undermine the global task of separating and excluding falsities.

Laudan himself develops an alternative to realist conception of scientific progress in *Progress and Its Problems*.[10] There he claims that the goal of science consists in solving problems — both empirical and conceptual problems — and avoiding anomalies. We have to choose theories — or research traditions — with a high rate of problem-solving efectiveness. I must confess that, from the point of view of precision, I see no advantage in replacing the rate of verisimilitude for the rate of problem-solving efectiveness. To start with, we have no clear criteria about what to count as a problem. Let us grant that we arrive at a precise definition of what counts as a problem and that we can neatly distinguish between two different problems and two different formulations of the same problem. Yet efectiveness in solving problems is not merely a matter of counting solved problems. The resolution of a certain problem may be crucial for ulterior developments in the discipline, or perhaps, for devising successful technology to face practical pressing needs. Since not all the

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9 See R. Boyd, «Metaphor and Theory Change» and E. McMullin, «Metaphor in Science», in *Metaphor and Thought*, A. Ortony (ed.), (Cambridge: Cambridge U.P., 1979). A more recent attempt — a hybrid causal and description theory of reference, as is defined by its author — can be found in D. Cummiskey, «Reference Failure and Scientific Realism», *Brit. J. Phil. Sci.* 43 (1992), 21-40. A well known for causal theories of reference is *Naming, Necessity and Natural Kinds*, P. Schwartz (ed.) (Cornell U.P., 1977). For an account of approximate truth distinguished from verisimilitude, probability and mere vagueness, see T. Weston, «Approximate Truth and Scientific Realism», *Phil. of Science* 59 (1992): 53-74.

10 L. Laudan, *Progress and Its Problems* (Berkeley, CA: Univ. of Calif. Press, 1977).

problems have the same relevance, and their importance — and not only the number — is decisive for choosing one theory, we have to previously assess their relative weight. But, how can this be determined? If, in the end, we have to rely on scientific intuitions to assess the problem-solving efectiveness of two rival theories, then I don't think that Laudan is in a better position than the advocates of approximate truth. Problem-solving efectiveness is as fuzzy a notion as approximation to truth. Since we have no precise definition of both properties, Laudan's rejection of partial truth also forces him to abandon his own approach to scientific progress.

Laudan's second objection against approximate truth criticizes its purported link with predictive success: «No one of the proponents of realism has yet articulated a coherent account of approximate truth which *entails* that approximately true theories will, across the range where we can test them, be successful predictors».[11] He briefly discusses Popperian definition of approximate truth in terms of truth and falsity content and argues that it is possible we may not be able to ascertain that a theory T_1 is more approximately true than T_2 on the strenght of its predictive success (because its truth content is not the same as the truth content available to us: the former may be huge while the latter poor.) If the successful predictions of T_1 are not available to us, we shall not consider it as a better approximation to truth than T_2, even though it may be so indeed. It would be equally possible that T_2 be more successful than T_1, although it is further from truth because its falsity content unknown to us is greater than the falsity content of T_1. Laudan's second objection must be understood as a concern with *detection* of approximate truth, and this conflates it with the third one — the need of an epistemical criterion for adscriptions of approximate truth. He points out that success is not a reliable indicator of approximate truth insofar as the realists have not demonstrated a connection between approximate truth and success.

A few remarks are in order here. There are several ways to define approximate truth. The Popperian approach — an algorithmic one — is just an example and perhaps it is not on the right track. On the other hand, having granted, as Laudan does, that the connection between truth and success is self-evident, I see no problem in affirming a connection between approximate truth and success. Despite the fact that the Popperian attempt to define closeness to truth in terms of truth and falsity content is open to the logical objections raised by Laudan, realists are not bound to this definition. Approximation to truth could be understood as a consequence of a more exact determination of the entities with which we causally interact by means of sophisticated devices.[12] The plausibility of IBE as a general pattern of reasoning is untouched after replacing truth for approximate truth. The question now is: on which grounds may we infer partial truth from success? This takes us from logical to historical considerations.

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11 SV, 120 (words in italics have been added). Laudan suggests that a connection between approximate truth and success may exist: «I must stress again that I am not denying that there may be a connection between approximate truth and predictive success. I am observing only that the realists, until they show us what that connection is, should be more reticent than they are about claiming that realism can explain the success of science.» (SV, 119, footnote 21).

12 See above footnote 9.

(b) **History of Science**.

Realism affirms the existence of theoretical entities, while an antirealist like Laudan prefers to stay at the observational level. Obviously, if we took IBE as a conclusive argument from a logical point of view we would be committing a formal fallacy. Realism does not pursue such a kind of basis: IBE only claims that the antecedent is highly probable, not certain. However, Laudan finds evidence against the alleged connection between properties as «empirically adequate» and «true» in the history of science. In fact, some theories were once successful, well confirmed and widely accepted but now they are considered plainly false: the ploghiston theory, the caloric theory of heat, the humoral theory of medicine, among other examples. (*SV* 121) Accordingly, if success is not an indication of truth, we are not entitled to infer the truth of theoretical postulates from their empirical success and the very existence of the entities is seriously questioned.

There have been several attempts to meet the challenge,[13] and I think the most promisory defense of realist convictions consists in admitting that success by itself is not a sufficient condition for truth, while maintaining that truth is a basic notion in order to understand the workings — and the success — of science.

First of all, «realism is not a blanket approval for all the entities postulated of the past.»[14] A theory could be firmly believed by the scientific community and — according to success standards of the age — regarded as a successful theory, but this is not enough to infer its truth. Success has to be assessed during a significant period of time and it has to be accompanied by other important epistemic values that Laudan completely neglects.

Take, for instance, «ad hocity». Ad hoc explanations are not legitimate ones. They can be temporarily accepted, if there are no better alternatives. But it is commonly held that ad hocity is an undesirable feature, even though an ad hoc theory encompasses a large amount of empirical phenomena. A good and well-known example is Ptolemy's heliocentric system. It could have been successful, from a predictive point of view, during a large period of time but it is not true. Its truth can not be inferred from its predictive reliability; but its falsity can be inferred from its ad hocity. To save the phenomena is not enough and it is even a symptom of something going wrong.

Ad hoc theories go after observation, while scientific method somehow anticipates itself to phenomena. Of course, a successful prediction involves that we have anticipated what is going to happen, but I am thinking of a special kind of predictions: what has been called «novel predictions». The ability to make unexpected predictions is an epistemic value (fertility) that Laudan does not discuss.

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13 A. Rosenberg and C.L. Hardin, «In Defense of Convergent Realism», *Philosophy of Science* 49 (1982): 604-15; D. Cummiskey, «Reference Failure and Scientific Realism: a Response to the Meta-induction», *British Journal for the Philosophy of Science* 43 (1992): 21-40; J.W. McAllister, «Scientific Realism and the Criteria for Theory-Choice», *Erkenntnis* 38 (1993): 203-22; E. McMullin, «A Case for Scientific Realism», in J. Leplin (ed.), op. cit., 8-40; J. Leplin, «Truth and Scientific Progress», in id., 193-217.

14 E. McMullin, op. cit., p. 17.

Ad hoc theories are not fertile theories, and in that sense they do not anticipate themselves to phenomena. Fertility is closely related to explanatory power, another value that is not in Laudan's agenda. Sometimes a theory works in a new field though, in principle, it was not thought out to handle it. This sort of success is not simple predictive success and, whatever the name we choose, it is more difficult to explain by antirealists. In such a case it seems that we are entitled to infer that the underlying mechanisms of the different kinds of phenomena are the same. The theory is anchored to solid rock by identification of theoretical entities, mechanisms and processes that really exist and it unifies previously separated realms increasing explanatory power. Thus, atomistic theory showed its explanatory power by dealing with heat, even though it was not primarily designed to apply there.

Therefore, the fact that scientists distinguish between ad hoc explanations and more natural ones gives reason to believe that there are other factors in addition to predictive success which function as reliable indicators of truth. These factors are worthy properties — values — as fertility and explanatory power. The lack of any allusion to them reveals an important neglect in Laudan's axiological discussion. Observational success by itself may not be sufficient for truth, but there are other values that give some grounds to believe that truth is not so a blurry notion as Laudan suggests. Approximate truth of theories, and the existence of referents partially similar to the theoretical posits, can be inferred when predictive success plus fertility plus explanatory power go together.

How to measure values as fertility or explanatory power? Certainly, they are more difficult to assess than predictive accuracy or instrumental success but we can recall historical examples to show that they can be discerned. It is commonly assumed that Newton's explanation of free-fall is better than Galileo's one, even though both are false. But the superiority of the former is not simply a question of predictive success, rather it suceeds in offering a more comprehensive and accurate picture of physical phenomena. This is not surprising. One of the most peculiar features of scientific methodology is self-correctness. The criteria of what counts as a good explanation have changed for centuries and the scientific community modifies them in order to make them more powerful and effective in representation and manipulation of phenomena. In D. Shapere's words, it is not only a matter of coming to know about the world, but of learning how to learn, to think and to talk about nature as well.[15] However, to deffend the legitimacy of truth as a scientific aim it is not necessary to be commited with a perfect theory as the result of the iterated application of scientific methodology. I think we can hardly make sense of that notion indeed.

In his first book Laudan provocatively compared pursuit of truth with pursuit of immortality, of the philosopher's stone, ... as if it were a completely misguided enterprise, if not a chimerical dream.[16] But to affirm that predictive success — which is relatively easy to assess empirically — is not enough for truth does not involve that truth is a misterious and undetectable property. On the other hand, our judgments concerning theoretical truth are historical. They are determined by the

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15 See the chapters 10 and 19 of his *Reason and the Search for Knowledge*, (Dordrecht: D. Reidel, 1984).

16 *Progress and Its Problems*, ch. 4, footnote 2.

amount of knowledge we have at certain time and we may fail in our ascriptions of truth. Again, this does not mean to equate «T is true» with «T is widely accepted» or «T is justifiyingly believed» because truth is not a purely epistemic concept. It has to do not only with the way we represent the world but with the way the world is, and a historical approachment to mature sciences shows certain referential stability and an increasing detail of the internal mechanisms postulated.

4. Conclusions

It is now time to look into the merits of Laudan's attempt to set up objective criteria in order to settle axiological controversies in science.

PR is more disputable than PC. Notice that PC is closely related to a version of PR: the demonstrable utopianism. To refuse a goal for being demonstrably utopian means to call for some kind of coherence. At the bottom we have to deal with coherence because in both cases the rejection of an aim is made in order to avoid incoherence between what we claim and what we do, on one side, and what we try to achieve, on the other side. Why does not Laudan subsume demonstrable utopianism under PC as a particular version of incoherence? There is a subtlety. Demonstrable utopianism allows us to infer conclusively the impossibility of the aim in question from the accepted theories and to reject it outright, whereas the revision of the aim according to PC is the result of repeated failures in achieving it. We could say the former points at a theoretical incoherence; the latter at a practical one.

In relation to semantic utopianism, I have already pointed out why Laudan's argument is not sufficiently powerful. Now, I would like to make a more general remark. Laudan discards simplicity and elegance implicitly assuming another value which he does not argue for: precision. Precision is the value that supports the charge of semantic utopianism but then we have to adress some questions: why is precision a more fundamental value than simplicity or elegance? what sort of justification could we offer for precision? could it not be that precision was also a utopian goal according to some of the three modalities suggested by Laudan? The point is that PC and PR are themselves grounded on values. Stating the problem in a more general form: are we not forced to show that the values involved in the analysis of science are justified from the very science (in accordance with a naturalized conception of knowledge)? I am not sure this is a severe requirement for Laudan's reticulated model. Perhaps Laudan could reply that answering this general question goes beyond an analysis of scientific rationality, but I think it would be desirable to fill the gap. Meanwhile the legitimacy of the principles that legitimate scientific aims is in question.

Regarding the last sort of utopianism — epistemic utopianism —, truth is not so utopian as Laudan claims. Problem-solving efectiveness is not clearer than partial truth. On the other side, there are basic distinctions in the appraisal of theories that could not be grounded if we do not assume an ability to identify actual constituents of the world on the part of some scientific theories. The way to truth is neither straight nor conclusive but taking into account epistemic values as fertility and explanatory power is neccesary to sustain the realist cause.

For Laudan, axiological choices are on the same footing as the theoretical and methodological ones: all of them may be objectively grounded. The generality of the principles and their naturalistic flavour are the most remarkable merits of Laudan's

account but the results are rather meagre. PC may be, in the end, a mere *a posteriori* justification of changes in axiological direction carried out by the scientific community. The rejection of a demonstrable utopian goal, granting naturalistic assumptions, is completely sound but it has a very limited scope. I am afraid science could not *demonstrate* much about goals. From the rejection of semantic utopianism we can draw a need for a previous clarification rather than sustantive criticisms and, finally, Laudan's charge of epistemic utopianism is very controversial, as I have tried to show.

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When Is If?

WHEN IS IF?

M. G. Yoes, Jr.

Not even the most compelling laws of logic escape philosophical challenge, as attacks on laws of the conditional illustrate. It is well-known that counterfactual conditionals present special difficulties. But recently, even the venerable Modus Ponens and Modus Tollens of indicative conditionals have been called into question.[1]

Consider Adam's example:

(1) If it rained, it did not rain hard.

(2) It did rain hard.

Therefore,

(3) It did not rain.

Modus Tollens, it might seem, has absurdly led us to call this intuitively invalid argument valid. A counterexample to Modus Tollens? What has gone wrong?

Indeed, if we accept

(4) If it did rain hard, it rained

as logically true, then Modus Ponens leads from (2) and (4) to

(5) It rained

which together with (1) and Modus Ponens again implies

(6) It did not rain hard.

Should we say, then, that if we accept Modus Ponens and (4) as a logical truth, we are stuck with saying that the premise set {(1), (2)} of the argument is itself inconsistent? That the argument 'If it rained, it did not rain hard; therefore, it did not rain hard' is valid'? Is Modus Ponens to be indicted as well?

No. This is a case of unusual symptoms but mistaken diagnosis. For the problem is not in the logic but in the representation of the logical form of (1) as a conditional, a problelm of surface grammar being a false clue. Here is another

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[1] Vann McGee, «A Counterexample to Modus Ponens», *The Journal of Philosophy*, Vol. LXXXII (September, 1985), 462-471; and Ernest W. Adams, «Modus Tollens Revisited», *Analysis* 48.3 (June, l988), 121-127.

example of Russell's lesson that grammar can hide logical form. For (1) can be paraphrased as something like

(7) It may have rained, but it did not rain hard,

a mere conjunction. (1), then, on this reasonable representation, is a conjunction masquerading as conditional. The idea in (1) is **not** somehow that its not raining hard is conditioned on its having rained, but, as it were, to allow the possibility that it rained while denying that it did rain hard.

The matter can be put more cautiously. Perhaps (1) **could** be used to state a genuine conditional, but it would be a strange conditional indeed. If (1) **is** represented as a conditional, and we keep that assumption firmly in mind, then the surprise of these examples shifts from the validity of the argument to the peculiarity of the premise. If it rained, **then** it did not rain hard?

The analysis roughly is this. (1), and presumably some other classes of 'if' statements, are not conditionals at all but conjunctions in disguise. The 'if' in these statements functions as some sort of modal but with small scope: if it rained,...; that is, it may have rained, but.... . (Not that this analysis works for all 'if's, for 'if's are not univocal.)

On this analysis, of course, (3) does indeed follow from (1) and (2), though not by Modus Tollens since (1) is not a conditional. The premise set {(1), (2)} is likewise inconsistent on this analysis and the argument '(1), therefore ¬(2)' is valid. The argument 'If it rained, it did not rain hard; it rained; therefore, it did not rain hard' is valid on this analysis, though not by Modus Ponens since again (1) is not a conditional.

An analysis that makes 'If it rained, it did not rain hard; thus it did not rain hard' valid may seem counterintuitive; conditionals do not imply their consequents and the trained logical intuition sees a conditional behind every 'if'. Still, recognizing that one quite normal reading of 'If it rained, it did not rain hard' is 'It may have rained, but it did not rain hard' may blunt the intuition.

This is not unlike Austin's example[2]:

(8) There are biscuits on the sideboard if you want one.

If (8) is true and there are no biscuits on the sideboard, is it a fault in ancient and modern logic that no one would accept an inference to your not wanting a biscuit? No. Again we are better off saying that (8) is a hidden conjunction something like:

(9) There are biscuits on the sideboard and perhaps you want one.

Like any conjunction, of course, the whole will be false if either conjunct is false; so,

(10) There are no biscuits on the sideboard

implies that (9), and thus (8), are both false. But (10) does not imply that you do not want a biscuit. Likewise (2) does imply that (7) and thus (1) are both false. But (2) does not imply that it is false that it may have rained.

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2 J. L. Austin, «Ifs and Cans», *Philosophical Papers* (Oxford: Oxford University Press, 1961)

Construing (8) as a conjunction, despite its conditional disguise, allows us to infer ¬(8) from (10) since (10) gives ¬(9). But what if the right conjunct of (9) is false:

(11) ¬(perhaps you want a biscuit).

If (11) means

(12) You certainly do not want a biscuit

does ¬(8) follow? Isn't (12) consistent with (8)? Might it not be true that there are biscuits on the sideboard if you want one, while you certainly do not want one? Similarly, might it not be true that if it rained it did not rain hard, while it could not have rained? It is not merely a question of whether there is a likely conversational implicature that perhaps you do want a biscuit brought off by anyone who says there are biscuits on the sideboard if you want one. It is a question of truth conditions.

Yet there is no doubt that (8) is in some way incomplete. The implicit modality which the analysis in (9) brings out is necessarily *read in* so as to capture the weak 'if'. There is ellipsis here. And perhaps 'perhaps' is not quite the correct modality; the speaker may have to disambiguate for us. One reading which casts matters in a different light is

(13) There are biscuits on the sideboard and you may have one.

This statement **is** falsified, of course, by the assumption that you may not have a cookie. So if (8) is captured by (13) then (8) is likewise falsified, and the thesis that (8) is some sort of conjunction is confirmed. Moreover, it seems clear that

(14) There are biscuits on the sideboard if you want one; but you may not have one.

is more than conversationally at odds with itself, but in some way actually inconsistent.

The general point does not rest on the particular analysis of (8) as (9), but rather on the hypothesis that there exists a class of statements superficially of the form 'A if B' which are best understood as conjunctions of the form 'A and (Modal(B))'. What the modal actually is may strongly depend on context. Indeed, without a specific context (13) seems as good a candidate as (9) for a paraphrase of (8).

'If' may function as a mere modality, introducing doubt, uncertainty or whatever, in which case it is not functioning as a conditional, as a real iffy 'if'. This mere modality of small scope is marked by the fact that it is hardly comfortable with a corresponding 'then': If it rained, **then** it did not rain hard? If you want one, **then** there are biscuits on the sideboard?

When, then, is *if*? When it *functions* as an *if*. When is that? Standard formal logic answers: when it satisfies Modus Ponens, Modus Tollens, etc. Some 'if's do not satisfy these formal properties, and therefore, according to this standard, are not conditionals at all. Thus do the formal properties define the conditional, as they define the other logical notions. The force of this definition is that it unmasks logical constructions in disguise.

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Truth, Knowledge and Reality

TRUTH, KNOWLEDGE AND REALITY

Cristina Lafont

To avoid the misleading impression that such a general title might give, I would like to point out that in the following essay I will only refer to the different ways in which these three concepts are connected in the current philosophical discussion about truth. Obviously not all explanations of the concept of truth refer to the other two concepts, but given the internal connection that exists, in my opinion, between these concepts, one can show that any other attempt to account for some of these concepts, either because it does not appeal to the others or because it reduces them into each other, inevitably contain aporias. The two extreme versions of such attempts are, on the one hand, the extreme realist position which considers truth to be a **radically non-epistemic** concept (that is, in which the concepts of «truth» and «reality» are connected without appealing to «knowledge»); and, on the other hand, the extreme epistemic position which interprets truth as a merely **epistemic** concept (that is, in which the concepts of «truth» and «knowledge» are connected without appealing to the concept of «reality»).

Both positions seem to be supported by intuitions that are clearly related to the concept of truth: it would be difficult to argue, against the realist perspective, that truth depends on something other that what is the case; conversely, faced with the epistemic perspective, it seems equally difficult to question that «true» is a predicate which has an internal relation to our knowledge. Nevertheless, the fact that these positions are in contrast seems to exclude the possibility of accounting for both intuitions together.

Usually two sorts of arguments are advanced from the epistemic standpoint against realists. Either, it is said, the realists have to limit themselves to the assertion that the meaning of the concept of truth is completely captured by the equivalence formulated by Tarski (the «Convention T», ie the equivalence of the type «the sentence 'the snow is white' is true, if, and only if, the snow is white»), in which case all **philosophical** relevance is taken away from such a concept by recognizing only its «disquotational» use and consequently by favoring a deflationary position.[1] Or, if realists want to situate the concept of truth in the context of our beliefs, then

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[1] See St. Leeds: «Theories of Reference and Truth», in: *Erkenntnis* 13 (1978), pp.111-130; P. Horwich: «Three Forms of Realism», in: *Synthese* 51 (1982), pp.181-201; M. Williams: «Do We (Epistemologists) Need a Theory of Truth?», in: *Philosophical Topics* 14 (1986), pp.223-242.

it is claimed that they must accept either a relativistic conception of Tarski's theory that considers the question of truth merely as **internal** to a given language or theory (thus reducing the meaning of the predicate «true» to «true-in-L», and in this way giving up precisely the realist intuition)[2], or they must accept a metaphysical interpretation of Tarski's theory by postulating a correspondence between language and reality[3] that, apart from being inexplicable, can only be asserted from the «God's eye» point of view — to use Putnam's expression. The realist, then, would be faced with a choice between renouncing to give an explanation of the connection between «truth» and «knowledge», consonant with her radically non-epistemic position, or, if she intends to explain such a connection by recurring to realism, appealing to a reality in itself which guarantees such a connection, but which confronts the difficulty that Wittgenstein already pointed out — and that is manifested precisely by Tarski's equivalence — namely: «the limit of language is shown by the impossibility of describing the fact that corresponds to the proposition (...) without repeating the same proposition.»[4] Precisely because it is not possible to have access to facts independently of the language in which we describe them, we cannot have, from the epistemic standpoint, a concept of «reality» (or of «that which is the case») other than the one which is equivalent to our «knowledge»: the connection between «truth» and «knowledge» explains the concept of «reality» and not conversely.

Now, the defenders of such an epistemic position seem to have two options that are equally unsatisfactory: either, given their radically epistemic perspective, they renounce to the concept of «reality» reducing it to that of «knowledge», thus falling into the relativism that consists in declaring any candidate to «knowledge» (or justified belief) to be true, that is, accepting as many «realities» as there are sets of «knolwedges»[5], or, if they insist in explaining the concept of «reality» from the connection between «knowledge» and «truth», they must appeal to an emphatic

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2 See W.v.O. Quine: *Word and Object*, Cambridge 1960; also: *Ontological Relativity and Other Essays*, N.Y. 1969.

3 See Tarski: «The Establishment of Scientfic Semantics», in: *Logic, Semantics, Metamathematics*, NY 1956; also: M. Devitt: *Realism and Truth*, Oxford 1984; for a physicalist version, see H. Field's article «Tarski's Theory of Truth», in: *The Journal of Philosophy* 64/13 (1972), pp.347-375.

4 L. Wittgenstein, «Vermischte Bemerkungen», in: *Uber Gewissheit*, Werkausgabe, vol.8, Francfort 1984, p.463.

5 From a radically epistemic position as the one of Rorty it is always argued, against the defenders of a concept of truth as «rational acceptability under ideal conditions», that they are not sufficiently consequent with their own position because they do not eliminate «reality» or the presupposition of a «shared objective world» from their theories, since this presupposition, according to Rorty, cannot be more than a «residue
of the theory of correspondence», that is, «it would make sense only if what is true is determined in some way by such a world» (in: «Sind Aussage universelle Geltungsansprüche?», p.10-11, Manuscript version.)

concept of «knowledge» that is as suspiciously metaphysical as it is antifallibilist[6]. Such a concept of **one true knowledge** (or the Peircean idealization of an «ultimate opinion») — which, as such, cannot be conceived as fallible — remains as inaccessible to our beliefs as the «reality in itself» of the defenders of a correspondence theory of truth, as Davidson made clear in his critique of the epistemic conception of truth as «rational acceptability under ideal conditions»: «One suspects that, if the conditions under which someone is ideally justified in asserting something were spelled out, it would be apparent either that those conditions allow the possibility of error or that they are so ideal as to make no use of the intended connection with human abilities.»[7] It seems, then, that the defenders of an epistemic position are not better off than the realists when faced with the dilemma that Wellmer correctly characterized as the «antinomy of truth»[8]: either one tries to defend the absolute (or normative) sense of the concept of truth, thereby appealing to **metaphysical** theses, or one criticizes such an absolutism in virtue of its metaphysical character, but one thereby incurrs in an inconsistent **relativism**.

These types of difficulties are no doubt the sort of thing that have made some authors, like Davidson,[9] consider that the concept of truth is effectively captured by the equivalence formulated by Tarski — the «Convention T» — not in the sense that its use is merely «disquotational», but rather in the sense that Tarski's formula expresses a previous meaning of truth which every speaker understands intuitively — i.e., that a proposition is true if it expresses what is the case[10] — and whose clarity

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6 The antifallibilism that is implicit in the epistemic conception of truth as «rational acceptability under ideal conditions» is pointed out by Putnam himself when he states in «Realism and Reason» (in: *Meaning and the Moral Sciences*, London 1978, pp.123-140): «The supposition that even an 'ideal' theory might really be false appears to collapse into **unintelligibility**.» (p.126) A more detailed exposition of this conception of truth can be found in H. Putnam: *Reason, Truth and History*, Cambridge 1981, pp.54ff.; also: J. Habermas: «Wahrheitstheorien», in: *Vorstudien und Ergänzungen zur Theorie des kommunikativen Handelns*, Francfort 1984, pp.127-186; K.-O. Apel: «Fallibilismus, Konsenstheorie der Warheit und Letztbegründung», in: Forum für Philosophie (ed.): *Philosophie und Begründung*, Francfort 1986, pp.116-211.

7 D. Davidson: «The Structure and Content of Truth», in: *The Journal of Philosophy* 87/6 (1990), p.307. A more detailed exposition of such an argument can be found in C. Wright: *Truth and Objectivity*, Cambridge, MA. 1992, p.37ff, especially p.45.

8 A. Wellmer: «Wahrheit, Kontingenz, Moderne», in: *Endspiele: Die unversöhnliche Moderne*, Francfort 1993, p.158.

9 See fn.7.

10 In this respect, Davidson remarks in his «A Coherence Theory of Truth and Knowledge» (in: E. LePore (ed.): *Truth and Interpretation. Perspectives on the Philosophy of D. Davidson*, Oxford 1987): «What Convention T (...) reveals is that the truth of an utterance depends on just two things: what the

cannot be increased by any attempt to reduce that central concept to any other one. The concept of truth must be considered to be **primitive** (or undefinable). Taking this position into account, the dilemma seems to offer, again, two possibilities: either one holds on to the realistic meaning of such a concept, thereby paying the price of not being able to give a philosophical account of it (that is, holding on to its undefinability, and avoiding metaphysical assertions); or one can explore along epistemic lines the connection of this concept with our practices of justifying beliefs, thereby renouncing any account of its realistic meaning — and paying the price of having to appeal to a justified knowledge which, in order to preserve the absolute validity of truth, has to be conceived as infallible.

When the issue is put this way, and if one persists in the attempt of giving a philosophical explanation of the concept of truth, it seems most reasonable to seek a third way that would give an account of the different intuitions that stand out in each of these perspectives, while avoiding the bad alternative between triviality and antifallibilism. In what follows I will try to sketch an argumentative strategy through which it may be possible to articulate a way out of such dilemmas. One can describe the attempt in this way: the eminently **realist** meaning of our intuitive concept of truth is effectively captured by Tarski's equivalence precisely because it expresses the indissoluble connection between «truth» and «reality»: the statement p is true if, and only if, it is the case that p; but this merely semantic explanation of the meaning of the concept of truth turns out to be philosophically trivial. Now, this triviality, as such, probably has less to do with the questionableness of the expressed **content** — which can hardly be denied — than with the **perspective** adopted in order to give an account of such a content. Put otherwise, it may be that from the epistemic standpoint adopted by those who want to give a philosophically relevant explanation of the concept of truth, that is, one that connects this concept to our «knowledge», such explanation of the **realist** sense of the triviality far from being itself trivial would give the key to resolve the dilemmas that emerge when one tries to reduce the concept of truth to a merely **epistemic** concept. The required explanation would adopt an epistemic perspective that allows to explain the connection between «truth» and «knowledge», and from which it is also possible to account for the concept of «reality» without appealing to metaphysical suppositions — that is, without falling into the epistemic realism of a theory of correspondence that postulates a reality «in itself».

In my opinion, such a perspective can be found in the **formal pragmatics** espoused by Habermas in his theory of communicative rationality. In the reconstruction that such a theory gives of the normative presuppositions inherent in the processes of communication, one can also find an explanation of the concept of «reality» that is carried out in strictly **formal** terms and therefore permits the difficult combination which I mentioned above: to avoid completely the supposition of a world in itself as guarantor of the validity of our knowledge and yet to conserve the normative (counterfactual) sense that such a supposition implies and that permits an account both of the fallibilist intuition regarding the permanent revisability of our knowledge and of the absolute validity that we attribute to truth. I am referring to the formal-pragmatic concept of a **shared objective world** that Habermas introduces in the *Theory of Communicative Action* as a inevitable presupposition of

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words as spoken mean, and how the world is arranged.» (p.309)

communication (as well as of the discursive practice of questioning and revising our validity claims).

Nonetheless, Habermas does not bring into play such a supposition when he accounts for the concept of «truth»; on the contrary, his discursive interpretation of rational acceptability seems to require him to conceive of truth as a merely epistemic concept (that is, reducing it to the concept of «rational acceptability under ideal conditions»). In order to defend the possibility of giving an account of the concepts of «truth», «knowledge» and «reality», without reducing them into each other, I will try to show how it is possible, within the discursive framework of rational acceptability developed by Habermas, to account for the **realist** sense of the concept of truth — appealing thereby to the formal-pragmatic supposition of one objective world — and, further, how this account allows us to give up the supposition inherent in the epistemic conception of truth, namely, that of a **true knowledge** (or an «ultimate opinion»), which is as metaphysical as it is incompatible with fallibilism.

The **pragmatic** perspective from which Habermas tries to clarify the meaning of the concept of truth[11] is what allows him to show the insufficiencies of the attempt to explain such a concept without situating it in the context of the practices of revision of our knowledge. In fact, if one considers only the «disquotational» use of the predicate «true», then one inevitably reaches the conclusion that saying that «p is true» does not add anything to the mere assertion «p»; this observation leads to the conclusion, suggested by Ramsey's redundancy theory[12], that such a predicate is logically superfluous — and, therefore, that a theory of truth is also superfluous, as suggested by deflationists. If, on the contrary, one adopts a pragmatic perspective, that is, if one considers in what context we use such a predicate, the difference between both things becomes evident: to add «is true» (or «is false») to assertions ceases to be superfluous — as Habermas rightly shows — as soon as we situate ourselves in the context of putting into question such assertions since, in such a context, the truth claim, which is undoubtedly already implicit in the assertion, becomes explicit through remarks of the type «p is true/is false» precisely in order to indicate the controversial character or the need for justification of these assertions. Such remarks point out the need for an explicit thematization (in a «discourse») of the truth claim of the problematized assertion in order to analyze the degree of justification of the same. From this perspective one can see the other uses of the predicate «true» over and above the «disquotational» use: we can call these uses, following Rorty[13], the «endorsing use» and the «cautionary use» of such a predicate

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11 In what follows I will refer basically to Habermas's article «Wahrheitstheorien» [WT] (in: op. cit., pp.127-183).

12 F.P. Ramsey: «Facts and Propositions» (1927), in: *The Foundations of Mathematics*, London/N.Y. 1931.

13 Rorty distinguishes in his article «Pragmatism, Davidson and Truth» (in: E. LePore (ed.): *Truth and Interpretation*, Oxford 1986, pp.333-355) along with the disquotational use of the predicate 'true' two other uses of the same: the endorsing use — through which we assent or approve what is said by someone — and the cautionary use — through which we question the truth of what is said by someone. Returning to this distinction in his article «Universality and Truth» (1993), Rorty considers that the cautionary use —

— that is, the role played by such a predicate as a warning or reserve in regard to the possibility that our assertions may turn out to be unjustified or, even though they seem to be justified, may turn out not to be true. By analyzing these uses it becomes evident that such a predicate not only is not superfluous but, above all, its use is internally related to the epistemic processes of revision of our knowledge.

From this perspective it is understandable that the discursive theory of truth grounds itself in a formal-pragmatic analysis of the cognitive use of language, specifically of the constative speech acts, since, even though that about which we say that it is true or false are statements, these, taken by themselves, merely express possible states of affairs. For a statement to be true, though, the expressed state of affairs must be a fact. Habermas remarks in his article «Wahrheitstheorien» that «we call statements true or false in relation to the states of affairs that are expressed or reproduced in them. (...) To each statement we can assign a state of affairs, but a statement is true if and only if it reproduces a real state of affairs or a fact — and not if it presents a state of affairs as if it were a fact». (WT,p.128) For this reason, Habermas considers that only when a statement «is placed in relation with the external reality of that which can be observed» through an **assertion** does this statement actually remain tied to the validity claim «truth» — a claim that such a statement «in as much as it is a non-situated sentence, a mere grammatical construction, neither requires nor can satisfy.»[14] To this extent, the meaning of the predicate «true» is correctly interpreted only if one understands it as a validity claim that we attach to statements **when we assert them**. Now, that someone asserts a statement means, at the same time, that they **believe** or **know** that such a statement is true; in this sense, the statements which may be true or false, express beliefs that, if they are true, can be considered «knowledge». For this reason, the validity claim «truth» that we link to our statements becomes explicit (through remarks of the type «p is true/is false») in the context of putting into question and revising our knowledge.

These methodological considerations are reflected in the three theses with which Habermas characterizes the discursive theory of truth in his article «Theories of Truth»:

> First thesis. We call truth the validity claim that we attach to the constative speech acts. A statement is true when the validity claim of the speech acts with which (...) we assert that statement is justified.
>
> Second thesis. Questions of truth are posed only when validity claims are problematized (...) For this reason, in discourses in which hypothetical validity claims are examined, the remarks concerning the truth of statements are not redundant.

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that is, the use whereby we contrast «true» to «justified» — is the only use that cannot be eliminated from our linguistic practices, since, in his mind, the other two uses can be easily paraphrased in terms that do not require the predicate ‘true’.

14 J. Habermas, «Was heißt Universalpragmatik?», in: *Vorstudien und Ergänzungen zur Theorie des kommunikativen Handelns*, Francfort 1984, pp.388-89.

> Third thesis. (...) Whether a state of affairs is the case or is not the case, is not decided by the evidence of experiences, but by the result of an argumentation. The idea of truth can only be developed with reference to the discursive cashing in of validity claims. (WT, pp.135-136)

The second thesis expresses the intuition, which is undoubtedly justified, that truth cannot be considered as «radically non-epistemic»: «true» is a predicate that we attribute to our beliefs; in this sense, there exists an internal connection between truth and knowledge. This point, in turn, justifies the third thesis, that is, the consideration that only an explanation of the function of such a predicate in the praxis of testing and revising our knowledge can exhaustively account for the meaning of this predicate without leading us to the conclusion either that the predicate is completely superfluous — in the sense of a theory of redundancy — or that any attempt to explain it makes no sense — as the deflationists hold — or is not possible — as Davidson[15] concludes.

The first thesis, though, contains the nucleus of an epistemic interpretation of the concept of truth because it affirms not only that there is an internal connection between truth and knowledge — in as far as the candidates for truth and falsity are our beliefs — but also makes the decisive step that leads to the epistemical conception of truth characteristic of discursive theory, since this thesis allows Habermas to reformulate the necessary and sufficient condition for truth stated at the start — namely, that «a statement is true if and only if it reproduces a real state of affairs or a fact» — in such a way that it is now possible to claim that «the truth condition of statements is the potential agreement of everyone else (...) The truth of a proposition means the promise to reach a rational consensus over what is said.» (WT, p.137) In order to evaluate the justification of such an epistemic conception of truth — in which truth does not depend on what is the case but rather on the rational acceptability of what is said — one must analyze in detail the argumentation that lies at the basis of such a thesis.

The connection between assertability and truth that is expressed in the thesis at issue is justified by the following reflection: «truth is a validity claim that we attach to statements when we assert them (...) In asserting something I make the claim that the statement that I assert is true. This claim I can make with reason or without reason.» (WT, p.129) From this follows, as Habermas subsequently points out, that «*the assertions can be neither true nor false*, but rather they are justified or not justified.» (ibid., my emphasis) This is undoubtedly correct, because the justification or rational acceptability of assertions indeed does not **only** depend on the truth of the corresponding statement. When I assert something I do not only make the claim that what is asserted is true but also that I **know** that it is true and that, when they are called for, I could give reasons that support my belief in the truth of such a

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15 This does not imply, of course, denying Davidson's thesis that truth is a primitive concept in the sense that it is undefinable, but only that it is possible to explain aspects of its use in the context of the revision of our beliefs that shed light on the meaning of this concept in its internal relation to other concepts, for example.

statement. As traditionally stated,[16] the necessary and sufficient conditions for establishing that someone **knows** something are the following ones: S knows that P if and only if

- (1) S believes that P
- (2) P is true, and
- (3) S is justified in believing that P.

The irreducibility[17] of these three conditions is evident: that my statement is de facto true (2) does not mean that I must know what is expressed in it, that is, that I can give reasons for my belief in it, and therefore that this statement is justified or rationally acceptable (3). On the other hand, it is not sufficient that I have good reasons[18] that support my belief in such a statement (3) for it to be true (2).

If we keep this in mind, the first thesis stated by Habermas, i.e. that «a statement is true when the validity claim of the speech acts with which (...) we assert this statement is justified», is either trivial or false. If the truth condition of the statement is that its assertion is justified, in the sense that it can be considered a «knowledge», then the thesis is trivial because, keeping in mind the justification conditions of something as «knowledge», with such a thesis we would only be asserting that the condition under which such a statement is true is that, among other conditions, it is true. Nevertheless, if what one is asserting as truth condition of the statement is that the corresponding assertion is justified in the sense that there are good reasons that support it (i.e. that the statement is rationally acceptable), then the thesis is false. The truth of the statement cannot depend on the justifiability (or rational acceptability) of the assertion, that is, the condition (2) cannot be reduced to the condition (3), as claimed by all epistemic theories of truth. That such a reduction is what the three theses imply is manifest in the conclusion that Habermas draws from them, which I have already mentioned, namely: that «the truth condition of statements is the potential agreement of everyone else (...) The truth of a proposition means the promise of reaching a rational consensus over what is said.» (WT, p.137)

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16 Although the conditions for knowledge that I point out here are usually attributed to Plato (in: *Theatetus* 201 and, maybe, also in *Meno* 98), my recapitulation of these conditions follows (with slight variations) the one given by A.J. Ayer: *The problem of Knowledge*, London 1956, p.35, and R.M. Chisholm: *Perceiving: a Philosophical Study*, New York 1957, p.16.

17 In this context I do not consider the difficulties pointed out by E. Gettier in his article «Is justified true belief knowledge?» (in: *Analysis* 23/6 (1963), pp.121-123) because they are intended to show the incomplete nature of these conditions whereas my argument is exclusively based on the unquestionable irreducibility of the same.

18 Unless we understand the expression «good reasons» as an «achievement word» (G. Ryle), that is, taking for «good» not those reasons that «could be considered by all as being convincing» but only those that are actually correct; this second use, nonetheless, obviously presupposes already the truth as a condition, that is, would be the result of taking together 2) and 3).

An essential difference that forbids the identification of the truth of the statement with the rational acceptability of the assertion is rooted in the **unconditional validity** that we suppose in the former but not in the latter. This difference appears clearly in two characteristic traits of the functioning of the concept of «truth» that Putnam[19] has correctly emphasized in his criticism of Dummett, namely: the **binary** functioning of the opposition true/false in contrast to the **gradual** functioning of the concept of justification or rational acceptability, and — derived from this — the **fixed** character that we attribute to truth in contrast to justification, that is, the fact that we consider truth to be a property that statements **cannot lose**.

Indeed, the unconditional validity that we attribute to truth is internally connected to the binary functioning of the opposition true/false because such functioning can be reconstructed as the expression of the following trivial condition: that «if a statement is true, it cannot be false at the same time». If to this condition we add the fixed character that we attribute to this property, it becomes clear that when we affirm the truth of a statement we are necessarily supposing something more than its rational acceptability, namely, that it **will not turn out to be false**.

This absence of analogy between the concept of truth and that of rational acceptability has also been emphasized by Wellmer, in his critique of the discursive theory of truth[20], when he insists on the «'plus' that the idea of truth contains with respect to everything that we may claim in each case to be well-grounded knowledge for us» (WB, p.340). The reason for such a fundamental absence of analogy is due, in Wellmer's opinion, to the fact that «a good grounding cannot guarantee by itself **the anticipation of a future accreditation** that is contained in truth claims» (ibid.) Precisely the fact that such an anticipation, inherent in the unconditional validity of truth, is absent in what is rationally acceptable allows for the conversion of truth into that instance which makes us aware of the **essential fallibility** of all knowledge: «truth is a regulative idea not in the sense that it refers to the **telos** — which may not be attainable — of the end of a pursuit of truth, of a definitive consensus, or of a 'final' language, but rather in the **critical** sense whereby we maintain with regard to all knowledge, all rational consensus, and even with regard to our agreement **in** language, a permanent reserve.» (ibid.) Here it is clear what is at stake if one accepts the identification between «truth» and «rational acceptability» proposed by the defenders of the epistemic perspective; as Wellmer says: «fallibilism is, so to speak, the explanation of the difference between assertability and truth.» (WB, p.342)

In order to render plausible this point of view in contrast to the epistemic conception of truth one would have to show, through an analysis of the «plus» that truth contains with regard to rational acceptability, that it is possible to account for

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19 See H. Putnam: *Reason, Truth and History*, Cambridge 1981, pp. 54ff.

20 This criticism is elaborated from different perspectives in the following writings of Wellmer: *Ethik und Dialog* [ED], Suhrkamp, Frankfurt 1986, pp.51-113; «Was ist eine pragmatische Bedeutungstheorie?» [WB], in: A. Honneth, T. McCarthy, et. al. (eds): *Zwischenbetrachtungen. Im Prozess der Aufklärung*, Suhrkamp, Frankfurt 1989, pp.318-372; «Wahrheit, Kontigenz, Moderne» [WKM], in: *Endspiele: Die unversöhnliche Moderne*, Frankfurt 1993, pp.157-177.

the unconditional validity of truth without appealing to the counterfactual supposition of a definitive consensus or an infallible «knowledge»; and this — as I will try to show in what follows — is only possible if one breaks with the interpretation of truth as an epistemic concept.

The defenders of the epistemic perspective try to preserve the unconditional validity of truth following the stretegy proposed by Putnam and Habermas, that is, by equating truth not with the factical acceptability but with the «rational acceptability under ideal conditions». This implies that the «anticipation of a future accreditation», pointed out by Wellmer, is interpreted as a counterfactual supposition of an **epistemic** kind; or, stated otherwise, as an epistemic promise of accreditation. Such an interpretation is explicitly made by Dummett in his article «What is a Theory of Meaning? (II)»[21] when he states that «an assertion is a kind of gamble that the speaker **will not be proved wrong**.» (p.126, my emphasis) Even Wellmer himself seems to interpret such an anticipation, inherent in the normative sense of the concept of truth, in **epistemic** terms when he remarks in his article «Wahrheit, Kontingenz, Moderne» that «whenever we raise truth claims based on good arguments and convincing evidences we **presuppose the epistemic conditions given here and now to be ideal ones** in the following sense: we presuppose that in the future there will not emerge arguments or evidences that put into question our truth claim (....) to be confident that the arguments are good ones and the evidences convincing means **to exclude the possibility that these will become problematic in the passage of time**.» (WKM, p.163, my emphasis)

As we can see, this strategy of interpreting the commitment implied in our assertion that a statement is true — i.e., that it will not turn out false — in the sense of an **epistemic** promise of accreditation, forces us to suppose, if only in counterfactual terms, an emphatic concept of «knowledge», that is, implies the **exclusion of a possible fallibility** of such knowledge. From this perspective, then, there seems to be a lack of justification for the fallibilistic intuition that Wellmer appeals to in order to account for the specific meaning of the concept of truth in contrast to that of rational acceptability — that is, its function as permanent reserve with regard to the essential fallibility of our knowledge. This conclusion is inevitable if we consider the general strategy inherent in the epistemic perspective.

In order to transmit the normative sense of the concept of truth to what is rationally acceptable under ideal conditions, one has to reinterpret the trivial condition, mentioned above, in such a way that it will be valid to say «if a statement is rationally acceptable under ideal conditions it cannot be false at the same time». In this sense, Putnam states: «the supposition that even an 'ideal' theory can really be false seems to collapse into pure unintelligibility».[22] Given that the absolute

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21 In: G. Evans/J. McDowell (eds.): *Truth and Meaning*, Oxford 1976, pp.67-137.

22 See footnote 6. Putnam has recently rejected (see «Comments and Replies», in: P. Clark/B. Hale (eds.): *Reading Putnam*, Cambridge, MA 1994, pp.242-295) his own conception of truth as «rational acceptability under ideal conditions». The only aspect that he maintains of the epistemical position is the intuition that a philosophically relevant explanation of the predicate 'true' has to analyze our use of this predicate in its internal relation with concepts

validity of truth now has to be derived from the absolute validity of what is rationally acceptable, this implies the presupposition of a consensus over what is rationally acceptable that, given such a validity, must be seen as **definitive** or **unrevisable**. This obligates, in turn, to presuppose counterfactually not only the rational justification of our knowledge but also the possibility of reaching an absolutely grounded consensus — grounded on a knowledge which is, therefore, absolute. In other words, it presupposes the possibility of a **definitive** cashing in of the truth claim raised in regard to such a knowledge. The attempt to explain the concept of «truth» in epistemic terms, that is, by placing it exclusively in relation to the concept of «knowledge», forces one to conceive the latter **nolens volens** as equally endowed with unconditional validity, and thus as infallible. Such strategy must necessarily fail the moment that it tries to explain the fallibilistic intuition to which Wellmer himself appealed, that is, when it tries to explain how the concept of truth **makes compatible the unconditionality** inherent in its validity with its function of **fallibilistic reserve** with regard to the validity that we attribute to our knowledge.

Keeping in mind the above, it seems clear that any attempt to articulate an alternative would require a different interpretation of the unconditional validity of truth, or, put otherwise, would have to show that the commitment acquired by the speaker in asserting that a statement is true — namely: that it will not be false — is not correctly interpreted if one understands it as an **epistemic** anticipation of accreditation. Now, in order to achieve such an epistemic neutrality it would be necessary to appeal to a concept other than «knowledge». For this reason, the explanation in non-epistemic terms of such a normative commitment — which I will try to render plausible in what follows — is based on a **realist** strategy to the extent that it corresponds to the attempt of deriving the unconditional validity of truth not from its connection to the concept of «knowledge» but from its connection to the concept of «reality».

As we saw previously, the commitment acquired by the speaker in asserting that a statement not only is rationally acceptable but is also true manifests itself in that the speaker inevitably must suppose that in the future such a statement will also not turn out to be false. Such a commitment obviously proceeds from the binary functioning of the opposition true/false: to assert that a statement is true implies a commitment that such a statement is not false — given that it cannot be both things at once. Now, precisely because of this, such a commitment does not imply any

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such as «rational acceptability», «epistemic conditions», etc. But now he rejects the veritable concession he had previously made to the epistemic position, namely, «the idea that truth could never be totally recognition-transcendent» (p.243). To explain this rejection Putnam appeals precisely to fallibilism: «Not only is truth not always recognizable by using anything that could be called a decision procedure, even under the best epistemic conditions; it is obvious that, in the case of empirical statements, decision as to truth are generally defeasable (and so are decisions as to whether one's epistemic position is good enough to decide on the truth of a statement)». (p.289, my emphasis) Given that this change of position is a recent one it remains to be seen if this argument will make Putnam, by his own logic, recognize that the very idea of «an 'ideal' theory», that is, of a theory that **could not be false** cannot be sustained.

evaluation of the quality of the reasons that support the assertion of the statement, that is, it cannot be understood as an **epistemic** anticipation (of my incorregibility) but exclusively as a condition of a logical nature, namely, that the statement will not turn out to be false, **if** it is true. This condition, as such, only commits, in a strict sense, to the recognition that the statement **either is true or is false**, and, thus, that the testing of the reasons supporting such a statement will have to be directed toward the exclusion of one of the two possibilities. Such a supposition is too modest for it to contain an epistemic promise of future accreditation, since the epistemic sense inherent in the supposition not only does not imply an irrevisability of my beliefs but what it actually anticipates is **the obligation to revise the acceptability of contrary beliefs**: if the statement turns out to be false, if the reasons submitted for examination make this manifest, I will not be able to continue asserting that it is true (or that it used to be true). In any case, if I were to continue affirming that it is true — in spite of my inability to give reasons for its rational acceptability — we would find ourselves in the situation pointed out at the beginning: no one would accept that such a statement amounts to «knowledge».

Indeed, given that «knowledge» has as its necessary condition «truth», it does inherit, in a certain sense, the unconditional character of the latter; this inheritance is shown by the absolute character of the opposition knowledge/error. Now, such an opposition also cannot be understood in the sense of an emphatic concept of non-fallible knowledge, that is, our claim to knowledge cannot be interpreted as connected to an anticipation of incorregibility — as Dummett and Wellmer suggested. In the same way that anticipating that **if** the statement is true then it **cannot** be false at the same time, means anticipating a condition and not the **satisfaction of one of the two possibilities** (except as a mere forecast of subjective probability), so in the case of the opposition knowledge/error, anticipating that **if** I know something then I **cannot** be wrong at the same time about it, means a commitment to that excluding condition and not the anticipation of the satisfaction of one of the two possibilities. Such a condition only implies the exclusion of the possibility that both things — that I know and that I am wrong — can turn out to be valid **simultaneously**, but it does not anticipate a situation in which I could not be wrong. That if I know something I cannot be wrong does not mean that there is a situation in which it is impossible for me to be wrong, i.e. in which my belief would be **necessarily certain**, but only that it is impossible for there to be a situation in which I know something and at the same time I am mistaken. From this one can deduce only that there are possible situations in which I am not mistaken, in which **de facto** my belief satisfies the conditions mentioned previously — i.e. that the belief is justified and is true — and that such situations are, by definition, the only ones that count as «knowledge». If we keep this in mind we cannot say, in a strict sense, that «an assertion is a kind of gamble that the speaker will not be proved wrong» — as Dummett asserted — nor can we say that it implies the anticipation that «in the future there will be no pertinent counterarguments» (ED, p.83) — as Wellmer affirmed — but only that, if there are such counterarguments, if the speaker really turns out to be wrong, then she will obviously have to retract her claim to «knowledge».[23] The epistemic consequences of the excluding condition inherent in

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[23] That an antifallibilist interpretation of the opposition knowledge/error cannot be extracted from our use of such a pair of concepts is shown by the fact that it is neither contradictory nor problematic to say «I believed that I

the opposition true/false — namely, that our statement **either is true or is false** — more than implying any incorregibility seem to be, in fact, clearly fallibilistic.

In order to explain why the concept of «truth» is tied to such a binary condition, or, put otherwise, why the absolute validity that we suppose of truth forces us to accept such an excluding condition, one has to keep in mind the internal connection between the concept of «truth» and that of «reality». Since only with the supposition of **one** objective world can one understand why a statement **must be true or false** and, along with this, why the search for a rational justification of the statement **must adopt precisely the form of excluding one of the two cases**. This intuition of **tertium non datur** inherent in the concept of «reality» — that is, inherent in the absolute character of the opposition «is the case/is not the case» from which depends the truth or falsity of the statement — is precisely the intuition that cannot be extracted from any epistemic concept of rational acceptability (among other things because there are contexts of rational justification that work in a different way for example, those in which we do not suppose an unconditional validity to our beliefs, like in the case of ethical convictions relative to what is good for me).

For this reason, even though from an epistemic point of view we cannot understand reality other than as «the correlate of the totality of true statements» (TKH, p.125-26) — that is, as the set of all facts expressed by true statements — there is a **formal** aspect inherent in the concept of «reality» that is not exhausted in its epistemical correlate: the absolute, non-relativizable character that we associate to this concept and that is manifest in our binary, non-gradual, use of the opposition real/unreal[24]. Such a formal component of our intuitive understanding of the concept — undoubtedly non-epistemic — of «reality» becomes manifest in the form of an essential and inevitable supposition of our practices of revising our beliefs, namely, the counterfactual supposition of **one objective world**. Such a supposition brings with it the principle of bivalence which is subjacent to the binary use of the opposition true/false and is responsible for the validity, transcendent of every context, that we attribute to truth. Only because truth is conceived as depending **exclusively** on what is the case can it preserve its unconditional validity **with respect to any epistemic criterion whatsoever** of rational acceptability and, vice versa, only because these criteria are necessarily dependent on a **non-epistemic** instance are they inevitably conceived (without exception) as, in principle, fallible. The internal relation between the concept of «truth» and the concept of «reality» is, for that reason, what permits to combine the unconditional validity that we attribute to truth with the application to instances, to beliefs which are more or less justified, whose validity can never be unconditional. In this sense, the transcendence of every context that we suppose to the validity of truth — because of its dependency on **one** reality, on **one** objective world — is nothing other than the correlate of our fallibilistic understanding in relation to all knowledge.

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knew it». My belief that I know something can turn out to be as wrong as any other belief.

[24] This opposition can be understood both in the sense of the opposition «exist/does not exist» (relative to the reference of the terms) and in the sense of the opposition «is the case/is not the case» (relative to the truth of statements).

Precisely such a **formal** explanation of the concept of «reality» in these terms can be found in Habermas's *Theory of Communicative Action* when he points out that: «validity claims are in principle susceptible to critique because they are based on **formal concepts of world**. They presuppose an identical world for **all possible** observers or a world that is intersubjectively shared by **all members** of a group, and this **in an abstract form, that is, disconnected from all concrete contents**.» (TKH, 1, p.82) The merely **formal**, counterfactual presupposition of one objective world, identical for all observers, appealed to by the transcendence of every context inherent in the unconditional validity of truth, does not imply, therefore, an epistemic access to any «world-in-itself»[25] but is simply the other side of our fallibilistic intuition about the revisability of our knowledge; it is simply — as Habermas himself points out — the supposition that allows the speakers «not to **pre-judge**, with regard to **content**, the relation between language and reality, between the means of communication and that about which there is communication. Under the presupposition of formal concepts of world and universal validity claims, the **contents** of the linguistic picture of the world must remain **separate from the order itself that is supposed to the world**.» (ibid.) **The reflexive capacity that lies under this fallibilistic renunciation** — which permits us to consider our beliefs as distinct «from the order itself that is supposed to the world» but dependent on it — **could not be obtained without that normative «plus» that the concept of truth possesses with regard to that of justification (or rational acceptability) thanks to its ultimate realistic sense, that is, to its internal connection to the concept of «reality»**.

If one keeps in mind this explanation of the concept of «reality» in formal-pragmatic terms — which, evidently, manages to avoid any metaphysical interpretation of the concept — it seems clear that the discourse theory of rational acceptability developed by Habermas does not depend on the anti-realist turn which is proper to the epistemic conception of truth, because by recurring to such a concept — which is already at our disposal in the theory of communicative rationality[26] —

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[25] To insist on the realist meaning of the concept of truth does not require adopting any concrete position in relation to the question of our epistemic access to the world. To that extent, the explanation of rational acceptability given by the discourse theory of Habermas, in itself — that is, in so far as it merely gives an answer to the epistemical question — is immune to these realist considerations. This can be seen in the central intuition of Habermas's discourse theory in relation to rational acceptability, namely: that «the satisfaction or non-satisfaction of truth conditions can only be stated through the argumentative cashing in of the corresponding validity claim.» (*Die Neue Unübersichtlichkeit*, Frankfurt 1985, p.228, my emphasis.) Undoubtedly, this discursive conception of rational acceptability is more convincing that any position of epistemic (or metaphysical) realism that has to appeal, in order to explain rational acceptability, to a correspondence or a causal relation between our beliefs and the «world in itself».

[26] In spite of the fact that Habermas introduces the formal concepts of world expressly as a correlate to the universal validity claims and even in contrast to relativist positions, such as Rorty's, he indicates that: «in the pragmatics of every use of language there is included the supposition of a

it is possible to avoid the two problematical traits of every epistemic conception: on one side, the renunciation to give an account of the **realist** sense of such a concept, either by eliminating it or substituting for it the concept of «rational acceptability under ideal conditions»; and, on the other side, the recourse to an emphatic concept of infallible knowledge in order to preserve the unconditional validity of truth.

To insist in the **realist** sense of the concept of truth, that is, to maintain — as Habermas himself does at the beginning of his article «Wahrheitstheorien» — that the only necessary and sufficient condition of the truth of a statement p is **that it be the case that p**, loses its triviality precisely when one situates such a condition in the context of an explanation of rational acceptability, that is, when one explains the function of fallibilistic reserve that such a **normative** supposition carries out in the context of testing and revising the rational acceptability that we attribute to our beliefs — by making us conscious of the permanent possibility of having to revise these beliefs, or the criteria of acceptability that support them, in relation to a reality that is logically independent from them.

In its turn, this fallibilistic consequence shows that keeping the connection between «truth» and «reality» further allows us to elude a prolematical recourse to an emphatic concept of «knowledge», that is, a knowledge that, in order to maintain the unconditional validity of truth, must be conceived as infallible: if one brings back such unconditional validity to the internal connection between «truth» and «reality» it is possible to explain the connection between «truth» and «knowledge» without recourse to any supposition of incorregibility.[27]

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shared objective world» (in: *Nachmetaphysisches Denken*, p.178). In order to defend such a position one would have to specify **where o by means of what is this supposition anchored in every (cognitive) use of language**. Such specification could be attained by means of a theory of reference that showed such a supposition as one of the inevitable normative presuppositions tied to the activity of referring that is proper to the cognitive use of language (as opposed to, say, the fictional use of language) as well as through a clarification of the realist sense of the concept of «truth», in which is shown the important normative function of such a supposition in our practices of revision and testing of the rational acceptability of our knowledge.

[27] This paper has been translated by by Miguel E. Vatter.

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Abstracts of the Papers

ABSTRACTS OF THE PAPERS

Natural Kinds and Projectible Predicates

The focus of this article is on the pragmatic presuppositions involved in the use of general terms in inductive practices. The main thesis is that the problem of characterizing the assumptions underlying the projection of predicates in inductive practices and the ones underlying the classification of crtain general terms as «natural kind terms» coincide to a good extent. The reason for this, it is argued, is that both classifications, «projectibility» and «natural kind term», are attempts to answer to the same semantico-epistemological phenomenon, viz. underdertermination. It is proposed a «deflationary» (i.e. non-essentialist) reading of the so-called «theory of direct reference» as to enable an evaluation of its contribution to epistemological problems associated with this kind of phenomena, as well as it is argued that a purely de facto account of projectibility (i.e. entrenchment) is not viable. The resulting hypothesis is that the conception of «natural kind terms» is only interesting insofar as they are seen as a kind of projectible general terms and thus as parts of classifications used in natural science, more generally, in inductive practices, and that this is a perspective that makes undue metaphysical readings avoidable.

Axel Mueller

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The «Right» Approach

While discussions about improving society are commonly conducted in terms of human rights, there are serious drawbacks to this approach. People may differ as to the relative importance or the very existence of specific rights, and there are no generally accepted methods for the rational resolution of such disagreements.

These difficulties can be avoided if proposed social changes are discussed with respect to a generally accepted end, rather than with respect to a set of rights. And agreement on such an end already exists, inasmuch as most advocates of social improvements want to see social arrangements changed in such a way that everyone will be able to lead a satisfying existence.

Ronald A. Cordero

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Meaning Realism and the Rejection of Analyticity

There is a widespread view in philosophy of language and in philosophy of mind according to which the «quinean» rejection of analyticity can be made compatible with some sort of realism about meaning. Against such compatibilist claim, Paul Boghossian (1993) has recently held the thesis that one cannot coherently reject the analytical/synthetical distinction maintaining at the same time a meaning realism. His arguments are very pervasive, but they can be replied. The main objective of this paper is to show that in fact it is possible to reject analyticity being at the same time a meaning realist, even a meaning realist of a non-holist kind. The prevailing view is basically right. Moreover, it is possible to go on maintaining the compatibilist claim in its most radical form. In short, even if we adopt a non-holist meaning realism, we must reject analyticity because language is always conceptually motivated and engaged with reality. There is no linguistic arbitrariness. That forces us to go far from classical conceptions of meaning and to have a much more pluralistic one. With respect to it, for instance, to say that some statements are true once their meanings are fixed would not entail that they are true by virtue of meanings. The problem to get such a conception of meaning remains open. However, the reasons against analyticity do not force us to any irreductible meaning holism.

Manuel Liz

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Epistemic Values in Science

The paper is a critical examination of some aspects of Laudan's views in his book *Science and Values*. Not only do the aims of science change; there are axiological disputes in science as well. Scientific disagreements are not solely theoretical or methodological. Progress in science consists not only in developing new theories more suitable for implementing certain epistemic values than earlier ones but also in reaching a deeper understanding of those values. The paper considers whether there are principles to guide axiological choices in science, whether the task of assessing the legitimacy of goals makes any sense. Larry Laudan's criteria to settle questions concerning the aims of science are critically canvassed. According to Laudan, axiological choices are on the same footing as the theoretical and methodological ones: all of them may be objectively grounded. The generality of the principles and their naturalistic flavour are the most remarkable merits of Laudan's account but the results are rather meagre. His principle of coherence may be, in the end, a mere *a posteriori* justification of changes in axiological direction carried out by the scientific community. The rejection of a demonstrable utopian goal, granting naturalistic assumptions, is completely sound but it has a very limited scope. The paper suggests that science could not *demonstrate* much about goals. From the rejection of semantic utopianism we can draw a need for a previous clarification rather than sustantive criticisms and, finally, Laudan's charge of epistemic utopianism is very controversial.

Valeriano Iranzo

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When Is If?

This papers deals with examples offered by Adams, Austin and others which seem to show that ‘if’ does not conform to all of the laws of the conditional. These a reconciled by treating them as conjunctions with embedded modalities.»

M. G. Yoes

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Truth, Knowledge and Reality

The main argument of this article is that the concept of truth is as much internally linked to the concept of knowledge as to the concept of reality. As a consequence it is affirmed that all attempts to explain its structure which are either exclusively biased in an epistemic point of view (that is, which connect only truth and knowledge) or in a purely realist metaphysics (which only connect truth and reality) are bound to fail. Instead this article proposes the adoption of a pragmatic standpoint which would permit to reconstruct the fallibilistic role displayed by the concept of truth in the epistemic practices of belief-revision, which must in turn be reconstructed precisely taking in account the connection of truth and reality. In that way both intuitions as to the concept of truth, the epistemic and the realist one, can be reconciled. Moreover this strategy provides as such, if correct, a strong argument in favor of an essential function of the concept of truth against contemporary deflationist tendencies.

Cristina Lafont