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VERA BÉKÉS

TOWARDS THE RECONSTRUCTION OF A "MISSING PARADIGM"

"Was sich aufhebt, wird dadurch nicht zu Nichts."

Hegel

"How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?"

T.S. Kuhn

According to his influential conception of paradigm, Thomas Kuhn asserts that the approach of a scientific revolution is signalled by an increasing number of anomalies (irreducible paradox) inside normal science. The scientific community is not easily persuaded that a problem can be such an anomaly: there are always some who are eager to refute the "scandalous" theorem, and others who try to develop alternative theories to avoid the emergence of paradox. The problem leads to crisis when these "extraordinary" endeavours, as it seems to normal research, do not succed. However, researchers who have, from the very beginning, regarded the problematic theorem as an anomaly insolvable within the prevailing paradigm, now more or less find themselves rooted in a *different* paradigm, and pass their judgements from that new standpoint.

In the philosophy of science, the theorem of incommensurability, and the meaning variance theorem connected to it, seem to be precisely such and anomaly. As is well known, these theorems assert that, in the absence of an absolute, neutral and independent "measuring rod", the consecutive paradigms are categorically, methodologically and epistemologically, etc., incommensurable. What is more, the terms used in these theories undergo a radical change of meaning. Thus, the theorem which was originally used to explain the radicality of shifts in the paradigms of the special sciences, and to render many paradoxes in the history of science interpretable, has itself created a new, meta-level paradox, emerging as *an anomaly within the philosophical systems* themselves which explore the history of the development of the sciences.[1]

Logical positivists have tried to refute this theorem, which they consider to necessarily involve both irrationality and relativity. But their arguments have either turned out to be irrelevant, or, if acknowledged as relevant, their only effect was to push the radicals towards a more consistent formulation in order to uphold the theorem of incommensurability and meaning variance. (It is worth mentioning that opponents of P. Feyerabend could attack his, in some cases, ambivalent, pre-"Against Method" views, but never the principle of incommensurability itself.)[2]

The "moderate" philosophers have tried to eliminate the incommensurability problem by developing alternative models. Their aim is, in direct opposition to Kuhn, to develop an evolutionary model wherein this paradox does not emerge at all.[3] Only a small group (first of all Kuhn, Feyerabend, Hanson and Toulmin) consider the incommensurability theorem to be a revolutionary, that is, "fatal" anomaly. For this reason, they are regarded as radicals. They seak a solution within the classical categorisation of rationality, which has been traditionally accepted since Descartes (but not before).[4]

In this paper, I will try to outline a conception which involves incommensurability, but does not view it as a disastrous catastrophy. On the contrary, I will use it as a *starting point, a presupposition.* All the paradoxes which have been formulated to refute this theorem will not only seem theoretical, and not at all following from the theorem as its consequences but, *just to the contrary, the theorem provides a formulation of real historical phenomena,* which occur when there is a radical transformation of scientific knowledge between two consequtive paradigms. (As long as we consider only two paradigms, the paradox will remain insolvable.)[5]

My starting point is a logical consequence of applying the principle of discontinuity to the history of science. The mere idea of historical continuity and cumulativity being a *myth* (without accepting the more radical incommensurability theorem) opens new perspectives for research in the history of science. In the same way, *using historical facts, and without discarding any radical conclusions, the results may even modify the theory of shifting paradigms*. Moreover, a conscious application of this theorem as a presupposition may reveal why its advocates had to stop short (that is to say, why the problem of incommensurability actually remained an anomaly). We may also get a glimpse of that new model which will be based on facts of the history of science, and in which the anomaly of incommensurability "can be made law-like".[6]

2. We can draw some interesting conclusions from the incommensurability theorem on the nature of anomalies in general.

What is the origin of these "fatal" anomalies? Kuhn answers this question - although not in an entirely resolute way -, saying that it is the paradigms themselves which produce the anomalies.[7] He believes this to be the case in the problem of incommensurability as well, and he thinks it is these very circumstances which make him stop short. All attempts at eliminating this problem must be hopeless, but, "...in the absence of a developed alternative", he finds it impossible to entirely relinquish the epistemological viewpoint that has largely guided Western philosophy for three centuries, although he feels it no longer functions effectively.[8] Kuhn also refers to "... what might, in *another* philosophical climate, have been taken for granted" (my italics, VBJ), precisely in the context of the equality of meaning variance to an altered world.[9] Feyerabend puts it in a similar way: "Incommensurable theories, then, can be refuted by reference to their own respective kinds of experience (in the absence of commensurable alternatives these refutations are quite weak, however)."[10] To put it in another way, the followers of the incommensurability theory claim that it is precisely the *absence* of commensurable alternatives which effects its paradoxical conclusions. However, it is to be noted that Kuhn formulated this theorem and the whole paradigm conception in general, as if from outside, on the basis of *another* paradigm.[11] As far as Kuhn's sources are concerned, it would seem that the majority lie beyond the atomist, inductivist, sensualist, etc., paradigm, and the same can also be said for Feyerabend. One could say that these elements are already conceptions belonging to a new paradigm. (Kuhn also says that "often a new paradigm emerges, at least in embryo, before a crisis has developed far or been explicitly recognized.")[12] The constitutive role played by these "other" (for the time being let us suppose new) embryo paradigms in formulating the theorem makes it questionable whether the anomaly has indeed come into being within the prevailing paradigm. If we set out to investigate the sources within their historical context, the whole problem might receive a new and unexpected perspective.

3. The application of the principle of discontinuity in the history of science has already changed, to a great extent, the overall attitude of the historigaphy of science: the methods, and also the subject matter, have undergone modification. The aim has ceased to be the search for forerunners and doing them justice under the aegis of the cumulativity myth. Now research is aimed at a discontinuous historical reconstruction of competitive scientific paradigms. As the paradigm theory showed that, in the absence of a means of neutral measurement, the border between conceptions regarded as scientific, or pseudo-scientific, is relative and often arbitrary, Kuhnian historiography extends into territories barred by positivism. (Of course, this type of historicity does not go back to Kuhn himself, and it is significant that

one of Kunh's basic sources is the work of the non-positivistic Duhemian, Alexandre Koyré. This is again characteristic of the anomaly-generating capacity of the "other" paradigm.)

The research done by the new generation has, understandably enough, turned towards epochs and topics which have, until now, counted as "terra incognita". This throws light on two *imbalances* which are even characteristic of non-teleologistic reconstructions. On the one hand, the overwhelming majority of research has been directed towards the origin and development of *natural sciences*. Research directed towards social sciences, itself on a much smaller scale, has shown little interest in the concept of paradigm. Nevertheless, the majority of sources contributing to the elaboration of the paradigm theory and the incommensurability problem *are not* from natural sciences, but from social science. On the other hand, there is a similar imbalance in that the reconstructions in the history of science (which, in the literature, mostly means natural science) usually come to an end at the beginning of the nineteenth century. They then skip over a *dim epoch* of several decades, resuming the reconstruction somewhere around the 1880's, thus concentrating on the direct precedessors of the 20th century. These imbalances seem to be significant, and can only partly be explained by the survival of the cumulativity myth. This odd situation is highlighted in the fact that the sources in social sciences which influenced Kuhn are theories which are *not integrated into the positivist paradigm*. They are, metaphorically speaking, "*inclusions*", or "*islands*", and they have their origin in the *pre-positivist period*, that is, *precisely in that dim epoch* which falls beyond the above-mentioned, historical investigations.

We have more and more reasons to assume that this epoch, which remained outside the scope of historical research, and, what is more, of *our historical consciousness*, seems to be *an independent, sui generis, but lost or "missing paradigm"*.

Contrary to the generally held belief that, since Descartes, the sciences have been based on a homogeneous tradition with regard to their epistemological, logical, methodological, ethical, etc., aspects, we must now acknowledge another, forgotten paradigm which constituted a different, *non-Cartesian alternative*. In addition, this alternative not only existed as a philosophical (epistemological, antropological, etc.) conception, (it was recognised as such in the great *dialectical period* of classical German philosophy) but, at its height, the "missing paradigm" had, in all probability, full control over the scientific theory and praxis of the era. This paradigm had its origins in the conflict with Enlightened Rationalism and perished in the fight against Positivism.

The fact that this paradigm is lost to our present range of historical knowledge may substantiate the theorem of incommensurability and radical meaning variance: as long as there are two paradigms and they are incommensurable, there is no possibility of translation between them, and the conquered paradigm will *vanish*. Examining the history of the "missing paradigm" may throw light upon the real function of the principle of incommensurability.[13]

4. But, adhering to the presupposition that elimination in the sense of "Aufhebung" does not mean entire liquidation (this kind of dialectics is again part of the "missing paradigm"), we can conclude that the vanished paradigm has, in fact, partly survived. Theories on the development of science do not give due importance to this phenomenon, for they only know the alternatives of either accepting the victorious paradigm or facing excommunication from science. However, it is also possible that, after the disintegration of the old paradigm, fragments of it survive as "*inclusions*" which the conquering paradigm cannot either liquidate or integrate. We can come to some understanding of the nature of an unintegrated conception by drawing the following comparison, using the original meaning of 'paradigm': an unintegrated and unintegrateable theory behaves *like the irregular conjugation of a verb in grammar* (a verb which does not follow the usual pattern). Every natural language is known to have such irregular conjugations,

which are, in fact, *fragmented remnants of lost, archaic paradigms.*

A common feature of such enclosed conceptions is that their structure and attitude are incommensurable with the prevailing logic. Owing to radical meaning variance, their subject matter is so different, and their means of discussion (methodological incommensurability) so deviant, that the conquering paradigm will never be willing and able to incorporate them. However, due to various factors, their existence must be tolerated.

So the vanished paradigm continues to exist in the form of "inclusions" (tendencies outside the mainstream of science, separate schools, isolated chairs at universities, or single researchers), and its latent existence has extraordinary significance in the elaboration of new, emergent paradigms. The fact that a conception is an inclusion involves a specific relation which always expresses the mutual positioning of *three* paradigms (the eliminated (A), the prevailing (B) and the forthcoming (A′) paradigms). The consecutive paradigms are incommensurable, although there is a historical genetic continuity between the ceased paradigm (A) and that which is forthcoming (A′), regardless of whether those in an "inclusion" position are aware of it (traditionists) or not (pioneers). If we examine the history of science from this angle, we find that, in most cases, those who champion the new paradigm (A′) conflict with the prevailing one (B) because of their affinity with the logic of the missing paradigm (A). That is why they eventually develop their incommensurable, "new" alternative, which is at the same time "old", corresponding to the paradigm before last. However, since they are mostly unaware of the missing paradigm as their actual frame of reference, they consider their sources, identified as inclusions, to be the first, embryo appearances of the forthcoming victorious paradigm (A′). I could indeed state the most important point in my paper as follows: the new paradigm overcoming the actual one

(B) is incommensurable with (B) but, nevertheless, it is commensurable with (A) (the missing one, whose existence is to be historically acknowledged), *therefore it will be (A'), not (C).*

According to their general affinity with the logic incommensurable with the prevailing paradigm, inclusions can be assigned to four *stages*.

(i) During the normal period of the prevailing paradigm (B):

First stage: (which could also be called "zeroth stage"): entirely isolated conceptions superseded and crowded out of science. (Usually we are not aware of them at all, or they are accounted for as curiosities or negative examples.)

Second stage: surviving schools and tendencies. The normal scientific community holds the opinion of their presuppositions that "though this be madness, yet there is method in't". Generally, questions posed by these schools do not in fact cause problems because (and this is one of their main characteristics) they are not even worth arguing about, they are simply avoided by the prevailing paradigm. Their existence at all is seen as an anomaly, especially when the prevailing paradigm holds methodological monopoly to be an eminent value. Inclusions of this type consciously preserve continuity and their channels of transmission are known to them. They do not contest the theories of the victorious paradigm, as they preserve and reproduce, in relatively clear contours, the characteristics of the *normal period of the missing paradigm.*

Third stage: Theories *in opposition* to normal science (B), where the dimensions of controvery are unclear to normal science. At first these theories exert an all round effect in which, however, the original intentions are misunderstood. Thus they are present in the collective consciousness and, after the first "refusing" generation (contemporaries), the "interpreting" generation (the disciples) follows. In this epoch, certain elements of the theory may penetrate normal science, contributing to its growing sate of crisis. If the disciples are not "well-versed" enough, and the conception itself is rather important, the role of the penetrating ideas, even though their original import cannot be substantially integrated, may become decisive in

(ii) the critical period of the prevailing paradigm (B).

At this point, the third generation appears and this generation is always very responsive to the original conceptions. They do not interpret; instead, they interiorize the counter-prevailing view-points and attitudes characteristic of the inclusion.

The authors concerned with inclusion theories at this stage are not usually conscious of their connection to the missing paradigm. The channels are hidden. Their itentions are identical to the program of the *extraordinary* phase of the missing paradigm, although there is no point of reference and no feeling of community. They start their fight "under the nose" of a prevalent paradigm. This temporarily very disadvantageous fight demands the most devoted intellectual courage and creativity, yielding an independence which may prompt new, original thoughts.

Fourth stage: Theories formulating the "fatal" anomalies of paradigm (B), itself in crisis, belong here. At this point, they are no longer inclusions, but are rather theories belonging to the new paradigm (A'). Its representatives are partly the disciples of the second stage in the formation of enclosures and partly the third generation followers of the third stage, who can, with their "new" theories, successfully integrate conceptions which had hitherto been indigestable. In their revolutionary offensive, they take their selectional aspects mainly from the results of the third stage. I believe, for example, that the Wittgensteinian distinction between private language and non-private language conceptions can prove very fruitful far beyond the philosophy of language, in discovering paradigmatically different viewpoints. (This distinction is no less operational than the widely accepted heliocentric, non-heliocentric, division of modern astrophysics.) The second stage mainly offers a pattern by which the new paradigm (A') can be made more coherent. This stage and the first stage also involve curiosities for later historiography, because they will be the main target of the "rehabilitating" or precursor vindicating manoeuvres of the history of science. This ideological, self-asserting component of "doing justice" to predecessors can hardly be eliminated from historiography. However, it will contribute to the actualizing of the potentials of the missing paradigm, which is preserved in inclusions.

5. In the ensuing example, I will illustrate my case. Kuhn mentions, in the Preface to his *Structures*, the importance he placed on his discovery of B. L. Whorf's theory regarding the effect languages exert on worldview. Kuhn's arguments for the incommensurability theorem nearly always seem to be Whorfian and if they aren't, then this deviation produces severe consequences. It is worth mentioning that neo-Humboldtian linguistics, principally associated with F. Boas, E. Sapir and Whorf, forms no organic part of linguistics as such. It is an unintegrated conception, an inclusion. Humboldt himself cannot be accounted for in the traditional historiography of linguistics.[15] The explanation for this is that contemporary linguistics did not come about from nothing, from the sphere of "unscience". It emerged from a fight against "philosophical linguistics" linked with German Sprachphilosophie and directed by the now missing paradigm. Humboldt was the most outstanding figure of this school holding a non-private conception of language, which was eventually overthrown by positivism. The American neo-Humboldtian school of linguistics is only an inclusion belonging to the second stage of a missing paradigm. Their fundamental theorem, drawn from

Humboldt, has played a decisive role in the formulation of the incommensurability problem: the native language influences decisively and a priori our way of perceiving and interpreting reality. Non-privatistic epistemological presuppositions have *paradigmatically demarcated* this theory from normal linguistics, which now stands on a positivist, privatistic base, and which has been homogeneous since the appearence of the "Young Grammarians". When Kuhn thus argues about the conversion, he is "conjugating according to the missing paradigm", albeit unconsciously because he sees the difference between our relationship with our native language and with foreign languages to be paramount. (We find the same distinction in Wittgenstein's conception: when asking for meaning, we should look at how children start to use language. When learning to speak, we also learn to experience, and so our experience is determined and regulated by the native language of our community. This process is *unreflected*.)

It is now more understandable that in the discussion on incommensurability and meaning variance, the different partners were not talking about the same thing: *they gave different meanings to the same terms*. Logical positivism, and private language conceptions in general, find no philosophical difference between the learning process of the native language and that of a later-acquired language. According to this view, people acquire both by translating their direct, neutral sense-data and thoughts into a common language, which, as a mechanical tool for communication, is only secondary to private *thinking*. We can conclude that the problem of radical meaning variance is *essentially incomprehensible on the basis of private (positivist) theories*. The origin of Kuhn's problem lies with an inclusion with a non-private basis and, for this reason, he is able to conclude the following:
"To translate a theory or worldview into one's own language is not to make it one's own. For that one must go native, discover that one is thinking and working in, not simply translating out of, a language that was previously foreign."[16]. He is con-

sequently arguing that the shift, the conversion which comprises the very heart of revolutionary process is not a matter of individual deliberation and choice. In many cases, a scientist who has made this choice intellectually has perceived that "... the conversion required if it is to be effective eludes him. He may use the new theory nonetheless, *but he will do so as a foreigner in a foreign environment, an alternative available to him only because there are natives already there.* His work is parasitic on theirs, for he lacks the constellation of mental sets *which future members of the community will acquire through education.*"[17] (My italics, V.B.)

Kuhn, however, does not seem to be aware that the distinction between the native language and a foreign language - together with its philosophical presuppositions - belongs to the missing paradigm. Thus, his arguments "in the absence of a developed alternative" are now becoming uncertain and controversial: "...*In the absence of a neutral language, the choice of a new theory is a decision to adopt a different native language and to deploy it in a correspondingly different world.*" (My italics, V.B.) Then he just adds "... That sort of transition is, however, not one which the terms 'choice' and 'decision' quite fit, though the reasons for wanting to apply them after the event are clear".[18] However, he has conceded on important issues to his critics from the standpoint of logical positivism. According to the original non-private notion, such choice or conversions are *incomprehensible*. The idea of 'another, new native language' is absurd, or, to put it another way, the grammar of the notion of a 'native language' *excludes* the possibility of its connection with the words 'new', and 'another'. If *such* a conversion still exists, his critics have every right to challenge Kuhn by asserting that, in this comparison, "just the processes before and after the acquirement of a new language remain unnoticed. We learn a new language with the help of the old, and we do not generally forget the old after having acquired the new."[19].

It is not by chance that Kuhn fails to notice the linguistic process of acquiring. According to the original paradigm, the native language is the *first* language, *not acquired by translation*. The native language is therefore *without* any lingual precedessor. Obviously, Kuhn is forced into this paradox by his presupposition that "new paradigms are born from old ones".[20] By contrast, if we consider the effects of a third, latent paradigm, this assumption is modified: new paradigms are not born from the prevailing old ones, but from inclusions in which a third (missing) paradigm survives within the prevailing paradigm. All paradigms produce insolvable problems, but their perception (their definition as fatal anomalies) originates from outside, from the inclusions. Inclusions increase the number of anomalies by "smuggling" incommensurable problems into the paradigm. Consequently, those speaking the native language of the new paradigm must already have been "native speakers" of this old-new language before the revolution and, owing to the inclusions, they have learnt a responsiveness which is incommensurable with the prevailing logic. Occasional moments of failure can be explained by the fact that the adherents of the new paradigm have not attained an adequate knowledge of their own operating frame of reference. The same holds true for the incommensurability problem: it will not be a "law-like phenomenon" but an anomaly as long as it is not reflected in its own commensurable basis. Until this occurs, they use the terms, even while posing the questions, in a sense which is interpreted, namely distorted, by paradigm (B). When there is a radical shift in paradigms, a radical conversion or translation is only possible in the debate if the party to be converted already has some sort of latent "inclusion experience". In order to have a real dialogue and not to talk through each other, the participants must resort to such experience as a "common language". Communication breakdown can only be eliminated in this way, by having two commensurable languages communicating. If no latent inclusion experience can be found to use as a basis of reference, *no conversion* or persuasion will follow, only incommensurability.

6. What I have called "missing paradigms" are missing because we are not aware of their existence and because there are no alternative suggestions given to ease the crisis. The missing paradigms are not exceptional or specific, 19th century phenomena. For those who accept the Kuhnian model, the relationship between discontinuous incommensurable paradigms is characterized by a specific kind of asymmetry: the overthrown paradigm's memory is only annihilated in those types of paradigm-shifts (although the shifts are always radical and the consecutive paradigms incommensurable) where the victorious paradigm is atomist, inductivist, and sensualist, that is, private-based. This tradition is strongly characterized by an aspiration for methodological monopoly. In the other type of paradigm-shift, where non-privately based paradigms are victorious, the situation is different. As Hegel put it, scientific activity is not regarded here as a conversation of Objective Reason with Itself.[21] So it is possible that the presence of methodological plurality and the high number of rival theories, which Kuhn considers to be characteristic of an unripe, pre-opulent period preceding each paradigm occurs much more frequently than was thought, and this in itself is sufficient evidence of a missing paradigm.

Adding the conception of the "missing paradigm and its inclusions (in the next paradigm)" to Kuhn's paradigm theory, we receive a new model. In this model, *we may preserve all notions of science, even that of the development of science without posing* an impossible demand for a neutral, absolute rule of thumb. The criteria of development will necessarily be *relative*, but *relativity is not identical to an absence of measurement criteria*. Here I only repeat an argument which has been formulated during the past hundred years mostly by occupants of an inclusion position.[22] This acceptance of relativity is a *result of the insight* that our actual scale and criteria of measurement coordinating our activity *is not the one and only possible* rule of thumb. In the absence of knowledge about other scales, we maintain an unconscious relationship (that is, we cannot speak of any relationship at all) with our criteria, or frame of reference. What must be unconditional in the end is not the absoluteness of the scale, but the fact of the actual existence of such a scale.

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NOTES

1. The present discussion of the incommensurability theorem, the crisis it has caused and the debate it gave rise to owes a great deal to the work of Márta Fehér. Cf. M. Fehér, *A tudományfejlődés kérdőjelei. A tudományos elméletek inkommenzurábilitásának problémája* (Questions of Scientific Development. The Problem of the Incommensurability of Scientific Theories) Akadémiai Kiadó, Budapest, 1983.

2. Cf. M. Fehér, *op.cit.*, p. 60

3. Cf. M. Fehér, "Thomas Kuhn tudományfilozófiai paradigmája". (Thomas Kuhn's Paradigm in/of the Philosophy of Science) Postcript to the Hungarian edition of T.S. Kuhn, *The Structure of Scientific Revolutions* (A tudományos forradalmak szerkezete) Gondolat Könyvkiadó, Budapest, 1984, p. 317

4. T.S. Kuhn, *The Structure of Scientific Revolutions.* 2nd edn., enlarged. The University of Chicago Press, Chicago, 1970, p. 195

5. The term "paradigm" is here used with the intention of retaining Kuhn's *original* conception, in all the "not less than *twenty-one* different senses" identified by Mar-

garet Masterman. (M. Masterman, "The Nature of a Paradigm", in: I. Lakatos - A. Musgrave, (eds.) *Criticism and the Growth of Knowledge*. Cambridge, 1970, p. 61.)

6. T.S. Kuhn, *op. cit.*, p. 86

7. Cf. *Ibid.*, p. 52

8. *Ibid.*, p. 126

9. *Ibid.*, p. 102

10. P.K. Feyerabend, "Consolations for the Specialist", in: I. Lakatos - A. Musgrave (eds.), *op. cit.*, p. 227

11. An observation made by M. Fehér, *A tudományfejlődés kérdőjelei...*, p. 39

12. T.S. Kuhn, *op. cit.*, p. 86

13. By a "missing paradigm", I mean M. Masterman's "metaphysical paradigm" or "meta-paradigm". (*op. cit.*, p. 65).
In an unpublished doctoral thesis (V. Békés, *Tudományelméleti és nyelvfilozófiai elképzelések a magyar nyelvújítási mozgalomban 1818 és 1874 között*, 1982; - Conceptions of the Theory of Science and Linguistic Philosophy in the Hungarian Linguistic Reform Movement, 1818 to 1874), I tried to demonstrate the way this paradigm had controlled the scientific activity of the day, under the aegis of the Hungarian Scientific Society. There I identified the paradigm through its features as that of Romantic Liberalism. However, both liberalism and romanticism seem to arouse, at least to my judgement, misguided and misleading sentiments, for which reason I would now prefer to call this the Göttingen Paradigm. Perhaps this choice is warranted by the fact that, as it has emerged from various historical studies, the central place of the development of this paradigm, radically different from those of both the Enlightenment and Positivism, was at Georg-August Universität, Göttingen, at any rate a school more famous for its students than its teachers.

14. Webster's Ninth New Collegiate Dictionary, for example, details the meaning of "inclusion" as "something that is included", like "a passive product of cell activity (as a starch grain) within the protoplasm" or "a gaseous, liquid, or solid foreign body enclosed in a mass (as of a mineral)".

15. I owe thanks to Professor Zsigmond Telegdi for a thorough introduction to Humboldtian and Neo-Humboldtian linguistics.

16. T.S. Kuhn, *op. cit.*, p. 204

17. *Ibid.*, p. 269

18. T.S. Kuhn, "Reflections on My Critics", in: I. Lakatos - A. Musgrave (eds.), *op.cit.*, p. 277

19. M. Fehér, *op.cit.*, p. 52

20. T.S. Kuhn, *The Structure of Scientific Revolutions*. p.149

21. Cf. Hegel, *Lectures on the History of Philosophy*. II.I.3

22. For this problem, see. e.g. Karl Mannheim's explication: "Relationism, as we use it, states that every assertion can only be relationally formulated. It becomes relativism only when it is linked with the older static ideal of eternal, unperspectivic truth independent of the subjective experience of the observer, and when it is judged by this alien ideal of absolute truth." K. Mannheim, *Ideology and Utopia. An Introduction to the Sociology of Knowledge*. Routledge & Kegan Paul, London, 1954, p. 270. M. Fehér has shown that Mannheim's approach to epistemology has been generally *misunderstood* and that "... the concept of 'ideology' in Mannheim's sociology of knowledge as well as his interpretation of the ideological nature of knowledge was *so much at variance* with both the sensualist, Baconian epistemology of early positivist philosophy of science and the Cartesian epistemology of later philosophy of science and internalist history of science that there

were hardly any grounds for even a debate between them." (my italics V.B.) M. Fehér, *A tudományfejlődés-elméletek története* (A History of Theories of Scientific Development). A Filozófia Időszerű Kérdései, 38. Budapest, 1980, p. 61.

BALÁZS DAJKA

SOCIAL LIFE AND SOCIAL SEMANTICS

The term 'social life' had had a long history: it was used with more or less rigour by various trends of social thought and inquiry to stand for different concepts which appear to be of central interest to the philosophy of social science. These concepts were always closely connected to the assumptions underlying the particular theories of the different traditions of social research, therefore their analysis must also begin at a general level. Another methodological imperative is to approach the different concepts according to their distinctive contexts, for they often appeared in conflicting theories. 'Post-Kuhnian' social science methodology has made attempts at carrying out this seemingly impossible task, one of which, the programme of the semantic interpretation of social scientific problems will receive especial attention in this essay. The semantic approach has been based on the work of Donald Davidson and employed by G. MacDonald and P. Pettit (1981) to analyse a wide range of concepts which, outside the meta-level of analysis, remain unrelated as parts of diverse theoretical constructions. Some aspects of the use and applicability of, as well as certain limitations to, the semantic pursuit are also expected to be borne out by the present paper.

Social science, taken in some conveniently wide sense, has often been criticised for failing to fulfil one or another of its numerous tasks, or to live up to a wide range of possible, philosophical or methodological, standards. It has also been made responsible for the welfare of man in society in general, similarly to natural science which, on the other hand, is accused of the detrimental effects of its discoveries. Is this only related to the problems known as those of applied science, or does even this apparently extraneous aspect of role and value ascription concern 'pure', basic research?[1] Let us take as an example Karl Mannheim's condemnation of the psychology and sociology of his day for having lost sight of man's goals with science: "The mechanistic and functionalistic theory [...] is of assistance only as long as the goal or the value is given from another source and the 'means' alone are to be treated. The most important role of thought in life consists, however, in providing guidance for conduct when decisions must be made. Every real decision (such as one's evaluation of other persons or how society should be organized) implies a judgment concerning good and evil, concerning the meaning of life and mind.[...] Men strove to know the world so that they could mould it to conform to this ultimate goal; society was analysed so as to arrive at a form of social life more just or otherwise more pleasing to God; men were concerned with the soul in order to control the path to salvation. But the further men advanced in analysis, the more the goal disappeared from their field of vision, so that to-day a research worker might say with Nietzsche 'I have forgotten why I ever began' (*Ich habe meine Gründe vergessen*)." (K. Mannheim, 1954 (1936), pp. 17-18.)

If we decide, with Mannheim, that such a concern with goals should characterize both applied and pure science, we can still distinguish between at least three degrees of value-ladenness, which the semantic approach correlates with three degrees of value-realism. (Cf. G. MacDonald - P. Pettit, 1981, Ch. 4: Truth and Value) The latter approach also considers that "the social sciences are enmeshed in evaluative issues to the extent that their methods raise ethical problems about experimenting with human beings and to the extent that their results are used in the formation of social policies." However, within the semantic framework, the importance of these issues is minimized, for the authors say that "value commitments raise a problem when they are likely to lead different inquirers to different results. The commitment to truth and honesty is not liable to have this effect since it is assumed to be universal. And neither are commitments about the importance of certain topics, or about the propriety of certain methods or policies. Such commitments may vary as between people but they determine the inquiries made by researchers, not - or at least not necessarily - the results reached in those inquiries." (G. MacDonald - P. Pettit, 1981, p. 153)

This is remarkably in line with Mary Hesse's distinction. "Value judgments related to science may be broadly of two kinds. They may be evaluations of the *uses* to which scientific results are put, such as the value of cancer research, or the disvalue of the nuclear bomb. But they may also be evaluations that enter more intimately into theory-construction as *assertions* that it is desirable that the universe be of such and such a kind *and* that it is or is not broadly as it is desired to be." (M. Hesse, 1978, p. 2) According to this and the preceding statement quoted, one kind of value judgment can be excluded from the analysis even though it is not shown to be irrelevant to the scientific enterprise. This is precisely "the value ... given from another source", as Mannheim put it, and it is in virtue of its externality that it is often neglected by philosophers of science. The other kind of values which, as Hesse claims, "enter more intimately into theory-construction" seem to serve as grounds of value judgments which "issue in assertions rather than imperatives, and hence involve a transition from *ought* judgments to *is* judgments." (*Ibid.* , p. 2) Since logical inference is thus inappropriate here, she can elaborate her version of the pragmatic criterion of truth as a "modification of the traditional empirical criteria of confirmation and falsifiability." (*Ibid.*, p. 4) It must be noted, however, that the pragmatic criterion is not only applicable to the second but to the first kind of value judgments as well. To the extent that most imperatives may involve a transition to 'is' judgments, they allow of reformulation so that even the first kind of value judgments enter inextricably into theory-construction.

We have seen that, for Mannheim, the ascription of values was closely related to the ascription of meaning. For the semantic approach based on the description theory of reference, the meaning of an indicative sentence is closely related to its truth conditions.[2] We cannot consider here all the major traditions of social thought which have expressed themselves on the problem of that threefold relationship in social life, but we must at least mention the Marxian, the phenomenological and the 'verstehende' approach, which made extensive use of the organic metaphor of the 'life' process of society.

The choice of a term like 'social life' has important implications. In terms of the above value distinctions, there is a difference between one research project in which the events taking place in society are seen as constituting a 'life' process and another project which envisages those series of events to belong to a 'lifeless' process. It would be a crude typology to list theories in the individualistic tradition under the former, and collectivistic ones under the latter category. In many respects, the contrary classification would also be warranted. A recent interpreter of Marx's work has argued convincingly, in a dialectival vein, that there a synthesis of the two approaches can be discerned. She "treats Marx's analysis of capitalism as exemplifying a phenomenological mode of theorizing, one that is characterized by inquiry into grounds or presuppositions of our knowledge of social life. Marx's version of phenomenology differs from others in certain specifiable ways." (R.W. Bologh, 1979, pp. 1-2) Following Wittgenstein, the author uses "the term, 'form of life', to refer to the productive relation of subject to object, the incorporation of an object into the life of a subject. The term, 'form of life', avoids the narrow economic meaning that the term, 'production', tends to have. Dialectical phenomenology inquires into the form of life in which an object of knowledge is embedded, its active relation to a subject." (*Ibid.*, p. 2)

'Form of life', then, is closely related to the concept of production, very much like in Marx's famous summary of his basic ideas: "In the social production of their life, men enter into definite relations that are indispensable and independent of their will, relations of production which correspond to a definite stage of development of their material productive forces. ... The mode of production of material life conditions the social, political and intellectual life process in general." (K. Marx, 1969, Vol.1, p. 503) It is very interesting that, for Marx, the term 'social life process' does not designate as general a concept as for many later theorists. Beside the social and apart from the material, we also find the political and intellectual spheres. The main distinction is identified by G.A. Cohen as the one between the material and the social properties of society. "Marx is frequently concerned to distinguish sharply between what is and what is not an economic or social characteristic. [...] These rulings rest on a distinction between the content and the form of a society. People and productive forces comprise its *material content*, a content endowed by production relations with *social form*." (G.A. Cohen, 1978, pp. 88-89) Cohen traces back a similar distinction to Max Weber, where we come back to the problem of meaningfulness or intentionality.[3]

At this point, beside the concept of social life, we must see what other concepts the semantic approach to social science makes use of in analysing meaning and intention. It is to be understood that this approach is not primarily based on the hermeneutic tradition, and in certain respects it elaborates a moderate reductionism. It makes fine distinctions between social events under different descriptions, and analyses the causal powers of institutions and of individuals' attitudes in the frame of Davidson's description theory of reference. In the chapter on "The Anatomy of Social Life", though the authors give no precise definition of the concept, they handle the problem of different social regularities within a coherent framework. Their standpoint is "that while society consists of individual people, and nothing is a social event except insofar as it connects with people, there are also important institutional entities on the social scene: specifically, groups and practices. If society is composed of people, these individuals are organised in such a way that groups and practices are equally part of the social world." (G. MacDonald - P. Pettit, 1981, p. 113) However, the use of the terms, 'social life' and 'social world' more or less interchangeably reveals the authors' indifference towards the concepts designated by them. The concepts of 'life' and 'world' are particularly inappropriate in one important respect: that of the autonomy of institutions as 'entities'. These in fact can only be 'entities' in a vague sense within the framework of the semantic approach, for they are characterized by their persistence in spite of changes in their components. That is to say, the 'lower level' of component individuals does not essentially belong to the higher organization of institutions and, consequently, the 'life' of an institution does not depend on the 'lives' of its components except for extreme cases. 'Social life' then seems to be stratified in a way which threatens its homogeneity. The 'social world' is similarly decomposed into micro- and macroworlds, not only methodologically but also ontologically. Warns the philosopher of science criticising reification in sociology in general: "The very data that we employ, the 'described phenomena' that are so central to any sense of empirical reference, depend upon our interpretative theories. If those theories continue of reify either structure or action, then our data will continue to reflect that reification, and sociology will remain fragmented in the most fundamental sense." (A. Tudor, 1982, p. 182)

The semantic approach also differs from the 'verstehende' tradition in its way of dealing with causal explanations. Let us quote a very useful summary of the latter's characteristics at some length: "It is extremely difficult to determine whether and to what extent a writer uses a *verstehende* approach. One can perhaps distinguish three elements of such an approach, though often they will go together and reinforce one another.

1. A general hostility to positivism, stressing the distinctiveness of the human sciences or, more generally, of 'the social'. There will tend to be associated with this a particular epistemological and methodological position on the way in which one can obtain access to data in this realm, e.g. by empathy, re-experiencing (*Nacherleben*) or imaginative reconstruction.

2. A stress on the usefulness of teleological explanations of human behaviour, as opposed to explanations cast purely in terms of efficient causes. This can be expressed in terms of the Marxist notion of 'praxis' and, in Sartre's terminology, 'project'.

3. The concept of 'form' or 'structure'. This element is perhaps rather less central, though it is emphasised by L. Goldmann, who derived it from Lukács and, he claims, from Hegel and Marx as well." (W. Outhwaite, 1975, pp. 63-64)

We should even need a more detailed characterization than the above in order to bring out the contrast of the semantic approach. Interestingly enough, the latter also professes "to vindicate the Verstehen point of view" but, as it appears, rather defending it from the outside than practising it from the inside. This must be due, in accordance with *criterion 1* above, to the decline of neo-positivism which urges social philosophers, too, to develop a new paradigm on the basis of one of the old ones. If we grant the vindication of the Verstehen point of view by the semantic approach according to *criterion 1*, we have to deny it that appellation as regards *criterion 3*. Without demonstrating the lesser significance of the Hegelian and Marxian tradition to social semantics, it may suffice to note here that the latter takes a modified methodological individualist stance, admitting what it calls the expressive autonomy of institutions but rejecting the supposition of their explanatory autonomy.

Now, coming to *criterion 2* it emerges that social semantics does not wish to rely exclusively on causal patterns of explanation in dealing with actions, but is not attracted by teleological explanations, either. After a careful analysis of views on the non-causal character of action explanations it appears to stay with a kind of causal model which, however, is based on a different conception of law. The idea is "that intentional explanation is not nomothetic, and that in this respect it contrasts with the general run of explanations of natural events." The non-nomothetic model requires that the postulates be "only indubitable explanatory principles and in the exercise there can never be a possibility of revising the principles and recasting the explanations." (G. MacDonald - P. Pettit, 1981, p. 99)

The substitution of "explanatory principles" for revisable "laws" seems to be an after-effect of the once crucial principle of falsifiability. Social semantics is a brave attempt to save social science in the face of an old challenge. It seeks to assign explanatory status to hypotheses which fail to be falsifiable, by changing their relation to laws. This "middle-of-the-road" approach characterizes the proposals for other solutions within this framework too, among them the treatment of values, the conception of agents, the issue of individualism vs. collectivism, and various processes of social life. Like many other efforts to draw on a great variety of sources and to appropriate the best of their outputs, this brand of semantic approach to the subject matter of social science remains an elaborate excercise in methodology without the appeal, at its height, of any of the old paradigms revisited.

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NOTES

1. This is just one way of formulating the question, which otherwise does not depend on any distinction between pure and applied science.

2. In an article dealing with realism vs. anti-realism, we find the following understatement: "Philosophers have found attractions in the idea that a theory of meaning for a language might include a component capable of specifying, for any indicative sentence of the language, a condition under which it is true." (J. McDowell, 1978, p. 127)

3. Though Weber's distinction at first sight seems to be merely methodological, it is well known that, in this way, he demarcates the social from the non-social: "There are statistics of processes devoid of meaning such as death rates, phenomena of fatique, the production rates of machines, the amount of rainfall, in exactly the same sense as there are statistics of meaningful phenomena. But only when the phenomena are meaningful is it convenient to speak of sociological statistics." (M. Weber, 1947, p. 100)

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SOME REMARKS ON THE KRIPKE-PUTNAM THEORY OF REFERENCE

During the debates going on now for almost two decades on the meaning variance of scientific terms there emerged the need for a new theory of meaning. In the eyes of some philosophers of science, the Kripke-Putnam causal theory of reference seemed to offer an escape route from the serious problems related to the so-called incommensurability of scientific theories. (This incommensurability, as was mainly argued for by Feyerabend, is a consequence of meaning variance.)

My aim in this paper is to point out some of the inherent difficulties in this new theory of meaning, put forward independently but convergently by Kripke and Putnam. [Kripke 1972, Putnam 1973, 1975]. Their theory offered a new alternative to the traditional conceptualization of the reference-relation and indicated a way of identifying the reference of a term independently of its sense. It was later called the "causal" (or "historical") theory of reference. It is well known that the starting point for Kripke was a critical assessment of the Frege-Russell theory of descriptions and proper names. However, he regards his results as valid even in the case of natural kind (or physical magnitude) terms. The cutting edge of Putnam's and Kripke's criticism was directed against those assumptions of the Frege-Russell theory that (1) senses are cognitive, i.e. they exist in the minds of the referers, and (2) that senses give those descriptions which the referents must satisfy in order for them to be the referents of the terms. In Kripke's opinion, it is simply false that, in cases of successful reference, the referent satisfies all or (as the cluster concept theory holds) most of the descriptions given in the sense. The referent may not satisfy any of the descriptions and still be the referent of a name or of a natural kind word. These latter are namely rigid designators, which, by definition, refer in every possible world to the same object, independently of the descriptions this object actually satisfies. According to Kripke, sense is not the chief (or only) means by which we pick out or fix the referent of a term.

For species as for proper names - says Kripke -

> ... the way the reference of a term is fixed should not be regarded as a synonym for the term. In the case of proper names, the reference can be fixed in various ways. In an initial baptism it is typically fixed by an ostension or a description. Otherwise the reference is usually determined by a chain, passing the name from link to link. The same observation holds for such a general term as 'gold'. If we imagine a hypothetical (admittedly somewhat artificial) baptism of the substance, we must imagine it picked out as by some such 'definition' as 'Gold is the substance instantiated by the items over there, or at any rate, by almost all of them'. (...) I believe that in general, terms for natural kinds (e.g. animal, vegetable, and chemical kinds) get their reference fixed in this way; the substance is defined as the kind instantiated by (almost all of) a given sample. [Kripke, 1972, p. 322]

In Kripke's opinion the references, not only of the natural kind terms, but also of such theoretical terms as 'heat', are fixed by *ostension* or by some contingent *description* (which may not be satisfied by the referent) and then continuated by a *causal* (historical) *chain* linking the users of the term. The reference-fixing descriptions (or identities) - as, for example, that gold is a yellow, malleable metal - are *a priori*, but not *necessary*, since gold, for example, may turn out not to be yellow, but the term 'gold' may nevertheless refer to the same natural kind, gold. In this view, however, there are so-called *theoretical identities*, which

> are generally identities involving two rigid designators and therefore are examples of the necessary *a posteriori* [Kripke, 1972, p. 331].

Examples of such identifies are: 'heat is molecular motion' or 'gold is an element with atomic number 79'. These identities hold in every possible world, so nothing counts as heat or gold which is not molecular motion or has not atomic number 79.

Quite similar views were put forward by Hilary Putnam in his [1973, 1975], under the title of the 'causal theory of meaning', for natural kind and physical magnitude terms. According to his theory the fixing and continuating of the references of these terms proceeds through a twofold causal chain. The first kind of causal relation connects the referent of the theoretical term, as a cause, to an observable phenomenon, as its effect. The second kind of causal relationship subsists between the referers i.e. the later users of the term, and the introducing event, or relates to earlier uses of the term. Thus referers using the term - say - 'electricity' are

> connected by a certain kind of causal chain to a situation in which a *description* of electricity is given, and generally a causal description - that is, one which singles out electricity as *the* physical magnitude *responsible* for certain effects in a certain way. [Putnam, 1973, p.203.]

The first occasion for giving a causal description such as that above, one which may only be approximately true of the referent, is called the *introducing event*, and each of the later uses is causally connected with this event. Thus, every user of the term in question can successfully refer to the same intended referent, even if the initial causal description failed to describe the referent correctly (or is altogether wrong). As Putnam later [1975] added, the individual referer may succeed in referring even if he does not know the introducing causal description (or cannot give any description) of the referent, but is causally linked to other individuals who were in a position to pick out the referent correctly, and who are able to give the required description (for example, experts). So, in reference, there is a 'division of linguistic labor' because the use of natural kind and physical magnitude terms, as well as proper names, is *collective*.

The extension of natural kind (and physical magnitude) terms can be given, according to Putnam, [1970, 1973, 1975] in two stages: 1) the word must be associated with an archetype (or stereotype, which resembles the Kripkean 'original sample'); and 2) everything which belongs to the extension of the term must bear the relation 'same kind' to the archetype. He assumes that this relation will specify the shared microstructure of the kind. (He construes the relation itself as an equivalence relation throughout possible worlds.) So, for instance, the referent of 'water' (or the extension of 'is water') is given by pointing out a standardized sample, an archetype or specimen of the kind (in this world) and though not *all* the features characterizing the archetype must be shared by samples of the kind in other possible worlds, according to Putnam, there are some of them which must be shared so that the sample will belong to the extension of the term in question. As he writes:

> Once we have discovered that water (in the actual world) is H_2O, nothing counts as a possible world in which water is not H_2O. In particular, if a logically possible statement is one that holds in some logically possible world, it is not logically possible that water is not H_2O. [Putnam, 1975, p. 151.]

Notice how strongly Putnam states his case (for the so-called strong stereotypes, such as that of 'water'): he does not merely say that 'nothing counts as water in some possible world, which is not H_2O', but that 'nothing counts as a possible world in which water is not H_2O' and that 'it is not logically possible that water is not H_2O'. This means that he regards such theoretical identities as 'water is H_2O', not only as metaphysically or merely epistemologically necessary, as Kripke does, but also as *logically* necessary. That is to say, he takes some (fundamental) assumptions of our present scientific theories to be logical truths, i.e. valid in every possible world. Or in other words, they are, for Putnam, the selective criteria for 'possible worlds' regarding their possibility.

To sum up: according to Putnam, linguistic competence is not just knowledge, as was held by Frege and Carnap, but it

> is a matter of knowledge plus causal connection to introducing events (and ultimately to members of the natural kind itself). [Putnam, 1973, p.209.]

The Kripke-Putnam causal theory of reference has been extensively criticised mainly for its essentialism [e.g. by Zemach, 1976, Mellor, 1977, Kleiner, 1977, Papineau, 1979] as well as modified or corrected [e.g. by Nola, 1980]. I do not intend, however, to recapitulate this criticism here, not even the main objections brought up so far. Instead I would like to put forward my own assessment of the Frege-Carnap versus Kripke-Putnam controversy and give my own account of the efficacy of the causal theory of reference.

1. *First*, I think, it is beyond doubt that the Kripke-Putnam theory has the great merit that it opens up a semantical-epistemological way to a referentially stable world, which differs rather sharply from the referentially fluid world of the Frege-Carnap theory of meaning. In this latter theory the sense or intension of the term serves as a selector function for referents, thus to use a technical term introduced by E. Fales, [1976] the reference of a term is 'floating': it is not anchored to one and the same object but always picks out that which satisfies the sense. That is why this theory of meaning is haunted by the problem of the so-called 'transworld identity', that is, the problem of re-identifying one and the same entity in different possible worlds, in which it can be referred to by the same term but with different, changed sense, i.e. satisfying different descriptions given in the sense. On accepting this latter conception of meaning, the central part of Feyerabend's incommensurability thesis follows, according to which the different theories talk about different worlds, since we have no means outside and above the theories

in question to determine the referents of their terms. If lingustic competence is a matter of knowledge, and it is the theory which gives this knowledge, then each theory has its own ontology or, in Feyerabend's words, its own cosmology. This is closed to other cosmologies, and there is no means by which entities belonging to such different worlds can be reidentified.

Now a great advantage of the Kripke-Putnam theory of meaning is that it can account for the re-identification of referents independently of sense, that is, it works even in cases of extensive descriptive changes. So it can explain referential stability even in spite of radical changes in senses or severe revision in intensions. I think, however, that the biggest difficulty with regard to the theory lies here: its world is too stable, and entities once introduced by initial baptisms or introducing events remain for ever in existence as referents. In one word, this theory cannot account for referential change, shifts in reference, reference to non-existing entities nor reference to entities whose existence is dubious at the very moment of, and for some time after, their introduction (as was the case with quarks or positrons and, formerly, with impetus). It cannot account for either reference to entities which are supposed to be non-existent at the moment of introduction (such was the point-charge electron), or for reference to entities disclosed as non-existing during the development of knowledge (such as phlogiston, or unicorns). There were, and are, theories in the history of science which explicitly postulate entities which are not taken to exist (e.g. idealizations) and, in addition, there were, and are, theories which suppose entities or natural kinds to exist which are later discovered to be not *really* existing.

Thus, the Kripke-Putnam theory of reference accounts for only those cases of reference where the intended referent exists (or, at least, is supposed to exist), and where itself or its effect is experimentally accessible, that is, where we are somehow acquainted with it. It cannot account, however, for reference to entities postulated for theoretical, explanatory purposes and whose existence is at least dubious.

Moreover, as Mellor [1977] has pointed out,

> 'Neutrino' is for Kripke a non-Fregean rigid designator since its referent in other possible worlds is not constrained to satisfy these theoretical descriptions which must be supposed to provide its Fregean sense. For this work, of course, there must actually *be* neutrinos near enough as specified. They need not be in the observable past to serve as archetypes, but they do need to be somewhere in this world (past, present or future). Otherwise the requisite uniqueness of reference would not be secured. Many different kinds of particle will satisfy our theoretical descriptions of neutrinos in various possible worlds, and nothing but the reality of one of these will single it out as the unique referent of 'neutrino'. Were there in fact no neutrinos, the term could for Kripke no more designate a natural kind than 'unicorn' can. [Mellor, 1977, p. 307.]

Thus my first objection against the Kripke-Putnam theory of reference is that it cannot account for referential change. It only provides a referential mechanism by which cases of referential stability are rendered understandable and theoretically, as well as pragmatically, possible.

So, it may be said that the causal theory of reference surpasses the Frege-Russell-Carnap theory in that it takes into account the important difference [first pointed out by Donnellan, 1966] between "failing to refer" and "falsely referring", while nevertheless referring to something existent. The causal theory does not, however, allow for systematic considerations regarding cases of *genuine* reference failure or reference to non-existents. Kripke and Putnam assume, I think, an *a priori* difference between terms (now known as) having really existent referents and those whose referent is not-existing, or whose extension is empty. They construe their theory of reference so as to account for the stability of reference of only the former kind of terms. The tacit presuppoition of the Kripke-Putnam theory is that reference can be made to what really exists, and, thus, for them, the ontological stability of the referent (and, finally, of the world) seems to involve the stability of reference, in spite of radical changes in our knowledge regarding the referent. As we shall see below, Putnam incorporates into the meaning of a term the fact that its referent exists, taking it as given in the linguistic character of the term:

> It may seem counterintuitive that a natural kind word such as 'horse' is sharply distinguished from a term for a fictitious or non-existent natural kind such as 'unicorn', and that a physical magnitude term such as 'electricity' is sharply distinguished from a term for a fictitious or non-existent physical magnitude or substance such as 'phlogiston'. [Putnam, 1973, p. 210.]

As to the background of this sharp distinction, neither Putnam nor Kripke is very explicit. But let us consider Kripke's own words:

> It is a common claim in contemporary philosophy that there are certain predicates which, though they are in fact empty - have null extension - have it as a matter of contingent fact and not as a matter of any sort of necessity. Well *that* I do not dispute; but an example which is usually given is the example of *unicorn*. So it is said that though we have all found out that there are no unicorns, of course there *might* have been unicorns. Under certain circumstances there *would* have been unicorns. And this is an example of something I think is not the case. Perhaps according to me the truth should not be put in terms of saying that it is necessary that there be no unicorns, but just that we can't say under what circumstances there would have been unicorns. Further, I think, that even if archeologists or geologists were to discover tomorrow some fossils conclusively showing the existence of animals in the past, satisfying everything we know about the unicorns from the myth of the unicorn, that would not show that there were unicorns. [Kripke, 1972, pp. 253-54.]

Putnam is, however, more permissive in the unicorn-affair:

> Indeed, I myself believe that if unicorns were found to exist and people began to discover facts about them, give non-obvious definite descriptions or approximately correct descriptions of the class of unicorns, etc. then the *linguistic character* of the word unicorn would change; and similarly with 'phlogiston'; but this is certain to be controversial. [Putnam, ibid., italics added.]

All this, however, is concerned only with how we can (or cannot) discover that a term thought to refer to something non-existent is, in fact, referring to something really existing. These authors do not deal with the question of how we can discover if a term does not refer, or better, refers to something non-existent. (All the counterfactual situations considered by Kripke and Putnam concern those cases in which a term retains its referent, although its characteristic features have changed; think of Putnam's Twin-Earth, or Kripke's example of gold turning out to be blue instead of yellow.) Thus, the causal theory of reference has been very effective in pointing out means by which we can stick to the referent of a term, in spite of extensive revisions in its sense, that is, in the descriptions which the referent is believed to satisfy. This theory, however, gives no criteria for the *limits* to this adherence to one and the same referent, nor has it pointed out under what conditions we should give up our assumptions regarding the *existence* of the referent. I believe, therefore, that the scope or generality of this theory of reference should be regarded as more restricted than was thought by its authors. It must not be regarded as a general theory of reference for theoretical (among them mainly natural kind and physical magnitude as well as substance) terms, but only as a theory of reference for those scientific (and everyday) terms which proved to be genuinely referring to *existents* over the course of extensive historical changes in their senses.

2. My second objection is thus that the immunity this causal theory seems to enjoy against even radical ontological revisions rests on a fallacy: namely, it mistakes the stability and identity of reference for that of the stability and identity of the referent as an existent in the real world. I mean that once a term is regarded as having a referent, it seems to retain it, no matter what descriptions that referent fits in different possible worlds. Thus, the *ontological sets* of the different scientific theories are always the same, only the descriptions given, or the beliefs held about the entities belonging to these sets, differ. New entities may be *added* to

this original, common set, but none of them may be *deleted.* At this point, the reader may perhaps charge me with a crude misinterpretation of Kripke's or Putnam's views. In fact, Kripke explicitly states that,

> When I say that a designator is rigid and designates the same thing in all possible worlds, I mean that, as used in *our* language, it stands for that thing, when *we* talk about counterfactual situations. (...) I ... do not mean to imply that the thing exists in all possible worlds, just that the name refers rigidly to that thing. [Kripke, 1972, pp. 289-90.]

Now, it seems clear that Kripke does not commit himself regarding the existence of the referents of rigid designators in all possible worlds. It is one thing, however, to declare one's non-committance and quite another to designate under what conditions we should regard the supposed referent to be non-existent, i.e. under what circumstances we should delete an entity from our ontological set. In the Frege-Russell theory of meaning the criterion for taking the referent of a term as existent was that it should satisfy all or most of the properties given in the sense of the term in question. If there is no such thing, then the referent does not exist. And thus, Kripke writes,

> A supposed statement about the existence of an object really is, so it is argued, a statement about whether a certain description or property is satisfied. As I have already said, I disagree. Anyway, I can't really go into the problems of existence here. [Kripke, 1972, p. 311.]

And I think, herein lies the weakest point in the causal theory of reference: it has no theory of existence, and, thus, it gives no criteria for abandoning beliefs on the existence and ontological status of a referent. And, furthermore, if the referent has *none* of the properties given in the sense by which it may be identified [see Kripke, 1972, p. 318], and if reference is continuated by a causal chain which only connects with former uses of the term and not with the referent itself, then nothing prevents us from believing in the existence of the referent *and infinitum.* Thus, according to the causal theory, causal chains must perpetuate *existential beliefs* as well as ways of identifying the referents independently of senses.

If we admit that the meaning and use of terms change, then one can either say that this change entails a new ontology, or claim that one is using new or the same terms with changed senses to redescribe the *same* entities, i.e. to presuppose the same ontology. The first position follows from the Frege-Carnap theory of meaning and was embraced by Feyerabend as the corner-stone of incommensurability. The second standpoint is that of Kripke and Putnam, whose theory of reference thus entails a complete ontological invariance between successive scientific theories. And this is what I find objectionable, for it seems to me that an adequate realist theory of reference must account for ontological *stability* as well as *changes* in ontological presuppositions. So, this theory ought, at least, include additional assumptions regarding the *disruption* of these causal chains under certain epistemological circumstances, so that it can account for real, historical changes in the ontological sets presupposed by different scientific theories.

The causal theory of reference cannot account for the most important scientific operations, among them the introduction of referring terms for hypothetical theoretical entities (such as positron or phlogiston) as candidates for existence (or, eventually, for non-existence); historical changes in the ontological status of a referent (for example, the fact that heat was once considered as a substance and, later, as a state of motion of particles constituting substances); or, again, the deletion of entities from our ontological sets.

The Kripke-Putnam ontology is insensitive towards changes in the theory, or, to use Quine's term, in the ideology.

You may perhaps ask, at this point, in what sense of the word do I speak about the 'sameness' of the ontological sets for different theories if, according to the Kripke-Putnam theory of reference, the descriptions which the referents fit may change. But this question rests on a Carnapian assumption of reference relation. According to this latter assumption by choosing a language, and, thus, a conceptual framework, a system of terms with given intensions or senses, we have already chosen an ontology (and so our ontologies vary with the senses or intensions of our terms). Thus, I would answer the above question somewhat loosely, speaking about the 'same ontological sets for different theories' in the same sense as Kripke and Putnam speak about the referents of rigid designators being the same in different possible worlds. And I would add that, therefore, in the Kripke-Putnam theory of meaning there seems to prevail a *principle of conservation of existence,* so to say, implied by the principle of conservation of reference through causal chains. For in this theory, 'to refer' means 'to have an existent referent'.

Thus the price paid for evading meaning relativism, and the disconnectedness of the ontologies of historically different scientific theories, is that reference becomes a static, unchangeable relation.
In order to avoid this danger, it was, I think, necessary for Kripke and Putnam to embrace a certain kind of essentialism regarding fundamental scientific terms, such as natural kind and physical magnitude or substance terms. But this much-disputed standpoint [see Mellor, 1977; Zemach, 1976] is beset by a historiographical provincialism or parochialism.

3. In this way - and this leads us to our third objection - they merely arrive at an especially rigid form of cluster concept theory which was otherwise so vehemently criticised by Kripke. By making theoretical identities such as "heat is molecular motion" metaphysically necessary (as Kripke does), or by postulating (as Putnam does) that "nothing is a possible world in which water is not H_2O", they are making unwarranted ontological assumptions and are committed to an ontology which is as susceptible to revision due to new scientific developments as any ontology which rests on the as-

sumptions of the Frege-Carnap theory of meaning.

In my opinion, there are no absolute and unchangeable essences which are given once and for all through scientific discoveries and which serve in the identification of samples of a natural kind in this and other possible worlds. However, I do not deny that, in different theories or in different stages of the same theory, we always think that certain (clusters of) properties of kinds or substances are more fundamental than others and, furthermore, that the former (called the real essence) are responsible for the manifestation of the latter (called the nominal essence).

It is true that in the Aristotelian theory, as well as in more modern scientific theories, there have always been assumptions regarding the essential underlying structures of things and kinds which are more important in fixing something as the referent of a term than those properties of them which, although manifest, may change without the object ceasing to be identifiable as such. But, as historical examples testify, nothing has ever prevented scientists from altering their ideas either about nominal essences (the manifest properties) given a priori and constituting the Kripkean meaning of the term or about real essences (the underlying structure) given a posteriori and constituting the Kripkean way of fixing the reference. [Cf. Papineau, 1979, p. 164.] Both of these alternatives are equally viable and justifiable, although they should not be used at the same time, or the referential continuity of the term will be broken. [As an historical example of these alternative processes, see the "True History of 'Copper'" given by Harré and Madden, 1975, pt. I.ch.VI., pp. 21-25.]

An advocate of the causal theory might, perhaps, argue against this, maintaining that what scientists *take* to be the real essence of a stuff does not matter as much as what its real essence in fact *is*. And this latter is unchangeable.

In Kripke's and Putnam's view, the boundaries between natural kinds are ontologically given and the statements of discoveries regarding their real essences (such as "gold has the atomic number 79") are necessary truths in the strictest possible sense [C.f. Kripke, 1972, p. 320], expressing natural, ontological necessities. We might discover that gold - say - has not the atomic number 79, but once we have discovered that it *has* that number, nothing counts as gold that has not that specific number characteristic of the atomic structure or real essence of gold. At this point, you might argue that these assumptions regarding the essences of kinds serve Kripke and Putnam as the necessary criteria of existence which I have claimed are missing from this theory of reference. But I do not think this is the case, since essences here serve only as a means of deciding whether a given sample or a particular thing does or does not belong to a given kind or species, and not whether the kind or species *itself* exists or not. Thus, in this way, we are able to assess if a given sample is not, for instance, water, if it is not H_2O, but we cannot establish that *water* is inexistent as a *kind* , or that water is not H_2O. This is because, as Putnam says (in the passage quoted above): nothing counts as a possible world in which water is not H_2O.

4. Stemming from this, I come to my fourth and, I think, most fundamental objection against the causal (as well as against the resemblance) theory of reference. While Kripke is right in stating that

> Frege should be criticized for using the term 'sense' in two senses. For he takes the sense of a designator to be its meaning: and he also takes it the way its reference is determined. Identifying the two, he supposes that both are given by definite descriptions. [Kripke, 1972, p.277.]

Kripke himself should also be criticized for not distinguishing between two senses of 'refer', that is, between (as I will call them) the *identificatory* and the *existential* component of the reference relation. He does not separate 'the way the referent of a rigid designator is fixed' from the existential commitment (on the part of the referer towards the existence of the referent) which, I admit, accompanies it, but which may be abandoned without thereby breaking the referential chain, that is, without leading to a failure to refer to the same thing as former users of the term.

Thus, I propose that we distinguish between reference to '*x*' and reference to '*x* as existent', following the pattern of the usual distinction between 'thinking of *x*' and 'thinking of *x* as such-and-such'. The idea of equating the two comes with Hume. I agree with Williams, according to whom

> Hume's claim that 'the idea of existence is nothing different from the idea of any object' must be rejected. [Williams, 1981, p.21.]

Kripke and Putnam construe their theory of reference so that, as I have argued above, it works for only real and enduring existents, which are so stable that they retain their essences even in other possible worlds. But the ways in which references are fixed - by initial baptisms, ostension or original samples - and continuated - by historical or causal chains - will not work when applied to hypothetical entities, that is, to those entities which, at the moment the terms referring to them are introduced, are supposed to be possibly, or sometimes surely, non-existent. (To support this point, one should recall the case of 'unicorn' as treated by Kripke and Putnam.) Thus, in this theory, we cannot refer to things whose existence is dubious or which are known to be non-existent. In my opinion, this is so because the Kripke-Putnam theory of reference is construed on the pattern of the theory of meaning for *proper names*. (Kripke is quite explicit on this point. He tells us that "terms for natural kinds are much closer to proper names than is ordinarily supposed". [1972, p. 322.])

5. The Kripke-Putnam theory surpasses that of Frege, Russell and Carnap in its sharp distinction between two components of the meaning of a term: on the one hand, its sense and, on the other the way its reference is determined. However, the authors of the causal theory failed to distinguish a third component or, rather, failed to distinguish between the two

parts of the second component, that is, between the pragmatic way the reference is fixed, or reidentified, and the *existential commitment* (of the referer) to the existence of the referent. Thus, I suggest that the meaning of scientific (mainly the so-called theoretical) terms should be composed of *three components:*

(1) the *descriptive* component (which corresponds to the Kripkean "meaning" or to the Fregean "sense" of the term), which is the cognitive part of the meaning;
(2) the *identificatory* (reidentificatory) or referential component, (which corresponds to Strawson's "identifying reference" or rather to "the way the referent is determined" according to Kripke and Putnam, and which may be given by causal or historical chains after its introduction by ostension, description or causal assumption). This is the pragmatic component of meaning, which is essentially of a methodological character; and
(3) the *existential* component, i.e. the existential commitment or assumption regarding the real existence of the referent.

Each of the three components may be changed or modified independently of the others, though it is usual to modify only one at a time and keep the other two fixed. There are many historical examples testifying to the fact that the process of the meaning variance of scientific terms consists of stages during which one or other of the above components varies rather radically or abruptly, while continuity in meaning is ensured by keeping the other two components unaltered for a time. It is, however, the third component which plays a crucial role regarding the use of the term, for when the existential commitment is abandoned, the term is not generally used to make scientific statements any longer and drops out of the standard scientific vocabulary (the term in question does, however, retain the descriptive, referential part of its meaning). Actually,

> there are cases of theories which continue to be regarded as explanatory, without an apparent continuing imputation

of truth to the explanans. [Leplin, 1981, p.291, n.25.]

Notice that if the third component is modified - let us say, for example, that the referent of the term is regarded as non-existent - then I would not say that the term 'does not refer' or 'fails to refer' (not even that it 'refers to nothing'). Since, in my conception, reference does not entail existence, non-existence does not entail failure or lack of reference. It is only a tacit and usually unwarranted (although warrantable) assumption on our part that, when we use a term referringly, we usually suppose that its referent exists (besides being given in the descriptive part of the term and determinable or to be picked out by the identificatory, referential part of the term). I would like to emphasize, however, that, in my view, a term does not cease to refer when we abandon our belief regarding the real existence of its referent. Furthermore, a term may refer even without any ontological commitment on its user's part towards the existence of the referent of the term.

As Leplin writes:

> One can refer to something without believing that it exists, even believing that it does not exist. [Leplin, 1979, p. 278.]

My conclusion is thus that for scientific terms it would be better not to include the existential component into the (linguistic) meaning of the term, as Kripke and Putnam did.

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MEASUREMENT DATA WHICH PLAYED A TRICK ON THEORY*

(The Role of Vapour Density Measurements in the Development of Atomic Theory in the 19th Century)

It is well known, after Knight's work on the topic that the history of chemical atomic theory of the 19th century divides into three phases.[1] The early impetus of atom theoretical thinking was followed by an ever growing mistrust and in the eighten-thirties a crisis emerged originating from the factual problems of research. This crisis was reinforced by a mostly empiristic belief about the essence of scientific work and by conflicting beliefs on the structure of matter. The anomalies which emerged in Dumas's measurements of vapour density played a significant role in the formation of the crisis. This aspect will be closer examined here.

There are measurements where the measurement technique brings with itself great difficulties. In other cases, however, the measurement can easily be executed but the interpretation of measurement results causes problems. In this historical case, the measurement could easily be executed by a technical master stroke but the interpretation of measurement data became ever more problematic.

The measurement of vapour density became, from 1820 onward, an important means for the determination of atomic and molecular weight. Here it will only be dealt with in connection with the determination of atomic weight.

We briefly recall here the development of chemical atomic theory at the beginning of the 19th century. In the years after the turn of the century, the law of constant weight-

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proportions was formulated and accepted. To that time Dalton's atomic hypothesis had become known, in which the law of multiple proportions was also formulated. The atomic hypothesis was of heuristic value, yielded a possible explanation for chemical phenomena, but it was not of a strictly predicative nature.[2] (As to the theoretical foundations given by the atomic theory in relation to the laws of weight-proportions, the teaching was, in a certain sense, also capable of prediction; the separate experiences in it concerning weight (volume) relations gained an overall validity.)

From the point of view of experimental research, the greatest defect of Dalton's teaching was its being based on arbitrary atomic-weight values. It must be remembered here that the simultaneously arising problems of the determination of atomic weights and the determination of the quantitative composition of the particles of the compounds together made a Diophantes-problem. In order to solve this, Dalton introduced - at least for temporary use - the arbitrary, so-called principle of simplicity. (According to this supposition, he took, for example, the composition of water to be HO and, consequently, the atomic weight of oxygen to be 5.5 (~8).

In 1808, Gay-Lussac discovered the laws of volume for compounding gases. Gay-Lussac supposed that the volume relations correspond to the particle relations. This supposition yielded the possibility of ruling out the principle of simplicity. On the ground of Avogadro's principle (1811 Avogadro, 1814 Ampère) a mutually unambiguous connection could be established between the measured vapour density values and the molecular weight, as well as an unambiguous connection between the measured density values and the atmoic weight values. Avogadro also realised that, with the supposition of the validity of the Avogadro principle, the composition of compounds can be rightly deduced from the law of volumes exactly in case when the molecules of elements can be supposed to be two-atomic or, as it could not be excluded as a purely theoretical possibility, sometimes even more-atomic[3]. The latter supposition will be referred to below as the *Avogadro hypothesis*, in contradistinction to the Avogadro principle.

But if this was so, then the results of Gay-Lussac and Avogadro yielded, therefore, no mechanically applicable rules for the determination of the atomic weight. Nevertheless, in principle they made it possible to replace the obviously arbitrary principle of simplicity with an experimentally justifiable supposition. The problem of interpretation (and not that of measurement) lies in the fact that the supposition concerning the possibility of different numbers of atoms contained in the more complex particles of different elements being thought of by Avogadro was yet to be set up and checked *in concreto*, in the case of each element. But, instead, the chemists first continued to work by simplifying the problem and used, if any, an inductive generalisation. Some of them, as Berzelius, worked with a "volume-atome" hypothesis, that is, they thought that at least the last particles of the simple gases should be one-atomic.

It was Berzelius who, with his experimental work done till about 1815, set up a comprehensive atomic weight table. With his measurements determining the atomic weights he did his best in view of the technical possibilities of his time. Beside the results of gas density measurements and the chemical analogies he also used for determination, from 1819 onward, the observations of Dulong and Petit concerning atomic warmth as well as the newly discovered phenomenon of isomorphy. But while the technical realization was unproblematic, the theoretical position of Berzelius and also his relation to Avogadro's hypothesis and to the law of Gay-Lussac was all the more problematic.

In 1813, Wollaston first expressed the view that chemistry does not need the knowledge of atomic weights at all, for the chemist can be satisfied with the purely empirical equivalent weights of combining materials. As a matter of fact, Wollaston formulated the point of view of an analytical chemist and also revealed his inclination for an empiristic, positivistic ideal of science.

So much for earlier history. In 1826, Dumas began his vapour density measurements. The goal of his measurements was not to gain empirical information about the vapour condition of a given material; he performed them to attain definite theoretical goals. La Chatelier described the situation in a short study in the following way: Dumas measured vapour density in order to determine the correct atomic weights.[4] We have yet to show thas this opinion should be formulated in a more differentiated way.

The method of vapour density measurement itself did not come from Dumas. Already in 1811, Gay-Lussac described an apparatus for this purpose and carried out measurements with it.[5] In these experiments he measured the volume of vapour of a given quantity of material taken at a definite temperature. Later, Despretz made vapour density measurements in order to study the behaviour of vapours, and not in order to determine the atomic or molecular weights.[6] Dumas notes somewhere that Dulong built an apparatus for the determination of the density of sulphur vapours and he was showed this apparatus by Despretz.[7]

It is interesting to mention that in his chemistry textbook Berzelius put forward the position that the methods of vapour density measurement bring along basic problems of a *technical nature* and therefore the realization of measurements needs great skill. Berzelius did not take vapour density measurement seriously as a possibility for the determination of molecular weight.[8] Despite this, it seemed quite obvious to consider, beside gas density measurements, vapour density measurements too in order to determine directly those atomic and molecular weights that had been determined up to then only indirectly, through analogies.

Dumas broke with the tradition of measuring the volume taken of a definite quantity of material and, just to the contrary, he took as the subject of his studies the weight of a quantity of material found in a given volume. Thereby the problems that emerged in the measurement technique after Gay-Lussac simply dissolved. Dumas extended his experiments over to high temperatures. He only had technical difficulties with keeping the temperature identical and with temperature measurement. The water, sulphur and fusion baths used by Dumas made it possible for him to make measurements in the whole interval, between room temperature and red heat. He used mercury and air thermometres for his measurements. The procedure applied by Dumas is a good example for the great significance of technical tricks in the development of experimental technique, in the spread of a given method. It has already been mentioned that the probably most skilful experimenter of the time, Berzelius saw little fantasy in the determination of vapour density. Dumas knew that he had succeeded in reaching, through the technical procedures introduced by him, a simplification that guaranteed the spread of the procedure in the laboratories. The problem to be clarified now was the following: what was the intention and what could this procedure be used for in the first half of the 19th century?

It must be remembered once again that the way from the data of vapour density to those of correct atomic weights led through the simultaneous acceptance of the Avogadro principle *and* what we called the Avogadro hypothesis. The main factor of the confusion arising in connection with the atomic weight was that the real significance of Avogadro's hypothesis about the existence of more-atomic element molecules was, nearly without exception, judged incorrectly by the chemists of that time. It was not clear for them that Avogadro's hypothesis is a necessary consequence if both the law of volume and Avogadro's principle are accepted. As has already been mentioned, some chemists accepted the view that the element molecules known up to that time should be seen two-atomic and handled this statement as an inductive working hypothesis. Berzelius, who did not accept Avogadro's hypothesis, saw the Avogadro principle as something valid only for some simple gases and worked with his own "volume-atom" theory.

Dumas began his vapour density measurements as a part of an overall program. In 1826 he evaluated the chemical atomic theory as a wonderful conception the significance of which grew with each day. The goal of his program was "to substitute positive concepts for the arbitrary data on which almost the whole of the atomic theory was based"[9]. Dumas thought correctly that "the [earlier] efforts to define the absolute atomic weights have led to uncertain results"[10], and "this uncertainty originated from the fact that ... the different methods used led only sometimes to the same result and the results of these methods were not comparable in most cases". Therefore he decided "to conduct a series of experiments in order to determine the atomic (and molecular) weights of a lot of bodies through the measurement of their vapour or gas density" ... "For this purpose it must be supposed that in the elastic fluids, under the same condition there is ... the same number of molecules" ... "The molecules of the simple gases must be considered susceptible to a final division which takes place at the moment of the combination". He thought he was forced to declare that "this final division takes place at the moment of the combination and it varies with the nature of the compund that comes into being".[10] He did not hesitate to conclude: "Considering the question under this aspect we can see that the determination of the real atoms through density measurements (of gases and vapours) presents insoluble difficulties in the present state of science. It is true that when the molecules (atoms) of simple materials in gas condition are associated to some extent yet then these materials can be well compared with each other under conditions under which they contain the same number of groups (molecules). But obviously it is impossible for us to know how many elementary molecules (atoms) are in any of these groups".[10]

The statements quoted from Dumas show that he was quite aware of the contradictory character of the situation: without correct atomic weights the chemical atomic theory remained arbitrary. But he did not see a way to follow on the basis of which the number of atoms contained in the element molecules could have been deduced. He rightly saw too, although he unfortunately expressed his ideas in a bad terminology, that Berzelius' atomic weight system was based on contradictory principles; that is, the formulas of the compounds did not refer to the volume-unity of the vapour or of the gas of the material.[11] Consequently, the atomic weights of Berzelius were highly questionable. (The theoretical significance of his work, seen from the point of view of our day, seems to be that he tried to put the determination of atomic weight on a solid empirical foundation.)

Let us examine the concrete interpretion problem which disturbed Dumas. For the determination of the atomic weight of sulphur, phosphorus, arsenic and mercury it had to be supposed that in vapour condition sulphur forms six-atomic molecules, mercury forms one-atomic molecules, while phosphorus and arsenic remain four-atomic. As we have seen, Dumas accepted Avogadro's *hypothesis* and these elements showed themselves as the first examples of the theoretical possibility predicted by Avogadro. But the hypothesis of a six-atomic sulphur molecule, and so on, seemed to be a clear contradiction because, according to the chemical analogy, the vapours of these elements should have contained two-atomic molecules. The molecular weights measured by the determination of vapour density and by chemical analogy were only consistent with each other when a vapour condition was supposed for these elements that was different from that of their chemical analogues.(These contradictions did not appear with Berzelius because he counted the atomic weight of the above mentioned elements on the basis of chemical analogies only.)

Between 1826 and 1832, Dumas conducted a series of analyses in order to solve the problem and also examined the elements showing abnormal vapour density again.[12] These measurements already took place in an intellectual setting where the word atom was used with an ever more confused meaning and the atomic hypothesis meant something already superseded in a certain sense. Moreover, it was not thought to be necessary, with the exception of some chemists, to exactly distinguish from each other the concepts of atom, equivalent, etc.[13]

In 1833, Mitscherlich performed vapour density measurements with a great number of elements[14]. It is worth noting how he introduced his measurements: he wanted to gain overall knowledge of those relationships that obtain between the volumina taken up by the compounds and by the elements (sic!).

In his study he evaluates the situation as follows: it seemed probable that in the same volumina the same number of atoms can be found. However, this supposition only proved to be correct for the simple gases and not for the compounds (sic!).[15]

The passage quoted reveals an intermingling of the concept of atom and that of molecule. Dumas interpreted his results about the vapour density of sulphur by proving that in the case of the simple gases the number of the atoms need not be the same just like it need not be so in the case of the gases of the compounds.

Mitscherlich took oxygen and water to be one-atomic. Phosphorus and arsenic are, in his conception, two-atomic, sulphur is three-atomic and mercury is half-atomic. Thus one can see that he mixed the concept of atom with that of molecule.

In 1831, Gaudin pointed out that a non-contradictory atomic weight table can be set up through the consistent application of the principle and the hypothesis of Avogadro - the latter also demanding a clear distinction between the concept of atom and that of molecule. Therefore the elements showing abnormal behaviour, as it was proved by Dumas, must be taken as consisting of one-, six-, and four-atomic molecules. At the same time, this would mean that vapour density measurement is a satisfactory means for the determination of atomic weight. It is well known that Gaudin's ideas elicited no response already at that time.[16]

The introduction of vapour density determination for the determination of atomic weight first seemed to be a task of simple collection of empirical information. The first problem came from the recognition of uncertainty concerning the number of atoms in the molecules of a given material. To solve this problem, one should have used probabilistic inferences supported by an even fuller system of analogies.[17]

For this reason, the ultimately valid confirmation of atomic weights was hoped to be the consistency of the results obtained by the most different methods. We may disregard the problems emerging with the determination of atomic warmth and also the problems of the method relying on isomorphy, but it must still be remembered that vapour density measurement, applied first as a simple way of gathering empirical information became ever more contradictory when used for atomic weight determination. Together with other problems of the atomic hypothesis, it seemed quite important that the recognition of the existence of anomalous vapours limited the validity of well-founded chemical analogies: for example that of oxygen and sulphur. Thereby the supposition of the very existence of more-atomic element molecules, otherwise also problematic, was to be completed by additional, individual suppositions always set up as ad hoc working hypotheses in order to save the original one. And even that could not be done without producing new contradictions.

In furthering the development of the chemical atomic theory, the chemists had to choose between two ways. The way chosen later (the acceptance of the Avogadro hypothesis and the associated vapour state of some of the elements as a phenomenon) seemed to be too much hypothetical. The other way, within the limits accepted by Berzelius, relied on a theoretically contradictory basis, which could not be followed by people who realized its inconsistency.

It is well known that, for a time, chemistry entered a third way emphasizing the Gmelin-equivalents. In other words, they neglected theory at all.

Thus the role of vapour density measurement in the structure of cognition changed around 1832. The results mentioned "proved" for Dumas ever more strongly that the atomic weight could only too hypothetically be deduced from the vapour condition of the elements. All this, together with the revision of the introduction of atomic theory into chemistry as well as the revision of its function have finally led to Dumas's passionate refusal of the teaching about the existence of the atoms of the chemical elements.[18] By 1836, Dumas viewed atomic theory as a teaching *dogmatically introduced* not only into chemistry but into science and philosophy as well, out of which certain consequences could be deduced, but this did not modify, in the least, the arbitrariness of the whole. This arbitrariness became manifest to him through the emergence and escalation of contradictions, among which we have only mentioned some concerning the vapour density measurement.

That is to say, all the efforts used to solve the problem of the determination of atomic weights only deepened the crisis. Let us see briefly some of the statements in "Philosophie chimique" that contains Dumas's lectures from 1836 on in the form of a book. (One part of the detail following summarizes his knowledge correctly while the other part contains pessimistic remarks about the possibilities of vapour density measurement.)

"The consequence", he says in connection with the hypothesis about the two-atomic nature of the molecules of the simple gases, "stated on the basis of observation of the four simple bodies, being, as to their nature, in gas-form, and on the basis of observation of the vapour of bromine and iodine, must obviously be refused on the basis of those observations that are connected to sulphur, phosphorus and arsenic."[19]

After this he still had the following, correct, statement to make: "it must be stated that gases, also when they are simple, do not contain, at the same volumina, the same number of atoms, that is, chemical atoms". This statement made with great emphasis for the first time in 1832 was, a year later, approvingly repeated by Berzelius. He commented: Dumas's results have shown that the vapour density measuring of the simple gases does not lead unavoidably and in a direct way to the determination of atomic weight.[20]

On the basis of all that has been said here about Dumas's ideas we clearly see a misunderstanding about him which, except Cannizaro (1860) many historians share, namely, that he did not realize the validity of the Avogadro principle.[21] Cannizaro could better judge the situation. It follows from the foregoing that by realizing the validity of the Avogadro principle Dumas was able to understand the importance of introducing the necessary suppositions but he found this way of thinking too dangerous. "We can admit that chemistry has a means for the determination of atomic weight (vapour density measurement); we can say that we find, in the same volume, sometimes the same number [of atoms], sometimes twice and sometimes three times more, but we never find less in it".[22]

For further research, Dumas proposes: "instead of going deeper into these hypotheses, it would be much better to find a more stable basis for solid theories. Undoubtedly one will think that it would be more useful to determine, with already existing or with new methods, the vapour densities that are unknown to us, while, at the same time, the study of the vapour of compound bodies is also not to be neglected. Though it is true that the study of the latter is obviously less important, it can still lead to significant volume-laws". He adds that "today we still cannot find absolute laws but only changing, although quite simple, relations".[22]

From the point of view of the theory of science, the case is clear: knowledge of the anomalies forcefully led to the completion, through hypotheses of association, of the Avogadro hypotheses which made the chemical atomic theory applicable to vapour density measurements. This step necessarily had an ad hoc character. Dumas, who saw the situation clearest, could not agree with the method of saving accepted hypotheses through another set of hypotheses because he was worried about this method becoming completely arbitrary. Some decades later, however, the association in vapour condition was already taken, for certain element molecules, to be an established fact supported from different sides, i.e., the association in vapour state became a recognized phenomenon. In this way, the anomalous vapour density data could no longer be seen as leading to contradictions with well established chemical analogies as facts, but only anomalies as facts, themselves to be explained on the ground of a more overall conception. So one could again pick up the line using vapour density measurements for the determination of atomic weights.

One component of Dumas critique which emerged in the thirties on the chemical atomic theory was based on the analysis of the arbitrary nature of the hypothesis of the *chemical* atoms, of just one permanent particle for each chemical element. He was right to see that, in this respect, the chemical atomic hypothesis was a translation of the philosophers' theory about the ultimate nature of the world, and thus it was, for chemistry, a rather uncertain hypothesis.

The story of the chemical atomic theory in the first half of the 19th century appears to be much more complicated than the way it can be interpreted in Kuhn's or Lakatos' model. Both of them can only be partially applied. For example, there was not just one paradigm of the chemical atomic theory but there were nearly as many variants of the atomic theory in the twenties and thirties as leading chemists and among these theories there only was a family resemblance. (Note that this period cannot be interpreted as a preparadigmatic phase.) Then the theory in the thirties, forties, fifties was not defended against emerging anomalies by progressive problem shift; the chemists worked otherwise. From the early thirties ownwards, the debate concerning the chemical atomic theory did not sharpen. Rather, the chemists' community *pushed it aside as a whole*. They used the hypothesis of chemical atoms in many ways just as a working hypothesis without great importance. Even those people changed their attitude who, like Dumas, were well aware of the problems expected to arise if chemistry did not make enough efforts to develop a consistent particle theory. Chemistry did not react to the crisis of the theory by reinforcing the debate about its grounds but, at least for a time, by *giving preference to other functions of scientific research* than achieving a consistently founded theory. This is not an uncommon situation in the development of chemistry, a very complicated science, which should receive more attention from the theory of science as a more general case of scientific growth if it is to construct possibly true models of complex processes in the history of science.

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NOTES

1. Knight, D.M.: *Atoms and Elements*, London, 1967
2. This feature constituted the basis of controversies about the value of the atomic theory.
3. See: *Molecules, Atomes et Notations Chimiques*, Paris, 1913, p. 20
4. *Molecules... loc. cit.*, p. 34
5. Gay-Lussac, J.L.: *Ann. Chim.* 29, 1811
6. Despretz, H.: *Journal de Physique* 21, 1822
7. Dumas, J.B.: *Ann. Chim.* 33, 1826
8. *Pogg. Annalen* 7, 1826
9. Dumas, J.B.: *Ann. Chim.* 33, 1826
10. *Ibid.*
11. Dumas is often misunderstood in this connection. His views about the existence of half-atoms is interpreted as if he did not understand clearly the difference between the concept of atom and that of molecule. In fact he only pointed

out the inconsistent understanding of these concepts by Berzelius.

12. Dumas, J.B.: *Ann. Chim.* 50, 1830; 52, 1832

13. It is well known that the meaning of the concept of atom in the first quarter of the 19th century was quite uncertain. Sometimes it was used as another expression for volume. It is worth noting here in what form Poggendorf published Dumas's article from 1826 in German. He left out the theoretical part and interpreted the basic idea of the work in the following way: according to Dumas's supposition all bodies unite in simple volume-relations and so the true atomic weight of bodies is given by the special weight of their vapours.
See: *Pogg. Annalen* 8, 1826 , p. 18. Mitscherlich visited Dumas in 1832 and they discussed the anomalous results of vapour density measurements. His article from 1833 shows that he did not ascribe any special importance to the exact use of the concept of atom.

14. Mitscherlich, E.: *Pogg. Annalen* 29, 1833, pp. 193-230

15. *Ibid.*

16. Gaudin pointed out quite clearly that, through the introduction of necessary ad hoc working hypotheses the atomic theory can be developed on the basis of a theoretically non-contradictory atomic weight system. When evaluating Gaudin's ideas it must not be forgotten, however, that at that time the dualistic theory had not lost its validity and therefore the supposition of more-atomic element molecules remained problematic. Moreover, Dumas felt it quite inconvenient that he could not exclude the supposition that the different element molecules in different reactions could be differently split. This possible thesis also made the chemical concept of atom problematic. Gaudin had no reason as well to argue why at all the concept of the chemical atom must necessarily be used in the

conceptual system of chemistry, that is to say, he was unable to push aside the empiristic counterargument.

17. The contradictions multiplied just in this way.

18. As it can be seen in his book (*Philosophie chimique*, Paris, 1837), already at that time he thought to have realized that the hypothetical-deductive introduction of atomic theory into chemical knowledge is a basic problem.

19. *Ibid.* Ch. 6

20. See Dumas's article in *Ann. Chim.* 69, 1832, and that of Berzelius in *Pogg. Annalen* 62, 1833

21. See, among others, in J.R. Partington: *A History of Chemistry* IV. London, 1964, p. 218. In this book one can find the following: Dumas supposed that "in all elastic fluids .. the atoms are at equal distances from one another". Also: "Since Dumas thought that the molecules of the elementary gases contain two atoms, he was unable to resolve these difficulties", i.e. the problem of the anomalous vapour densities.

JÓZSEF LUKÁCS - JÁNOS KELEMEN

SOME ISSUES IN SOCIAL SCIENCE METHODOLOGY - A HUNGARIAN PERSPECTIVE -

The founders of Marxism-Leninism left us an irreplaceable methodological heritage, but one that has been investigated only in part. A methodological conclusion enunciated by Marx and Lenin is particularly important. In order to obtain a picture adequately reflecting social reality, it is necessary to consider the object under investigation as a whole. The methodological principle of wholeness, as Lenin said, protects us from dogmatic one-sidedness.

The principle of concreteness of approach derives from the dialectical principle of wholeness, but does not exclude the use of "interpretative" abstractions, those that make possible the intellectual reconstruction of empirically given, concrete objects.

The pricipal conclusion from the methodological heritage of Marxism is the need to apply the principle of historicity in investigating social regularities. Marx developed that principle and applied it in contrast to unilinear evolutionism. He persistently emphasized the dialectics of the particular and the universal in history and the unevenness of the development of society.

Marx, Engels, and Lenin believed there are underlying laws in the process of history and on the basis of formulating such laws they outlined the principal criteria for historical progress. Their view about history becoming worldwide as it overcomes geographic and ethnic boundaries is important from the standpoint of the materialist understanding of history. Such criteria of social progress as the pushing back of natural barriers and the replacement of "naturally emerged" forms of community by societal forms have their important consequences in the field of the methodology of history. In the analysis of the course of history, the paired categories of continuity and discontinuity play an important role. Their employment in dialectical unity makes it possible for us to derive lessons from the past; but, nevertheless, it does not admit of mechanical application of these lessons to our day. Marxist methodology regards history as a dialectically determined process in which the necessary and the contingent, the absolute and the relative, do not exclude but presume each other, and in which the discovery of objective regularities has appropriate revolutionary actions as its goal.

The thesis of humanism in Marxist philosophy has a methodological significance too. According to it, the human individuval and the masses of the people must not be regarded merely as passive material for the historical process. They "make" their history and serve not only as actors but as authors in the drama of their own histories.

Consideration of Marx's analysis of what constitutes a socio-economic system, of the laws of the capitalist and communist systems, and of the transition from prehistory to genuine history is an absolute necessity from the standpoint of contemporary research. Marx's *Capital* is an outstanding example, in the history of science, of the application of the dialectics of the concrete and the abstract, and of the structural and historical approach. Being a model of the application of the Marxist paradigm as a method of criticism, it points out the direction to be taken in the study of methodological problems of the social sciences in our day.

In the social sciences, one has to be particularly cautious in applying abstractions, for only a theory taking the most numerous factors into consideration will be free of simplifying the object under analysis. It is true, however, that the social sciences, like the natural ones, are inconceivable without the use of abstractions of the very highest level. In the work of Marx and Lenin considerable attention is paid to the idealizing abstractions. According to them, genuine abstraction does not take one away from reality but, on the contrary, facilitates the identification and correct perception of a system of society and its place in history.

The process of introducing mathematics into scientific research and the utilization of symbolic logic have, in the social sciences, intensified the role of so-called semantic abstractions, formalizations. But formalizations require consideration of the exceptional complexity of the phenomena of social reality and, consequently, have definite limits. Hence, discovery of the limits within which formalization and semantic abstractions do not become absurdities is an important task for methodology. Naturally, the foregoing does not exhaust the rich methodological heritage of Marxism and the problems that emerged in recent discussions on Marxist methodology.

In Hungary, the last decade has witnessed a recognition of the importance of methodological investigations in social science. For example, a discussion focused on the specifics of social laws produced interesting contributions to methodology. An understanding of what is specific to social laws requires, in particular, an answer to the question of whether the methodological universalism proceeding from positivism and sometimes encountered in the writings of various authors in interpreting socialism is applicable to the analysis of social reality. One of the conclusions drawn in the discussion was that social regularities include an element of universality, no matter what specific forms they take. This conclusion is a starting point both for working out a general methodology and for contributing to the development of the individual social sciences as well.

In connection with the development of studies in Marxist sociology, the question of the relation between sociology and Marxist philosophy appeared on the agenda. In the course of the discussion of the interconnection between historical materialism and sociology, it became even more obvious that empirical methods of gaining knowledge of society and the philosophical interpretation of society are different forms of knowledge, though they supplement each other. Sociology cannot ignore the theoretical and methodological principles developed by philosophy; at the same time, the tasks of philosophy go beyond that discipline in the narrow sense and assume the presence of concrete sociological interpretations of societal relationships. On the other hand, it has become obvious (on the level of the interconnection between the historical sciences and sociology) how unacceptable is the view that history "collects" and "digs up" facts, and sociology arranges them in a definite system. The sciences can only fulfil their task - that of identifying one or another aspect of empirical reality - if their empirical and theoretical approaches act in unity.

The treatment in fuller detail of the category of socioeconomic systems produced lively discussions among philosophers, historians (particularly specialists in ancient history), and, above all, historians of culture. In those discussions it was necessary to oppose the point of view that cast doubt on the scientific significance not only of problems that have not yet found a final solution, such as the "Asiatic mode of production," but, even more, of problems concerning the very category "socioeconomic system." It proved necessary also to oppose those who, having offered a mechanistic, undialectical treatment of the essence of social development, were incapable of providing the required aid to the concrete sciences.

The question of the essence of structural analysis and critiques of structuralism have also provoked lively discussion in Hungary. Along with the question of historical and antihistorical approaches, that of the realtionship between ideology and methodology emerged in the centre of debate. In the course of the discussion it was demonstrated that, in themselves, structural descriptions, a striving for a higher degree of abstraction and formalization, and the use of models do not contradict the dialectical application of the principle of historicity. There can be no doubt that systems analysis has become an important factor in scientific analysis in our day. It is equally obvious that it is impossible to draw a sharp line between methodology and ideology. The discussion showed that the Marxist social sciences have to develop their research techniques on the basis of those sciences themselves.

In light of the present state of the social sciences in Hungary and those matters of principle that have been discussed above, we regard it as pertinent to focus attention on the investigation of the following methodological problems:

1. the dialectical unity of the historical and the logical, of the historical and the systems approach in the methodology of the social sciences;

2. the epistemological foundations of the methodology of the social sciences.

In planning the investigation of the first problem, we start with the point that society is capable of being theoretically cognized only if we are able, having abstracted from the level of particular events and direct manifestations of historical life, to reconstruct in depth the interaction of the regularities that are concealed, and sometimes distorted, by the contingent character of empirically accessible processes. It is only a correctly idealized abstraction that permits the replacement of a concrete reality, described with the help of empirical data, by an ideal model thereof and, on that basis, makes it possible to approach an adequate knowledge of the object, to reconstruct its concrete truth, and to generalize it at the system level.

One of the most fruitful postulates in the epistemology of social sciences is, as we have already stated, the organic unity of the historical and the logical. This thesis and the principle of totality are mutually interdependent. In view of them, the social sphere must be conceived as a developing, integral whole, and the empirical phenomena of social life can only be understood on the condition that they are described as parts of this totality. Naturally, the empirically given history of societal forms does not correspond entirely to the abstractions in which the essential characteristics of these forms are reflected. The historical existence of societies and the categories reflecting them are not parallel and cannot be interpreted from an evolutionist point of view.

Basically, it must be accepted that the fundamental categories reflect the forms of existence of historically given societies. At the same time, it would be erroneous to analyze the individual categories in the sequence in which they appeared. As Marx emphasized, the analysis must not proceed from a predetermined sequence, but from the totality of the relations that will stand opposed to each other in a more developed society.

This means that we have to take into account the dialectical and historically logical interconnection between the simpler (historically earlier) and more complicated categories. Accordingly, the most general abstractions regarding the life of society arise only on the basis of analyses of developed societies. Although abstract categories are applicable to any time, thanks specifically to their universality, for all their abstractness they possess historical determinacy and are manifested only within concrete frameworks. Thus, for example, in analyzing the notion of labour, Marx showed that only under capitalism did its universal character become obvious.

In distinguishing between the abstract and the concrete and between simple and historically more advanced abstractions, Marx employed such distinctions as a critical tool aimed against bourgeois economic and social theories. He shows that bourgeois political economy, finding itself stuck at the level of analysis of economic relationships, interprets them extrahistorically, looks upon them as independent abstractions, conceals the contradictions existing in reality, and engages in an apology for what exists, depicting it as logical necessity.

Marx's methodological principles demanded examination of the life of society in its historical development and therefore stimulated investigative thought toward drawing scientifically founded conclusions regarding the future state of society. In describing his method, Marx emphasized the significance of those points in the process of cognition in which structural analysis brings scholars to the need to examine a subject from a historical point of view. Marx particularly singled out those factors in the analysis of capitalist society and its mode of production that contained evidence of the previous state of society and compelled researchers to turn their attention to earlier historical modes of production. Naturally, in order to reveal the laws of capitalist economy, one does not have to describe the entire history of relationships of production and demonstrate in detail how they took shape in the course of history. It is not always necessary to reduce a complex system to those equations of the utmost simplicity that would point to the past and to the preexisting system. Reference is made solely to quite specific moments in the analysing work of a scholar in which proceeding to historical examination of the emergence of the object of investigation is necessary. However, by facilitating a correct understanding of the present, this kind of disclosure of the specifics of what has become the object of research, through describing the process by which it came into being, simultaneously provides a key to an understanding of the past. In this sense, it holds true as well that knowledge of the past presupposes an understanding of the present. Marx's method of examining what exists presumes, at the same time, identification of those aspects of the reality being analyzed in which the overcoming of the contemporary form of production relationships and the outline of the contours of the future are to be seen. In other words, the dialectics of the historical and the logical emerges as the basis, in philosophical methodology, of the study of the past, the present, and the future and of the prospects of communist social development.

Consequently, that the concepts employed in particular social sciences should be historically determinable is an exceptionally important aspect of the interconnection between the historical and the logical. This pertains equally to their epistemological content and their function in terms of world view. In further treatment of this range of problems it is necessary to make extensive use of the achievements of contemporary logic, systems analysis, and other areas of science. In a number of social sciences (linguistics, anthropology, and the historical disciplines), the problem of the interconnection of the historical and the logical arises in the context of the utilization of synchronous and diachronous approaches. The analysis of structural and functional connections also constitutes, for Marxism, a fruitful area of research that it would be wrong to yield to structuralism.

The method of idealizing abstraction (idealization) has become widely prevalent in the natural sciences. It provides an opportunity for logical reconstruction of the internal connections within the entity under study. Such disciplines in natural science as the biological theory of development, paleontology, cosmogony, and others include in their research objects the factor of historicity, which is an organic component of the notion of development. But it is only in the social sciences that the dialectical unity of the historical and the logical finds genuine manifestation and emerges as a necessary precondition for successful research.

When we speak of these methods, however, we must not overestimate them. Neither must we forget, in citing them, that an entity reconstructed by logical means must be referable, as the concrete in thought, to the reality empirically given, and that we may accept such methods as scientifically valuable only if they give us a more appropriate, profound, and integral interpretation of the experimental data than any other theoretical explanation. The social sciences also cannot do without the principle of the concreteness of truth and its verification by practice.

And with this we have already proceeded to a characterization of the epistemological problems of the social sciences. We shall list only the most significant ones.

How do social facts correlate with the theoretical foundation of that branch of science? What is the role of quantification in describing social facts, in identifying the periodicity of mass-scale phenomena, particularly in sociology, political economy, and anthropology? How is one to determine the bounds of applicability of quantification and formalization? What methods of explanation are inherent in the respective social sciences, and what is their logical structure? How are the specifics of the language of the social sciences applicable? What are the communicative potentials of the social sciences, and the obstacles to them? What are the heuristic and axiological functions of the materialist approach to social history in studies carried out by the social sciences?

Along with the explication of this list of epistemological questions, another task of methodological research in the social sciences is the identification of principles of classification and systematization. Thus far, Marxist methodology has given relatively little attention to the theoretical foundations, the cognitive aspects, and criteria of classification and systematization. Treatment of these problems is possible only on the condition that the approach of historical genesis be employed. The basic task consists of demonstrating the historical manifestation of various spheres of social existence and, parallel to that, the historical rise of sciences and their interconnection. Furthermore, a logical reconstruction of particular disciplines would further discovery of the conceptual limits of systematizaton.

The questions concerning the value of comparative approaches should be singled out particularly among the epistemological problems in the social sciences. Comparativistics within the framework of the Marxist methodology can be productive, of course, if the relativist tendency inherent in different schools of historicism (W. Dilthey, O. Spengler, A. Toynbee, M. Weber) is critically overcome, if one rejects a typology that denies both historical progress and historical regularities, and if one discards formalist schemes and subjective intuitive analogies. At the same time, it must effectively oppose that kind of structural functionalism which confines itself to discovering related or similar elements in different kinds of systems, without identifying what is specific to them, what determines the role and function of those elements in given historical frameworks. The Marxist typology of social forms has to be based on broad, comparative analysis in which historical criteria are deeply manifested. Marxist typology indentifies what is universal and law-governed in what is historically distinctive, and this cognitive procedure is regarded as the methodological base of the historical typology of various forms of consciousness (religion, art, etc.).

The topics considered here demonstrate how numerous the methodological problems of the social sciences are. We hope that, at the same time, though only in part, they have also made clear the principal tasks facing Marxists in this realm of research.

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DETERMINACY OF PHYSICAL EVENTS

In constructing a concept of determinism, it is essential to clarify the exact sense behind the determinacy of events and criteria under which we can consider a physical (or, generally, an objective material) event as determined. It is obvious that the determinacy of events cannot be interpreted too widely or it becomes meaningless, losing its heuristic value. Nor can it be too narrowly restricted, since it has to include the field phenomena known,or presumed, to exist today in order to be considered relevant in a philosophical sense (or on another level of generality).

Our definition of the principle of determinism is as follows: *Every objective event is determined by its interactions with other objective events, and only through them.*

We call an objective event determined when having observed it we find that an account of its interactions with other objective events makes it understandable why it happened in the way it did and not otherwise. Two essential points about this conception of determinism must be made:

a) in this definition of the principle of determinism, there is no reference to either the necessary, or the chance, character of the relationship of events;

b) by contrast, the definition itself already stresses that the relationship of events is characterized as an interaction.

On the Concept of State

In classical physics, state was immanently ordered to belong to physical objects or systems, and only through change of state was it related to other objects or systems. (This happened in a more or less abstract manner, through the concept of force: force here represented the intensity and direction of the effect producing a change of state. Yet this concept revealed next to nothing about the character of the physical system producing this effect.) In this way, the concept of state remained somewhat static and, retrospectively, we can see that it only covered a segment of a physical *process* under observation at a given moment in time. In classical physics, this "process-state relationship" was considered in a reversed order: it was not the state which was considered as a segment of the process at a given time, but the process itself was a time sequence of states. This explains why the concept of dynamism is primarily connected to the concept of change of state. In several concrete cases, it was indeed realized that the apparent stability on a given level (that is, the time constancy of the parameters of the state) was the manifestation of changes at other levels. However, it did not become clear that the essence of existing physical systems is not their constancy of state, but *interaction* at a lower level or with other systems: in other words, change.

Modern physical observations have demonstrated the insufficiency of a static, immanent approach to state. For example, the theory of relativity led to the conclusion that we cannot speak of a concrete state of physical events or physical systems, except in correlation with other (concrete) physical systems - with the so-called "reference systems". Microphysics, on the other hand, has proved that an object is not characterized, in itself, by its state but only through its relation to some sort of measuring arrangement (device). Moreover, particle-physics no longer considers its central problem to be determining the parameters by which we can characterize the state of given micro-particles, but concentrates instead on what conditions and in what manner the particles themselves *change* into other types of micro-particles.

That is to say, *the physical world today is no longer considered to be a system of things with immanent qualities* as in classical physics, *but a system of events in interaction with each other.*

The Problem of Individuality

Classical physics construed the world as a system of individual objects which may interact with each other, yet, in this interaction, they behave in keeping with their own immanent qualities. It was important that these qualities could be rendered more or less independent from the circumstances in which they occurred.

One of the basic observations in microphysical research is that it is no longer possible to separate the qualities of the object from its manifestation conditions (its concrete measuring arrangements): that is to say, these objects cannot be considered individual to the same extent as macro-objects are. Nevertheless, since the micro-objects - under well-defined energy conditions - demonstrate a definite (electron, proton, neutron, etc.) character, we cannot speak of a full evaporation of individuality. As against the closed nature of objects or systems in classical physics (which can be realised fairly accurately), *micro-objects and micro-systems are, to a large extent, "open"*. To designate this situation, we have previously used the epithet "quasi-individual".[1]

The Concept of Determinism in Classical Physics

In full harmony with the static, immanent approach to state was the generally accepted Laplacean concept of determinism, which involved an unambiguous and inevitable relationship between phenomena and states. This means that, at least theoretically, random effects were excluded from the determinant factors. However, in practice, unforeseen effects always occurred and, as a result of these, the course of concretely observed phenomena always showed a deviation from the lawlike. Classical theory attributed random effects to insufficient knowledge, assuming that, upon widening the field of information or "extending" the boundaries of the physical system under observation, the random effects could be brought within the framework of necessary and unambiguous laws. *That is to say, the accidental was considered as an epistemological category.*

Essence of Random Effects and their Function in Physical Events

Roughly speaking, the essential difference between classical and modern physics with regard to the problem of determinism lies in the fact that, in the classical concept of determinism, random effects could be excluded, at least theoretically, from the determinants of phenomena, while, according to recent determinism, this is not possible for the results of micro-physical research have shown that the principally statistical character of events is due to the fact that their course is basically affected by chance. Whereas chance could in fact, be neglected in classical physics (and, as we have seen, it was basically taken to be an epistemological factor), in modern particle-physics it plays an essential role. Owing to the basic character of the concept of chance, in an elaboration of a modern concept of determinism, the essential nature of chance and the role random effects play in the development of physical events or processes must be clarified.

The quality of a phenomenon on a given level is determined by some *constant* interactions, as a single interaction may produce a radical qualitative change. Other external and internal relations only affect the given phenomenon within its basic determinacy, in a way (differing from the above) *where only the momentary configuration of them all determines the nature of the effects which will be exerted.* The elements of these non-essential interactions, which lie outside the basic determinacy of the given phenomenon, are a series of events chiefly independent from each other, the only connection between them being through their interaction with the given phenomenon. Consequently, their appearance in the interaction ensemble of the given phenomenon is accidental in contrast with the basic interactions which form a necessary, permanent connection in its qualitative determinacy. The presence or absence of accidental interactions have different consequences, according to the combination of other effects with which they coincide.

To illustrate the aforesaid, the example of an artillery shot will be used. In a cannon shot - taking the gravitational field as a necessary external condition - three basic factors determine *unambiguously* that the missile will strike within a certain area, within the so-called target ellipse: the angle of the gun to the horizontal plane, the mass of the missile and the explosive force of the charge. However, with regard to where exactly each shot will make a hit, we can only make statistical assertions. Beyond basic determinacy, this may depend on a momentary combination of random interactions, such as the changing density of air along the trajectory, and the diameter of individual missiles (which can only be approximately identical) etc. Thus, if we say that a given shot hit

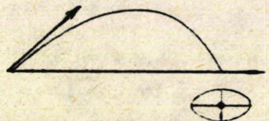

Fig. 1

a certain point of the ellipse due to a given level of air density,[2] and if we take this into consideration with the next shot, it does not follow that it will strike the same point. Such factors as the above, that is, fluctuations in the density of air at a given time and place and variations in the dimension of the missiles, are modified independently from each other, so their total effect depends on their random combination.

In reality, of course, not only those two, but innumerable such factors exist. *Interactions which fall outside the basic determinacy of a given phenomenon and whose eventual consequences depend on the momentary combination of other non-essential and transitory factors, can be called random interactions.* It is important to stress again that, in the example under discussion, the results would always be the same when the three basic factors are present, that is to say, the missile will always hit within the target ellipse, irrespective of interference from any other factors. The combination of these factors is connected by a necessary relationship to the outcome of the event: this is why we call them basic factors. It is evident from the example discussed that we are face to face with a necessary relationship, despite the fact that unambiguous determinacy in the Laplacean sense is missing. It is not only the possible impact points of the missile which are innumerable (indeed, if we consider the target ellipse as a continuum, it is infinite), but the same target ellipse can also be realized through various combinations in the joint presence of basic factors. However, joint random interactions cannot be given once and for all, as they are subject to constant change. So the effect of each element depends on a momentary combination of accidental factors.

The combination of interactions which fall outside basic determinacy forms a *background* to the phenomenon in question, and a highly dynamic one, which represents the relationship of the given phenomenon with a constantly moving, changing, qualitatively infinitely manifold material world. This accounts for the fact that, even if we could establish retrospectively that for the concrete outcome of the shot a concrete cause was primarily responsible, the presence of the same cause would not necessarily lead to the same result in a future occasion. These random causes or interactions are inherent parts of the background and their effects are dependent on its variations. In the above mentioned example, we can state, for instance, that a given shot "over-reached" its target because the missile passing through a less dense layer of atmosphere skidded. The presence of the same thin layer of air would not necessarily produce the same "long" shot again, however, because its effect may be compensated for by a momentary blast of wind from the opposite direction.

The fact that events otherwise independent from each other can come into contact through their accidental effect on a given event, influencing in this way its concrete outcome, renders it possible to define the most important thesis in our theory of determinism: *in the objective world there exist series of events governed by independent laws (independent causal chains).*

In the material world, there are neither specifically necessary nor specifically accidental interactions. An interaction can only be inevitable or accidental *in one concrete, objective relation. It is due to the fact that a group of interactions objectively determines the quality of a phenomenon (that is, these interactions enter into a permanent internal connection), that another group of interactions in the given relationship becomes accidental.* The same interactions connected to other phenomena, or on another level, can become elements of necessary connections.

The momentary configuration of accidental interactions - which is itself ultimately responsible for the outcome of the given concrete event - is subjected to inevitable change, so subsequent events cannot be identical. This is because the supposition that the combination of accidental interactions does not change would require either the complete isolation of the given system, or the complete immobility of the world outside it (that is, of the background, which is the source of accidents). In reality, this never occurs.

Accidental interactions which, at a given moment, react in diverse ways, *can to a certain extent compensate each other*: for instance, in the given example this accounts for the fact that the individual shots are accidental, that is, the exact point of the hit is not unambiguously determined.[3] Instead they are scattered around the theoretical target point according to well-defined laws, so that probability assertions on the concrete course of individual shots can be made.

It is evident from the above that *accidental interactions cannot be contrasted to causal interactions, for accidental changes also have their own causes; any event is the combination of random interactions emerging outside its basic determinacy*. It is therefore obvious that the *intensity and temporal stability* of a given event's basic interactions - which are the carriers of its individuality - and of the combination of random effects determine to what extent the accidental interactions affect the course of the given event. Classical physics was able to offer an adequate description of the phenomena in its field of study using the Laplacean determinism concept because, in these fields, the effects of accidental interactions are objectively negligible. In the application of Laplacean determinism, difficulties emerged when physics began to transcend the boundaries of the classical macro-world, that is to say, when it began to recognize a deeper interaction level, where accidental interactions play a more considerable role.

The Dynamic Character of the Determinacy of Events

Although causal interactions do not exhaust the determinacy of events, they undoubtedly constitute its basic components. So the character of causal interaction is significant from the point of view of a general determinacy, as well.

It is customary to define the causel connection as follows: if A exists, then, and only then, B exists. Symbolically:

$$A \longrightarrow B ,$$

where the symbols A and B can refer equally to a thing, a state, an event or a process. A more general concept of determinism than the Laplacean one demands the explicit statement that a thing or a state cannot be the cause, or generally, the determinant, of another thing or state: it is only an *event or process* which can exercise an active effect on the course of another event or process. Accordingly the symbolic form of the thesis is the following:

$$(A_1 \longrightarrow A_2) \longrightarrow (B_1 \longrightarrow B_2)$$

By applying the causal thesis to events and not to things or states, the dynamic character of the causal determinacy of events comes into the fore. (In order to make the time-dependent, or processual character of events more explicit, the following formula can be used:

$$A(t) = A(t_o) \longrightarrow A(t_1) \longrightarrow A(t_2) \longrightarrow \ldots \longrightarrow A(t_n)$$

For simplicity's sake, we continue to use the symbols

$$A_1 \longrightarrow A_2, \quad \text{and} \quad B_1 \longrightarrow B_2 ,$$

unless it is very important to make temporal dependence explicit.)

It is evident that, if the event $A_1 \rightarrow A_2$ has an effect on the event $B_1 \rightarrow B_2$, than $B_1 \rightarrow B_2$ also exercises an influence on $A_1 \rightarrow A_2$. Consequently, the causal connection has an interactional character and, accordingly, the final symbolic description is as follows:

$$(A_1 \rightarrow A_2) \rightleftarrows (B_1 \rightarrow B_2) \qquad (K_{a1}; K_{b1})^4 .$$

(Let us note for the sake of completeness that the simple arrow in the symbol of an event, or the double one marking an interaction, cannot be identified simply with their usual meaning of implication or equivalence in logic. The arrow in the scheme $A_1 \rightarrow A_2$, if the event is studied "in itself", only refers to some sort of order in the states, to a tendency towards state change in the events. Only by taking into account the causal relation with $B_1 \rightarrow B_2$ can it be considered as an implication, owing to random factors among the determinants, and then only in terms of "probabilitstic logic". If the double arrow symbolizing interaction were identified with equivalence, it would refer to the full symmetry of events forming the two sides of the interaction, but then the interpretation of interaction as a causal relation would become a void formality.)

Naturally K_{a1} is only one component in the determinants of an event $A_1 \rightarrow A_2$; an event $A_1 \rightarrow A_2$ simultaneously takes part in numerous other interactions. At a given moment, K_{a1}, $K_{a2}, \ldots, K_{an}$ interactions (determinants) together determine the event's concrete course. Similarly, the determinants of an event $B_1 \rightarrow B_2$ create the configuration of interactions $K_{b1}, K_{b2}, \ldots, K_{bn}$.

As an interaction, K_{a1} is identical to K_{b1}. However, interactions K_{a2} and K_{b2}, or K_{a3} and K_{b3}, etc., are naturally not identical, since events $A_1 \rightarrow A_2$ or $B_1 \rightarrow B_2$, outside the "common" interaction, are interacting with an entirely different event complex: the interaction structure of the two events is dissimilar. In other words, the pair of events forming the two sides of an interaction (K_{a1};K_{b1}) are dissimilar, so the same given interaction, as cause, leads to a different consequence if connected with event $A_1 \rightarrow A_2$ than with event $B_1 \rightarrow B_2$. *Therefore, from a causal viewpoint, the interaction* (K_{a1},K_{b1}) *is asymmetrical.* The consequence produced by the causal effect is also dependent on the "immanent" qualities of the event subjected to this effect, which, owing to the different interaction structure of events $A_1 \rightarrow A_2$ and $B_1 \rightarrow B_2$, is truly dissimilar.

At last, we should mention, in connection with the possibilities for generalization already referred to, that the events form three groups from the point of view of causality: an event can be described as a) the coming into being of something; b) the change of something; c) the duration, i.e. the relatively permanent existence, of something. In a given case, the definite objective conditions - or the concrete epistemological situation - select one of the three possibilities.

In accordance with the previously introduced notation, the above three types of causal relation can be described as follows:

let the two events be

$$A(t) = A(t_1) \rightarrow A(t_2) \rightarrow \ldots \rightarrow A(t_n)$$
$$B(t) = B(t_1) \rightarrow B(t_2) \rightarrow \ldots \rightarrow B(t_n) \;;$$

then a) *coming into being* as a causal relation:

$$[A(t_k) \rightarrow A(t_1)] \rightarrow [B(t_o) \rightarrow B(t_1)]$$

b) *change* as a causal relation:

$$[A(t_k) \rightarrow A(t_1)] \rightarrow [B(t_k) \rightarrow B(t_1)],$$

c) *duration* (relatively permanent existence)as a causal relation:

$$A(t_o - t_1) \rightarrow B(t_o - t_1) \quad .$$

In a), t_o refers to the fact that the event B has just come into being; in b), t_1, indicates that the event B has taken another shape in the mutual connection with A than it would have otherwise taken; in c), the argument $t_o - t_1$ refers to the time interval where both A and B exist, and B does not exist outside this interval.

Pre-Determination and Post-Determination

It is obvious that a concept of materialistic determinism basically depends on the fact that there should exist an inevitable relationship between a *completed event* and its determinants, since only in this way can the role of non-material, irrational factors be excluded. However, micro-physical experiments demand that *random factors* influencing the concrete course of an event should also be considered as determinants of the event in question.

Moreover, if we accept that the accidental interactions of a given event, or their momentary configuration, also belong to the determinants of the event, then it is necessary to distinguish between the problem of the determinacy of a *future event* and of one which *has already occurred*, in both an ontological and an epistemological sense. The first will be referred to as *pre-determination* and the second as *post-determination*. The basic problem is then how it is possible to reconcile the role of accidentals in the pre-determinative formation of events with the inevitable post-determinate relations of the emerging phenomena and its determinants without interfering with the objectivity of the accidental.

We must consider the following widespread argumentation: simultaneously with the permanent, essential, (necessary) interactions involved in the production of an event, accidental interactions also affect it. Thus, within a *finite time interval* before the actualization of an event, it is impossible to predict with certainty its concrete outcome, nor is it objectively, unambiguously determined, owing to unexpected random effects. If we consider the problem an infinitely small time interval *dt* before the emergence of the event, the situation is entirely different. In such a short time, new random interactions cannot be introduced and, at that point, given determinants and the developing event can form an unambiguous relationship.

Two remarks are to be made about this suggestion. Firstly, the expression "an infinitely small time interval *dt* before the emergence of the event" does not make any sense. Because, should the short interval *dt* mean an *infinitely* short duration, then new random interactions could not interfere with the event, *since, within an infinitely short time, no change can occur in the event itself.* (There are no timeless physical, or any other material, changes.) "An infinitely small time interval *dt* before the emergence of the event" indeed refers to the event itself. Secondly, should *dt* be optionally short, but finite, then not only may the event in question change, but new accidental factors may arise as well.

Every concrete event is determined as a process by the multiplicity of inevitable and random factors. As a result of the accidental factors the concrete occurrence of individual events can diverge but, at the same time, the necessary interactions (determinants), which are similar in similar types of processes, give a definite character to the particular event structure.

Evidently, the solution to the problem is to take the objective, material random interactions into account as determinants and *not to regard events pre-determinatively as unambiguously determined.* We can unequivocally reject metaphisical Laplacean determinism, if we accept the "open" nature of phenomena and the objectivity of the accidental. We believe that insistence on an unambiguous predeterminism only reflects a conscious or instinctive nostalgia for Laplacean determinism.

As far as post-determinism is concerned, we can say the following: with post-determination, we can consider (at least theoretically) all factors, both necessary and random, which affect the course of an event, that is, we recognize an unambiguous relationship between a particular consummated event and its determinants. At this point, however, we should make two remarks. Firstly, the mere fact that there is a post-determinative, unambiguous relationship between an event and its determinants does not mean that the random determinants lose their accidental character. Secondly, if we post-determinately apply statistical methods to numerous concrete events, we can only formulate a probability statement for the, as yet, unproduced event. Since the laws of nature are general, permanent *tendencies* in objective processes (or sets of events), the relevant basis of characterising such laws is the pre-determinative situation. Accordingly, statistical (probabilistic) laws are to be considered as the general form of the laws of nature. The fact that, despite an unambiguous relationship between an event and its determinants, we can only make probabilistic statements for future events is the subjective reflection of the objectively statistical character of the laws of nature.

All these have special significance with regard to micro-physical events. It is well-known, for example, that, as far as particles are concerned, the state of the environment is also important in deciding which side of the wave-particle dualism, a characteristic of the quantum mechanical state, will prevail; that is, whether the object will behave as a wave or a particle. (Micro-objects can be regarded as quasi-individuals.) In addition, among the interactions affecting the state of micro-objects, random interactions play an essential role. Actually, the two sides in a micro-physical interaction cannot be regarded as well-defined individual changes of state. Events $A_1 \rightarrow A_2$ and $B_1 \rightarrow B_2$, due to the interaction of micro-objects with their environment (due to the "open" character of the micro-objects themselves), represent a more or less large segment of the objective physical world, and, because of the accidental nature of some interactions, they are objectively ambiguously defined. Therefore, the concrete manifestation of the given interaction in the events constituting one or another side of the interaction is affected by both the individual qualities of the event and the structure of its interaction with its environment. The statistical nature of micro-physical causality, or, in a more general sense, the determinacy of micro-phenomena, is due to this fact.

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NOTES

1. MÜLLER, A.: Quantum Mechanics: a Physical World Picture Akadémiai Kiadó - Pergamon Press, Budapest-Oxford, 1974
2. This naturally is *ab ovo* an idealized supposition, since other factors, not taken into consideration here, may also contribute to the shot.
3. Of course, not determined in the Laplacean sense.
4. The interaction in question is referred to from the angle of the event $A_1 \rightarrow A_2$ as K_{a1}, from that of $B_1 \rightarrow B_2$ as K_{b1} .

KÁROLY REDL

ON THE FIRST EUROPEAN THEORY OF MONEY

(*Nicole Oresme's Treatise on Money*)*

> "Moneta apellata est quia monet ne qua fraus in metallo vel in pondere fiat"
>
> (Isidorus, *Etymologiae*, lib. XVI., cap. XVIII.)

1. Who was Nicole Oresme?

During the 14th century, the spreading nominalistic philosophy, the so-called "via moderna" created favourable conditions for new problems and stimulated the interest in the natural sciences as well as in the problems of social philosophy. For the development of natural science, it proved fundamentally important that Ockhamism related critically to Aristotelian philosophy and physics[1], and that it attributed decisive significance to "intuition", i.e. direct experience. A great number of scientists, e.g. the fellows of Merton College who pursued the traditions of the 13th century Oxford school (Thomas Bradwardine, William Heytesbury, Richard Swineshead), or the representatives of the Paris school (Jean Buridan, Albertus de Saxonia, Marsilius Inghen) reached insights in the area of natural sciences - mainly in mechanics and astronomy - which formed a transition to renaissance and modern sciences. The philosophers of the Ockhamist school (e.g.

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* Extracts from a longer essay. The author made the first Hungarian translation of Nicole Oresme's *Tractatus de mutationibus monetarum*. He gave a synopsis of it in Magyar Filozófiai Szemle [the Hungarian Review of Philosophy] 1982/3. Nicole Oresme's "Tractatus" is here quoted from Charles Johnson's bilingual critical edition. Charles Johnson: The De moneta of Nicholas Oresme and English Mint Documents. (London, 1956.)

J. Buridan, Heinrich von Langenstein, Gabriel Biel)[2] also show a lively interest in questions of social philosophy (ethics, economics). Among the great figures of "via moderna", we find Nicole Oresme who, through his significant mathematical, physical and astronomic accomplishments, was considered by P. Duhem to be the forerunner of Copernicus, Galilei, and Descartes, and is generally considered to be the most outstanding national economist in the 14th century.

Nicole Oresme was born around 1323 near Caen.[3] According to a note from 1348, he studied theology in Paris at Navarra College. He obtained his master's degree in 1356 and was a Grand Master at Navarra College from 1356 to 1362, where he knew J. Buridan, the well-known scientist. He was the tutor of the Dauphin, the future Charles V, and he probably wrote his famous financial treatise *De mutationibus monetarum* around this time, in the fifties. His other Latin works in the fields of mathematics and physics were presumably born during this same period. In 1362, he is canon in Rouen and later holds a canonship at La Sainte Chapelle in Paris. He makes a speech in front of Pope Urban V in Avignon at Christmas, 1363.[4] He directs attention to the dangers caused by the unreasonable concentration of property and wealth in the hands of the Church, as it sharpens the differences between the lower and higher clergy and between the ecclesiastical and secular spheres. He criticizes corruption and symony and warns the Church against withholding necessary reforms because of the illusion of the unchangeability of existing conditions.[5] In 1364, he gained the office of dean at Rouen Cathedral with the help of the Regent and after the Regent's crowning as Charles V the same year, Oresme became his advisor. By order of the king, he translated several of Aristotle's works from Latin into French (Politica, Ethica, Oeconomica, De caelo),[6] thereby promoting vernacular culture. He also wrote other astronomical and theological works. He became bishop of Lisieux in 1377 and died in 1382.

We cannot here deal with his work in the field of natural science.[7] We will only mention his astronomic theory belonging to the pre-history of the Copernican revolution: Oresme, in opposition to Aristotle's theory that the Earth is in a state of rest and the Heavens perform daily revolutions, brings arguments in favour of the viewpoint that the Earth moves and the Heavens are in a state of rest. In his interpretation, the biblical statements expressing a geocentric viewpoint need not be taken as literal scientific truth, as they follow generally accepted usage.[8]

We will prefer to deal with Oresme's famous treatise on the theory of money which has Latin versions as well as Middle French ones, and is directly connected with the topical problems of French financial policy.

There are several factors playing important roles in the birth of the treatise and the development of its theoretical standpoint. These are both theoretical and historical.

As far as the theoretical presuppositions are concerned, we must reckon with the ever more determined efforts from the beginning of the 14th century opposing the ideology of feudalism. First of all, the philosophical and political theories of nominalism, the "via moderna", and radical "Averroism" played an important role. These trends were effective in the separation of theology and philosophy from a methodological viewpoint, and, of the ecclesiastic and secular spheres from a political one, thus creating a favourable atmosphere for empiricism and scientific research on the one hand, and preparing the vindication of the principles of popular sovereignty and representational system, on the other.

From the point of view of social and historical factors, especially in the case of France, we must consider the consolidation of the third estate, the bourgeoisie, and the centralizing efforts of the royal power, noticeable from the end of the 13th century on. The financial and taxation policy naturally played an important role in the development of the centralized monarchy.[9] The intertwining of the financial and fiscal policies in the royal right for coinage created a characteristic dilemma as shown by Hector Estrup.[10] If the financial policy, or, more precisely, the right for coinage is considered to be the source of fiscal income, then the government is in a never-ending temptation fo find income through the manipulation of money weight, i.e. through currency debasement, which may lead to the faltering of the monetary system, the damaging and impairment of financial conditions. If the government leaves the financial system untouched, that may easily lead to a deficit in the State Treasury, considering the special political difficulties in the assessment of new taxes, and in tax increase.

2. *On the Topic and Sources of the Treatise*

From the very first sentence of his Introduction, Oresme presents the problem under consideration as a debated question, namely: is the alteration of money a royal privilege or not?

> Some men hold that any king or prince may, of his own authority, by right or prerogative, freely alter the money current in his realm, regulate it as he will, and take whatever gain or profit may result: but other men are of the contrary opinion.

This way of raising a question by itself represented a provocation against the feudal concept of financial affairs. And it grew into a definite opposition in Oresme's central thesis: "Money belongs to the community" (Moneta est communitatis).

The contents of the work's 26 chapters may be summarized as follows:

The first chapters (I-VII) discuss the general problems, regarding money, its origin, role, properties, manufacturing and ownership. The next 7 chapters (VIII-XIV) describe the different forms of money alterations. The next chapters (XV-XXII) discuss the inadmissibility of these alterations. Then 2 chapters (XXIII-XXIV) present the arguments offered for the monarch's right to alter the currency - and give the refutation of these arguments. The last 2 chapters (XXV-XXVI) discuss the political aspects of the question: besides criticism of the tyrannic exercise of power (XXV), he shows the harmful effects of the alteration of coinage on the future of the realm (XXVI).

As far as the sources of Oresme's Tractatus are concerned, he quotes some books from the Bible (The Laws of Moses, Prophets, Gospels), he repeatedly refers to Aristotle's Ethics and Politics, he refers once to Huguccio, the famous canonist, and quotes Cicero, Ovidius and Seneca from among Roman writers. One of his important sources is Cassiodorus who, standing on the boundary line between late Antiquity and Middle Ages, mediated the transfer of ancient cultural material to posterity. Cassiodorus, as it appears from Oresme's quotations, was concerned with the problems of the financial situation in Late Antiquity[11] and this is the reason for Oresme's interest who also had to face practical tasks in the area of financial affairs.

The above comprise practically all the source areas for medieval economic thinking. The Middle Ages developed their own standpoint, the ideology suited to the specific needs and trends of the period, by reprocessing, reinterpreting and developing these ideas again and again.

The Bible remains of course an unavoidable and indispensable source. The provisions of Mosaic law, the social criticism of the prophets and the ethos of the Gospels, contained many things which could be related to the economic problems of the Middle Ages.

The works of the ancient philosophers, especially Aristotle's Ethics and Politics, already known to 13th-century schoolmen, contain a manifold political and economic theoretical material, treated at a high level. It was studied by the schoolmen with great interest, as they discovered analogies between the city-life unfolding in front of their eyes with the organization and constitution of city-republics, and the ancient polis (civitas) analysed by the Philosopher.

The revival of the study of Roman Law and the systematization of canon law also helped them in the legal formulation of the problems occurring in the developing trade-and financial relations.

They naturally utilized the materials of the Christian intellectual tradition preserved by the Church Fathers and the Christian authors of Late Antiquity (e.g. Boetius, Isidore of Seville, Cassiodorus).

In all these areas, the Middle Ages found a specific circle of problems which had mainly developed on an ancient ground, but served nevertheless as a suitable starting point for medieval scholars in the formulation of their own problems. Oresme could, to a certain degree, adhere to this circle of problems.[12]

3. *The Significance of the Treatise*

In attempting to summarize the significance of Oresme's treatise in one statement, we should perhaps emphasize the fact that a definite step is made here from "oeconomica", expressing the ancient and medieval concept of the economy towards "oeconomia", the science of modern national economics. "Oeconomica" was essentially the science dealing with the direction and government of the family-type "small enterprise", the fundamental unit of production in subsistence farming, whereas now more comprehensive phenomena of the economic life emerge on the theoretical horizon, the problems of national economy as a whole are outlined for the first time, heralding the birth of "oeconomia", the new science christened "political economy" by Montchrétien.

We have referred earlier to the remarkable phenomenon consisting in the parallel interest of the thinkers of the "via moderna" in problems of both natural science and social life.[13] In both respects, we can see a process of thematic and methodological emancipation and of separation from the traditional framework of the theological-philosophical system. This process was realized through several stages: first of all, still within the framework of the great speculative systematizations, the "summae", certain problems were expounded in greater detail and more thoroughly than earlier, so that a given topic grew into an almost independent treatise.[14] Another stage is when, leaving the area of theological systems, the study proceeds on the level of philosophy in the narrower sense, usually in the form of commentaries written to Aristotle's works.[15] Finally, there appear treatises independent in genre, devoted to special topics. These are the first "swallows" of the increase of interest in scientific specialization.[16] It is advisable to consider these stages in a "logical", rather than a strictly historical sense, as earlier, e.g. in the 13th century, we can already find independent treatises on different natural, social or economic questions.[17] Such questions are discussed in a wider speculative theoretical context at a later date as well.[18] The formal- "logical" differences do however express a "historical" tendency: generally speaking, the separation process of specialized sciences from the unifying framework of traditional theological-philosophical systems.

Last but not least, the significance of Oresme's Treatise - even from a mere formal point of view - lies in the fact that he makes the first decisive step - at least in one respect, in the problem of currency -, by writing an independent economic work and opening the line of works devoted to economic problems.

The significance of Oresme's Treatise as regards its contents lies in the fact that it fits into the developing stream of 14th century criticism against the traditional forms of feudal ideology. In this way, a conception is emerging in his work which foreshadows and prepares, respectively, the economic outlook of the age of the original accumulation of capital, the viewpoint of mercantilism and its early stage, monetarism. It cannot be considered accidental that Oresme's Treatise saw numerous editions precisely during the 17th century.[19]

As in monetarism, and later in mercantilism, in Oresme's Treatise the interest is focused on the question of money. This means that the sphere of economy is approached from the side of trade, i.e. the circulation of goods. Ancient and medieval theories opposed "natural riches" to "artificial riches" and placed the stress on the activity producing use-value, consumption goods, corresponding to the leading role of "subsistence farming". It gave evidence of distrust for money as "artificial riches", owing to its destructive effect on the community and "morals" and only justified its use as the mediator in the exchange of goods. On the other hand, the advance of commodity and money economy, under feudal conditions, brought a new emphasis on the money problem which, on one hand, brought a more positive acknowledgement of money, while, on the other hand, kept alive the arguments and counter-arguments of traditional criticism against the increasingly independent movement of the currency.

Oresme's Treatise deals primarily and directly with the problems of the alterations of coinage in feudal practice. His theoretical starting point is the Aristotelian discrimition between natural riches and artificial riches and, accordingly, he tries to limit the role of money to its mediatory function in the exchange of goods, i.e. he attributes a normative importance to the G-M-G /goods - money - goods/ movement. However, we can clearly distinguish the importance of circulation and trade relations as well as, in harmony with this, the separation from the feudal legalistic conception of money and the advance toward monetarist metallism.

As is well-known, ancient and medieval theories considered money a measure and symbol of value determined by law and thought real riches consists in the abundance of goods /abundantia rerum/. On the contrary, the monetarist-metallist theory explained the special role of money through the natural characteristics of precious metals (gold, silver) and declared that "riches is money" ("Das Reichtumb, das ist Gelt")[20].

There is a close correlation and correspondance between the concept of the nature and right of money. The interpretation of money as a simple symbol corresponds to the legalistic theory and practice which thinks the determination of the value of the currency is the right of the legislative or a supreme authority. This conception served as a basis for the justification of the royal right for altering currency in the Middle Ages which, in practice, often meant the justification of forgery. Marx states in connection with the 14th century money policy of the House of Valois:

> Lawyers started long before economists the idea that money is a mere symbol, and that the value of the precious metals is purely imaginary. This they did in the sycophantic service of the crowned heads, supporting the right of the latter to debase the coinage, during the whole of the middle ages, by the traditions of the Roman Empire and the conceptions of money to be found in the Pandects. "Qu'aucun puisse ni doive faire doute", says an apt scholar of theirs, Philip of Valois, in a decree of 1346, "que à nous et à notre majesté royale n'appartiennent seulement ... le mestier, le fait, l'état, la provision et toute l'ordonnance des monnaies, de donner tel cours, et pour tel prix comme il nous plait et bon nous semble." It was a maxim of the Roman Law that the value of money was fiwex by decree of the emperor. It was expressly forbidden to treat money as a commodity. "Pecunias vero nulli emere fas erit, nam in isu publico constitutas oportet non esse mercem."[21]

The schoolmen who, in the wake of Roman lawyers, followed this legalistic interpretation and distinguished money from commodities, considering it essentially a state-determined measure of value whose role in the economic life, in the exchange of goods, was not so much due to its natural internal properties as to the fact that it was the "creature" of law, were in this "non-metallist" conception also backed by Aristotle's authority.[22]

In Oresme's Treatise, we can see significant differences from the traditional ideas in both respects: in the interpretation of the nature and the right of the currency. We witness a transition to a monetarist or mercantile conception as Oresme emphatically declares that money is not a thing arbitrarily alterable by the monarch but its value and functioning as currency are closely related to the natural properties of precious metals used as currency. (cf. Ch. X.)

This leads to the other significant change which separates Oresme's views from the former ones. This concerns the right of money which was considered a domanial right or regale in the feudal conception. Oresme, however, most definitely represents the standpoint that financial affairs are the business of the community or of its better, wealthier part (communitas sive pars valentior eius); the ownership of money is their legal due on the basis of *natural law.*

H. Estrup,[23] one of the reviewers of the Treatise shows that Oresme emphasises the institutional nature of money, i.e. that financial affairs should be assigned to the authority of the community as a unit. Another new idea is that the right to coinage differs fundamentally from domanial rights and privileges and that the financial system should be handled as a public affair and national interest by the supreme authority, rather than as the legal and official source of income to the royal power. Oresme's support for the claims of the estates and his democratic attitude are concisely and briefly expressed in his central statement: "moneta est communitatis", money belongs to the community.

Oresme's Treatise, its practical nature and close connection to politics remind us again of the approach of mercantile economists as he examines economic problems in their wide connections - on the level of the national economy. He attempts to scrutinize the nation's economic life as a whole and this whole is opposed to the conditions in other countries as economic units. His primary attention is drawn to foreign trade turnover and, consequently, to the questions of money circulation. This new viewpoint explains why economic questions emerge in their concrete political, legal or moral aspects and not as isolated problems, as pure theories.

Oresme's observation and analysis of trade conditions leads to statements which may rightly attract the attention of economic historians. It is known that the formulation of the first economic law is generally attributed to Thomas Gresham (1519-1579), the founder of the London Exchange (1566) who served both Edward VI and Queen Elizabeth. Gresham's law declares that if money of lower value is circulated in a country together with money of higher value, the former will crowd out high-value money.

We meet a formulation in Oresme's Treatise, some two hundred years earlier, which may be qualified as the anticipation of Gresham's law.[24] In Chapter XX, Oresme, analysing the consequences of the debasement of currency, writes as follows: "Again, such alterations and debasements diminish the amount of gold and silver in the realm, since these metals, despite any embargo, are carried abroad, where they command a higher value. For men try to take their money to the places where they believe it to be worth most. And this reduces the material for money in the realm".

Oresme points out the negative effects of these debasements from the point of view of the exchange of goods and foreign trade as well as the problems of the flight of capital: "Again, because of these alterations, good merchandise or natural riches cease to be brought into a kingdom in which money is so changed, since merchants, other things being equal, prefer to pass over to those places in which they receive sound and good money." (Ch. XX)

Economic discussions in the treatise are organically embedded in the political conditions. The demand for financial reforms is raised as a question in the struggle between the estates and the royal power. In this struggle Oresme appears as a representative of the estates whose affections lean toward the bourgeoisie. His bourgeois estate democratism is especially evidenced by the discrimination between tyranny and Kingdom. This democratism even employs the reference to national consciousness in the defence of estate interests.[25]

4. Final Remark to the Central Thesis

Oresme's central thesis, as we have seen, is the following: "Money belongs to the community". (Moneta est communitatis.) However, the question arises: who actually constitute this community? The problem of the social content of the expression "community" (communitas) is raised by E. Schorer,[26] editor of Oresme's *Tractatus*. He comes to the conclusion that this "community" does not mean all the people, but the community of private owners, of those who possess money if this possession is legal. We have indeed reasonable grounds for supposing that it does not simply mean the whole population of the country but rather the more or less wealthy social strata which really possess money, wealth and property. This is what Oresme refers to in the restriction of the expression "community": the community or the better part of it (communitas sive pars valentior eius).

The notions of "community" (communitas) and better part (pars valentior) are not accidental, ocassional formulations but the cardinal concepts of a wide-ranging, important theoretical conception, the so-called corporative theory.

The corporative theory, this characteristically medieval offset of legal theory in the 13th century brought the fundamental transformation in the outlook and theories about society, and the actual beginnings of a theory of the state as shown by Jenő Szűcs, a Hungarian researcher of this theory.[27] The significance of the corporative theory primarily lies in the fact that it is the first serious attempt to surpass the hierarchic view of political theory and to grasp theoretically the real, horizontal, autonomous structures of society. Corporation, universitas, communitas, are the basic model of an "autonomous" society.[28]

In reference to the principle of representation in the corporative theory, the "better part" (pars valentior) charged with the representation of the estate did not simply mean the quantitative "majority" but the qualitatively "more powerful" part. What does this "qualitative" content of the expression cover? The discussions of J. Quillet help us understand this through the analysis of "Defensor Pacis" by Marsilius.[29] His analyses show that "pars valentior" with Marsilius means the part representing the universality of the citizens (universitas civium) in a qualitative and quantitative sense at the same time, and is not merely a majority system. Hence the statement, often appearing in literature, according to which Marsilius is the mouthpiece of the principle of popular sovereignty must be handled with some reserve. The real meaning of the expression covers not only a numerical majority but also the part with greater riches. So we can translate this expression as the richer, wealthier part. This is witnessed by Aristotle's *Politics*, the main source of Marsilius, which he does not interpret in a "Platonic way" but "sociologically", i.e. he wants to draw the model of an easily realizable, purposeful constitution which assures state-existence, and its most important condition: peace, instead of an ideal but practically unrealizable state.

Aristotle says in his *Politics* (Book VI, Ch. 3/1318a):

> "Now they agree in saying that whatever is decided by the majority of the citizens is to be deemed law. Granted: - but not without some reserve; since there are two classes out of which a state is composed, - the poor and the rich, - that is to be deemed law, on which both or the greater part of both agree; and if they disagree, that which is approved by the greater number, and by those who have the higher [property] qualification. For example, suppose that there are ten rich and twenty poor, and some measure is approved by six of the rich and is disapproved by fifteen of the poor, and the remaining four of the rich join with the party of the poor, and the remaining five of the poor with that of the rich; in such a case the will of those whose [property] qualifications, when both sides are added up, are the greatest, should prevail." (Translated by Benjamin Jowett)

The political practice in the Italian city-republics, which Marsilius refers to, also shows that at the time of his writing the former democratic government of the communes had become oligarchic. The statutes of Padua, Siena and Florence in this period prove that the composition of the city councils is based on wealth, the definition of "pars valentior" being based on the movable and especially the immovable property of the bourgeoisie.

Presumably, the expressions in Oresme's Treatise like "communitas aut valentior pars eius", "optimae partes communitatis", or "meliores subditi" occur in the same or similar meaning and concern the wealthier, richer, more influential strata of the community. At any rate, for Oresme, the communitas-theory is obviously a means by which he validates the claims of the estates against the possible abuses of the central royal power, and not just the common claims of the three estates - clergy, nobility and bourgeoisie - but, as we may infer from certain of his allusions, he shows particular sympathy for the bourgeois estate.

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NOTES

1. The leading line of the attack against Aristotle's system and the development line of mechanics coincided in the Middle Ages - writes Károly Simonyi in his "*A fizika kultúrtörténete*" ["The Cultural History of Physics"] (Gondolat Publishing House, Budapest 1978, p. 128)
2. E.G. Jean Buridan (1300-1358) in his Comments on Aristotle's works (Quaestiones super decem libros ethicorum Aristotelis ad Nicomachum: Quaestiones in VIII libros politicorum Aristotelis); Heinrich von Langenstein (1325-1397) in his works about the contracts of sale (Tractatus bipartitus de contractibus emptionis et venditionis; Epistola de contrac-

tibus emptionis et venditionis ad Consules Viennenses); Gabriel Biel (1430-1495) in his treatise on money (Tractatus de potestate et utilitate monetarum).

3. About Oresme's life, activity and works, see M. Clagett, *The Science of Mechanics in the Middle Ages*. The University of Wisconsin Press, Madison, 1959. pp. 337-340. The catalogue of Oresme's manuscript and edited works is found in M. Clagett, *Nicole Oresme and the Medieval Geometry of Qualities and Motions*. The University of Wisconsin Press, Madison, etc., 1968, pp. 645-648.
A basic work from earlier literature about Oresme's economical and political views and about his age: Émile Bridrey, *La Théorie de la monnaie au XIV^{e} siècle: Nicole Oresme, étude d'histoire des doctrines et des faits économiques*, Paris, 1906.

4. "Sermo coram papa Urbano V et cardinalibus habitus anno 1364 (1363?)." Editions: Flacius Illyricus: Catalogus testium veritatis (Basel 1556; Lyon 1597); J. Wolf: Lectionum memorabilium et reconditarum centenarii XVI. Vol.2. (Lauingen 1660); independently: S. Gesner (Wittenberg 1604); O. Gratius: Fasciculus rerum expetendarum. Vol. 2. (London 1690) Cf: A. Dempf: Sacrum Imperium, München und Berlin 1923, p. 538.

5. Well fore the speech in Avignon, in his Treatise, Oresme compares the illnesses of the state to those of man based of the ancient Aristotelian parallel. (*Politics*, V.3., 1302b-1303a) He shows the danger the illness which the concentration of wealth into one part of the state body means for the working of the whöle organism: "As, therefore the body is disordered when the humours flow too freely into one member of it, so that that member is often thus inflamed and overgrown while the others are withered and shrunken and the body's due proportions are destroyed and its life shortened; so also is a commonwealth or a kingdom when riches are unduly attracted by one part of

it. For a commonwealth or kingdom whose princes, as compared with their subjects, increase beyond measure in wealth, power and position, is as it were a monster, like a man whose head is so large and heavy that the rest of his body is too weak to support it. And just as such a man has no pleasure in life and cannot live long; neither can a kingdom survive whose prince draws to himself riches in excess as is done by altering the coinage." (Ch. XXV)

Oresme uses the favoured ancient and medieval analogy of musical harmony in order to support the demand for the state's functional harmony. Extreme financial equality is as undesirable as extreme inequality is: proportional and measured inequality is necessary (Ch. XXVI).

Concerning Oresme's ideas on the aesthetics of music, expounded in his "Tractatus de configurationibus qualitatum et motuum", see: V.P. Zoubov, "Nicole Oresme et la musique". *Mediaeval and Renaissance Studies*, Vol.5, (1961) pp. 98-107.

6. Modern editions: *Maistre Nicole Oresme: Le livre de Politiques d'Aristote*. Ed. A.D. Menut. Philadelphia, 1970. Transactions of the American Philosophical Society, New Series, Vol.60, Part 6. - *Maistre Nicole Oresme: Le livre de Ethiques d'Aristote*.Ed. A.D. Menut. New York, 1940. - *Maistre Nicole Oresme: Le livre de Iconomique d'Aristote*. Ed. A. Menut. Phildelphia, 1957. Transactions of the American Philosophical Society, New Series Vol.47, Part 5 (1957), pp. 785-853. - *Le livre du ciel et du monde*, Ed. A.D. Menut and A.J. Denomy, C.S.B. Mediaeval Studies, Vols. 3-5 (1941-43). New ed. Madison, University of Wisconsin Press, 1968.

7. In a review of the medieval debate on "intensio et remissio formarum", focussing on "quantification" of qualities, Márta Fehér deals with Oresme's relevant views in: "A mérhető világ felé" [Towards a Measurable World]. In: *Világosság* 15 (1974), 12, pp. 730-738, especially pp. 736-737.

8. Károly Simonyi, *Op. cit.*, p. 128, deals with Oresme's astronomical views and publishes an excerpt from Oresme's Comments to Aristotle's *De coelo et mundo*, where Oresme discusses the arguments for the daily revolution of the Earth (pp. 129-131).

9. N. Elias, Über den Prozess der Zivilisation. II. Bd. Wandlungen der Gesellschaft. Entwurf zu einer Theorie der Zivilisation. Suhrkamp 1979[6], see especially Chapter VIII (Zur Soziogenese des Steuermonopols) pp. 279-311.

10. H. Estrup, "Oresme and Monetary Theory". *The Scandinavian Economic History Review*. Vol. XIV. (1966), No. 2, pp. 97-116, especially pp. 97-98.

11. An aspect of the characteristic crisis of the last period of the Roman Empire was the debasement of the currency and a shortage of money. Cf.: Jenő Darkó, *Császárimádó Róma - képromboló Bizánc* [Emperor-adoring Rome - Iconoclastic Byzantium]. Magvető Publishing House, Budapest 1977, pp. 17-18.

12. A good survey on the development of economic views from ancient times to the establishment of classical political economics: B. Gordon, *Economic Analysis before Adam Smith. Hesiod to Lessius.* Barnes and Noble, 1975. B. Gordon gives a detailed account of the developments of economic analysis between 1300 and 1600, including the formation of monetary theory and he deals with Oresme's Treatise briefly pp. 188-192.

13. In this relation, it is noteworthy that a pioneer in modern astronomy like Nikolaus Copernicus, in addition to his work in natural science, also devoted time to the study of economic, more precisely, financial problems, as proved by his "Monetae cudendae ratio". Cf: Edward Lipinski, "Nicolaus Copernicus, a közgazdász" [Nicolaus Copernicus the economist]. In: *Copernicus és kora* [Copernicus and his Age]. Selected and edited by Dr. Barbara Bienkowska. Gondolat Publishing House, Budapest 1973, pp. 159-174.

It cannot be acciendtal that Hegel, in a profound comparison which is a complete research programme in a nutshell, chooses to compare political economy and astrology, of all things. Cf. Hegel, *The Outlines of Legal Philosophy* § 198. and Appendix.

14. For example, the French theologian Petrus Cantor (d. 1197) already deals with financial problems in the 12th century in his *Summa de sacramentis et animae consiliis*. Cf.: John W. Baldwin, Masters, Princes and Merchants. Vol 1, Princeton, 1970, pp. 241-244. - In the 13th century, Thomas Aquinas devotes two independent "quaestio"-s in his Summa Theologica (Pars Secunda Secundae) to the discussion of economic-ethical questions raised in voluntary exchange affairs [commutationas voluntariae]: Quaestio LXXVII analyses agreements about sale (De fraudulentia in emptionibus et venditionibus contingente), Quaestio LXXVIII discusses usuries in loan transactions (De vitio usurae in mutuis).

15. E.g. in Buridan's *Comments to Aristotle's Politics* (In VIII libros Politicorum Aristotelis, lib. I, quaest, XI: Utrum mutatio et commutatio monetarum in policia bene recta sint licite).

16. Besides the above mentioned works by Heinrich von Langenstein and Gabriel Biel, e.g. Petrus Olivi (1248-1298), *Quaestiones de permutatione rerum, de emptionibus et venditionibus*; Alexander Bonini (the beginning of the 14th century), *Tractatus de usuris*; Lorenzo di Antonio Ridolfi (1360-1442), *De usuris*; Johannes Nider (c. 1380-1438), *De contractibus mercatorum*; Konrad Summenhart von Tübingen (1465-1511), *Tractatus de contractibus*; Thomas de Vio (Cajetanus) (1468-1534), *De usura, De cambiis*; Martin Azplicueta Navarrus (1493-1586), *Commentarius resolutivus de cambiis*.

17. E.g. Thomas Aquinas, *De mixtione elementorum, De occultis operationibus naturae, De motu cordis, De emptione et venditione ad tempus.*

18. E.g. Antoninus Florentinus (1389-1459), *Summa Moralis Theologiae*, Luís Molina (1536-1600), *De Justitia et Jure;* Leonard Lessius (1554-1623), *De Justitia et Jure.* These works as well as the above mentioned ones reflect, in several respects, the economic relations emerging in Europe and particularly in its most developed areas in the 15th and 16th century. The phenomena of economic life in Northern Italy, the Spanish Peninsula, France and the Netherlands, the problems arising from the increasing role of monetary policy, financial capital, banking, and from the incipient process of capitalization, exert an ever strengthening impact on the gradual transformation of the ever so traditional conceptual framework.

19. See C. Johnson: *The De moneta of Nicolas Oresme and English Mint Documents.* London, 1956. Introduction, p. XVII; Cf. M. Clagett, "Nicole Oresme and the Medieval Geometry of Qualities and Motions", p. 647

20. Cf. Antal Mátyás, *A polgári közgazdaságtan története.* [The History of Bourgeois Economics]. Közgazdasági és Jogi Publishing House, Budapest, 1963, p. 14

21. K. Marx, *Capital*, Vol.I. Progress Publishers, Moscow, 1954, p. 94, Note 1.

22. Paulus, the famous Roman lawyer writes about money in Digesta: "Origo emendi vendendique a permutationibus coepit. Olim enim non ita erat nummus neque aliud merx, aliud pretium vocabatur, sed unusquisque secundum necessitatem temporum ac rerum utilibus inutilia permutabat, quando plerumque evenit, ut quod alteri superest, alteri desit. Sed quia non semper, nec facile concurrebat, ut, cum tu haberes, quod ego desiderarem, invicem haberem, quod tu accipere velles, electa materia est, cuius publica

ac perpetua aestimatio difficultatibus permutationum aequalitate quantitatis subveniret. Eaque materia forma publica percussa usum dominiumque non tam ex substantia praebet, quam ex quantitate, nec ultra merx utrumque, sed alterum pretium vocatur". (D. 18.1.1.pr. Paulus) Quoted in: Róbert Brósz-Elemér Pólay, *A római jog* [The Roman Law]. Tankönyvkiadó, Budapest 1976^2, p. 420. - Thomas Aquinas calls money a measure determined by law in his comments to Aristotle's Ethics (lib. V. lect. IX.): "Et inde est quod denarius vocatur numisma: *nomos* enim lex est, quia scilicet denarius non est mensura per naturam, sed *nomo*, id est a lege: est enim potestate nostra transmutare denarios et reddere eos inutiles".

23. H. Estrup, *Op. cit.* pp. 98-99; 100-101; 116.

24. About these problems and the debates around them, see H. Estrup: *op. cit.*, pp. 104-105. Cf.: J.A. Schumpeter, *History of Economic Analysis*. New York, 1954, p. 343.

25. In Oresme's opinion, the difference between the tyrant and the monarch is that the tyrant wants to be more powerful than the community which he rules by force; while royal moderation is characterized by the fact that it is more powerful and bigger than any of its subjects, but in terms of strength and riches, it is yet smaller than the community as a whole, and thus it takes the middle place between the individual and the whole.

    Oresme clearly sees the danger of the arbitrary growing tendency of royal power, therefore he emphasizes the need for close control of this power: "But because the king's power commonly and easily tends to increase, the greatest care and constant watchfulness must be used, indeed extreme and supreme prudence is needed to keep it from degenerating into tyranny, especially because of deceitful flatterers who have always, as Aristotle says, urged princes to be tyrants." (Ch. XXV.)
    In order to keep the kingdom lasting and not to let it

degenerate into tyranny, this rule must be followed: "That is that the prince should not enlarge his dominion over his subjects, should not overtax them or seize their goods, should allow or grant them liberties and should not interfere with them or use his plenary powers but only a power regulated and limited by law and custom. For few things, as Aristotle says, should be left to the decision of a judge or a prince." (Ch. XXV.)

Finally, Oresme refers to the harmful consequences of a tyrannic rule which endanger the future of the royal house and the whole kingdom (Ch. XXVI.), and in an elevated style, he invokes Gallic national consciousness against tyranny: "God forbid that the free hearts of Frenchmen should have so degenerated that they should willingly become slaves; and therefore a slavery thrust upon them cannot last. For, though the power of tyrants is great, it does violence to the free hearts of subjects and is of no avail against foreigners. Whoever, therefore, should in any way induce the lords of France to such tyrannical government, would expose the realm to great danger and pave the way to its end. For neither has the noble offspring of the French king learned to be tyrannous, nor the people of Gaul to be servile; therefore if the royal house decline from its ancient virtue, it will certainly lose the kingdom". (Ch. XXVI.)

26. Edgar Schorer, *Nicolaus Oresme "Traktat über Geldabwertungen"*. Verlag Gustav Fischer, Jena 1937, p. 26

27. Jenő Szücs, *Nemzet és történelem* [Nation and History]. Gondolat Publishing House, Budapest 1974, p. 453

28. *Ibid*, pp. 456-509

29. Jeannine Quillet, *L'Aristotelisme de Marsile de Padoue*. Miscellanea Mediaevalia 2. Die Metaphysik im Mittelalter. Berlin 1963, pp. 696-706, especially pp. 702-703.

JÁNOS SIPOS

ON THE MATERIALISTIC APPROACH TO THE PSYCHE AND PROBLEMS OF PSYCHOPHYSIOLOGY

In modern psychology, *materialism* generally occupies a dominant position. Traditional spiritualistic views and the idealistic "phsychology of consciousness", which attribute conscious functions to a supposed immaterial "spirit", have mostly been excluded from the science of psychology. Today, the main question is what type of materialism it is that works on the different trends of psychology and what aspects rule it.

At present, a *materialistic monist* approach is the most characteristic trend in psychology, that is to say, the view that psychic functions - including their higher forms - are functions of the brain, special physical processes, or, in a wider sense, part of the only real world, the material world. This much is expressed by the behaviourist Donald Hebb, for example, when outlining his "monistic" approach, according to which starting point the psyche is a "physical process", "the functioning of the brain or part of it". This is also Rubinstein and Leontiev's standpoint, who both share the philosophical background of dialectical materialism. This view opposes all dualistic approaches, and is consistently monist, considering the psyche to be something which fits organically into the universal coherence of the material world's phenomena, something which is not only determined by its environment but is also determinative in itself, directly defining and ruling the responding activity of the organism. Rubinstein stresses the fact that, in the general ontological sense, psychic activity appears to be the material activity of a material organ, realizing the interaction between the organism and the outside world. According to Leontiev, the psyche is a definite form of vital processes, a form of material interaction between the material individual and the material environment; psychic reflection is a result of the actual mutual connection between the living material individual and the surrounding material reality; psychic processes are parts, derivates and mediators of the material individual's outward practical activity, and material processes realize the interaction between the material individual and material reality.

However, within the overall monistic view of modern materialist psychology, there are fundamentally important differences. The crisis in psychology at the turn of the century had strong vulgar-materialistic tendencies. Behaviourism, in opposition to spiritualistic, introspective "psychology of consciousness", gave up the study of the inextricable "mind". Watson emphasised the need to turn attention to observable behaviour. The above-mentioned D. Hebb, a representative of this particular theory himself, calls it "physiological or mechanistic". Its basis is the supposition that "soul" does not exist and everything can be explained in physiological terms. Behaviourists used Pavlov's results in the study of reflex activity. They believed human behaviour to be conditioned reflex activity but, unlike the Sechenov-Pavlov school of thought, they excluded the "middle phase" of the reflex process, i.e. they discarded psychic fact as idealistic nonsense. The study of these reflex mechanisms, entwined with current psychological tendencies, has formed the neurological trend in psychology which interprets the leading processes in objectively studiable behaviour as mere neuro-physiological processes. The *reduction* of psychic to neuro-physiologic is widespread in contemporary psychological literature. However, though this standpoint is really materialistic, the complete identification of psychic processes with those of neuro-physiology - although necessary to some degree - results in the disappearance of psychic processes themselves.

This neuro-physiological reduction, its ackwardness and insolvable contradictions are also felt by its adherents. It theoretically eliminates the psyche from its study, but, in practice(if approaching from the angle of outward behaviour), it still tries to find an explanation for psychic phenomena. Hebb, while stressing the revolutionary significance of behaviourism, reflexology and neurology, still comes to the conclusion that this theory is inadequate, as it neglects the psychic processes mediating between stimulus and response. He himself stresses that mediatory psychic processes and the psychic functions realized by them (thinking, comprehension, etc.) are different: the physiological approach or "neurologization", as Hebb calls it, has a "more molecular" tendency, is "more refined", and "is concerned with the units rather than the whole". So, a psychological approach with units on a larger scale is necessary.

Materialistic psychology based on *Marxism* has the advantage that, from the beginning, it pays great attention to the individual's social determination, social nature and changes of form. In other words, due to its historical approach, basic social orientation and its program of understanding man's social nature, Marxist psychology cannot be restricted a priori to studying psychic phenomena solely in connection with neural processes. It has had to take into consideration the fact that psychic processes, as brain processes, are different from the neurological, physiological mechanisms from which, nevertheless, they cannot be separated. They obviously reflect the surrounding world and this "picturing" changes along with the changes in the level and social position of man's social practice. After the development of the Vigotsky-Leontiev historical genetic approach, it has gradually become clear in dialectical materialistic psychology that the psyche is a relatively independent phenomenon with its own typical structural qualities, efficient mechanisms and laws of motion.

In Marxist psychology, too, it is basically important to explore the way in which psychic processes are tied to mental processes and to study the neuro-physiological-mechanisms carrying them. But, in contrast to other "physiologizing" efforts, Marxist psychology draws significant attention to other interactions in psychic phenomena. The psyche's "reflecting" nature is studied with special attention: that is, the fact that psychic processes are a part of the brain's operation which is basically determined by outside reality, as the psyche has a reflex nature. Following Rubinstein's analysis, it is the brain's response activity, through which the outside world is reflected. This determination is not only manifested in actual, everyday reflexes but also phylogenetically and ontogenetically (morphological and functional changes in the evolutionary organs responsible for adaptation and the transformation of reflex-like life functions, as well as the development of the individual's psychic abilities, all evolve in interaction with the environment, under its decisive influence). Marxist psychology at the same time discloses the *active* character of psychic processes, highlighting the fact that these are a way by which the living organism actively adapts to its essential conditions. In addition, Marxist psychology considers the influences in a changing environment which are asserted through the mediation of the organism's programmed activity, itself operating via an innate evolutionary programme (N.A. Bernstein). The living organism's psychic functioning mediates outside stimuli, then actively analyses, transforms and prepares these stimuli, and, on the basis of former experience, forms a response. With man, this active process takes the form of social practice and this practice mediates the effect of the outside world: as Engels stresses, this is why practice has become the primary element in cognition. Consequently, psychic functioning, as the element of active interaction between the organism and the environment, is determined by both environmental conditions and itself: it forms and rules the organism's responses. This ruling, or *directing function*, is the psyche's fundamental function, inseparably intertwined with its function of reflecting the environment. The psyche's reflecting and directing function in man has a characteristic, consciously volitional form which includes the development of the activity's theoretical plan.

Marxist based psychology pays scrupulous attention to studying and understanding the *social nature* of the human psyche through highlighting the fact that the peculiarities of human psychic functions, abilities, actions, and their products have their origin in the human psyche's links with the individual's social way of life. The human psyche, like the typical life functions of which it is the reflecting, directing part, is a process which is instrumental, mediated by tools and socially interpreted. Man is able to rule his environment and direct his own psychic processes with the help of those modes and instruments, including linguistic tools. These tools and forms of psychic activity are called into existence by man's social way of life and they are socially stored and inherited. Man's higher psychic functioning is founded beyond his biological organism, in his essential social life conditions (Luria).

The modern materialistic approach, breaking away from simplified naturalistic and "physiological" explanations, necessarily focuses on the peculiarities and complexity of the human psyche, as well as on its organic connection to, and relative independence from, neuro-physiological processes. However, special difficulties of interpretation arise in the dialectical materialistic approach which also disturb the development of scientific psychology.

In my opinion, the dualistic solutions found in Marxist philosophical and psychological literature may act as a kind of defence against "vulgar materialistic" mistakes and especially "physiologization". It was not only Mario Bunge who criticized these dualistic tendencies, but S.L. Rubinstein, and others, working is Socialist countries also voiced such opinions. Rubinstein highlighted the error implicit in the idea that mind, through the nature and essence of the psyche opposes the material in all aspects and is, thus, excluded from the phenomena of the material world. This error was rooted in the misinterpretation of a contrast between the mind's reflecting nature and objective reality, a contrast which is limited to the field of the theory of knowledge. As a result of theoretical developments from the late 1950's on, first in the Soviet Union, and then in the other Socialist countries (Hungary among them), the complex analysis of consciousness and being became accepted, emphasizing both their gnosiological and ontological aspects, as well as the unity of consciousness and matter, at least in that consciousness has its origin in matter and is materially defined.

International Marxist philosophical literature, however, has not yet come to apply materialistic monism consistently. I have just mentioned the significant role "vulgar materialistic" physiological reductionism has played. But, in the recent break-down of the process of clarification, a similarly large part is played by strengthened "ontologizing" efforts and ideas which are strongly connected with "praxis philosophy" and "social ontology". These tendencies move stress to the mind's ontological position and its objective social role through weakening or eliminating its reflecting, secondary nature and its dependence on vital social conditions. In the last analysis,they renew subjectivist social philosophies, stressing that the conscious, psychic factor is the basic and independent determiner of social movement and denying that objective necessities and laws control social processes.

The characteristic standpoint may be traced back to this view, which rejects the full exposition of the ontological aspect beside the epistemological. In this framework, contrary to materialist monism, the consideration of the psyche, at a general ontological level, as a materialistic phenomenon, as a real, existing part-process of the only material world, with a particular structure and parameters, is shown to be false. The following theoretical viewpoint takes shape at this theoretical level: the consciousness, from an ontological angle, is only material in its origin and determination, but, with regard to its nature it is "immaterial". As it is generally argued, this is because its crucial characteristic is its reflecting nature, and thus it necessarily and essentially opposes objective reality in all respects, being only its subjective reflection.

It seems, then, that this standpoint makes the same mistake Rubinstein warned us about *expressis verbis* 25 years ago, and to which Lenin drew the materialists' attention more than 70 years ago: it "absolutizes" and overstrains the opposition between the material and the mind, which only exists in a narrow sphere within the theory of knowledge. The opposition is transmitted to the level of ontological problems, to the general ontological relationship where the psyche, as a real existing phenomenon, appears as a "particle of nature" (Lenin), as a part-process in a material world which was thought "unique" by materialists, "besides which there is nothing else". To make matters worse, they contrast psychic phenomena to "material" phenomena, using "vulgar materialistic", and "physicist" arguments in a way which identifies the "material" with "physical" or "bodily" things. They state that a thought, in contrast to the above is imperceptible, intangible, it has no dimension, no weight, no colour, etc.

The fear of "vulgar materialism" and "ontologization" results in a blurring of the real problem in the greater part of contemporary Marxist philosophical literature. As a matter of fact, the fault of these trends at a theoretical level is not that they consider the psyche as something material in the general ontological sense (that is, as no part of a special supernatural mental world and not merely a subjective phenomenon). In my opinion, the fault lies in the fact that the "ontologizing" standpoint rejects the mind's secondariness, its reflecting nature, because it emphasises its objective existence, and makes consciousness the final determiner of social processes. Contemporary vulgar materialism, on the other hand, interprets the material and objective reality of psychic phenomena by identifying these phenomena with neuro-physiological processes. In this way, it eliminates "the psychic" from the processes of reality.

I only wish to discuss this latter problem here. I have to reject the rather widespread argument that the psyche, although something which really *exists*, is not material (in a general ontological sense) because it is "imperceptible", "invisible" and has "no extensity", etc, thus differing from the cerebral cortex and neuro-physiological processes. This rakes up an extremely vulgar idea of materiality and involuntarily leads back to a kind of dualism. It also leads to a situation where such hidden relations, inaccessible to the organs of sense, are automatically eliminated from material reality. In addition, this method makes a further coarse methodological mistake in that it confuses the *carried* process with the *carrying* process, misunderstanding that psychic processes represent a different level of mental activity from neuro-physiological, biochemical or bio-electronic neuro-mechanisms. The reason for this misunderstanding lies in that this argument wants to see the characteristics of neuro-physiological processes in psychic processes, and their absence leads the elimination of psychic phenomena from the material sphere. Within the sphere of carrying processes, carried phenomena always remain "invisible": life, for example is imperceptible at the level of chemical processes; and social structures and dynamics cannot be perceived at the level of individual motion either. The more complicated organizational spheres of reality have their *own* structural peculiarities, specific interactions and kinetic laws, which cannot be explored from lower, more involved spheres.

Neuro-physiological processes are carrying processes which do not determine either the contents or the structure of psychic processes. Receptors as stimulus collecting and transforming mechanisms, the transmission of electro-chemical impulses by neurons, the complex mental physiological mechanisms formed from these simple processes, the spread of the stimuli and the different forms of inhibitions - including differentiating inhibitions, - do not give us any information about the psychic processes they carry. The human brain produces rich, special psychic processes, for example, the speaking and thinking process, the abstract, notional reflection of reality's significant connections, the conscious control of activity, and new forms of perception and memory. These typical psychic forms are realized and carried by similar neuro-physiological mechanisms, which are the basic impulse generating and transmitting mechanisms of, for example, the neurons. The same mechanisms produce the simpler psychic processes of animals. Human speaking itself is an activity combining exceedingly different sign systems, but different languages - for example, English or Hungarian -, with their different vocabularies and grammars, are realized by the same neural and cerebral, etc., processes. Similarly, historically developing forms of logic are obviously independent of the neuro-physiological mechanims carrying logical processes. The same is true of the various ways of thinking among people living in different historical ages or belonging to different social layers: this typical, independent structure, and the characteristics of the ways of thinking, do not depend on the neuro-physiological mechanisms, as these remain identical and unchanging from one person to another. The roots of the antagonism between the collectivistic and the individualistic ways of thinking are social and not neuro-physiological, and changes in the idea of existence arise via unchanged brain mechanisms, obeying the impulses of the typical, relatively independent, laws of psychic processes existing under social conditions.

The development of *cybernetics* greatly helps us understand the mistake of the psyche's neuro-physiological reduction. Results in the information theory are especially important, as the higher forms of psychic operation belong to information phenomena. The information processes - studied in cybernetics - are processes of reflection and control, and it is in this that their speciality lies. These differ fundamentally from the processes studied by physics, chemistry and biology. Experts mostly agree that information phenomena represent a special type of material process. In these phenomena, the "materials" transmitted or transformed, are not physical, chemical or biochemical, since they are not primarily energy processes. The brain's function, for example, differs from that of the liver or the muscles: its typical function is not to produce some secretion, or to perform the body's movement, but to direct and regulate the movement and behaviour of the whole organism. The nervous system is an information processing machine or, more accurately, a governing apparatus.

All information processes are alike in that they connect defined,physical, chemical and bio-chemical "currents of material" or energy processes. The input, storage, transmission and processing of information cannot occur without energy, or defined physical or chemical processes. Transmission is impossible without a carrier which mediates the information and functions as its sign. Electric processes or defined groups of neuro-impulses carry the information, whose transmission and processing consume energy. But the information process *itself* belongs to a *different type*. Its characteristic feature is, as stressed in cybernetics, that it directs, and is able to direct, high energy processes with a small amount of energy. In consequence, the *carried* nature of informational processes is far more striking. It can be said that all processes in the material world are built upon other processes which carry and mediate them, and, without which, they would not exist. Special emphasis must be laid here on the relationship itself of *being carried*, as a crucial problem in examining the reduction of the psychic level to the neuro-physiological process carrying it. Cybernetic literature is unanimous in declaring that the nature of information processes differs completely from those processes carrying them. It also stresses that the same transmitting process may be realized by more carrying mechanisms and that the structure and contents of the carried information cannot be found in either the computer's hardware or the brain's neuro-dynamic structure.

Psychological sciences themselves can be relatively independent, due to the independent structure of psychic processes and their difference from the neuro-physiological processes bearing them (and from which they are inseparable). Perception, sensory experience, memory, imagination, thinking, speaking, attention, emotions and volition all represent a different level in a process. Their laws can be studied independently, without knowledge of their carrying neuro-physiological, or, even deeper, their bio-chemical, physical processes. Today, it is also known that new forms of psychic activity influence the carrying neuro-physiological mechanisms, re-organize them and lead to the the development of dynamic, new brain formations. The other side of this relationship is that after certain brain injuries, the re-establishment of psychic functions occurs along with the development of new brain organizations. The discovery of dynamic, functional organs in the brain strengthens the conviction that psychic functions themselves are the workings of defined brain organs and that these psychic functions of reflection and control transform and react upon the brain structures carrying them. They do so through transforming the organism's life activity and social practice, and generate new carrying formations, this being a function of the transformation.

This objectively existing, relative independence of psychic processes was the epistemological basis for the first naive materialistic theories. These theories could not functionally interpret the psyche and considered "spirit" a separate "thing" in some physical sense. The concept of ancient atomism was also extended to fit it. This independent structuralization and motion forms the basis of a spreading classical animistic spiritualistic theory which sets "spirit" against all other phenomena in an absolute way, tearing it away from unique material reality, from the human body and the brain itself, which becomes merely the tool of immaterial "spirit". The modernized spiritualistic theory, forced to adapt itself to the results of contemporary science, does not believe the psyche to be one of the organism's life-activities, the function regulating the interaction between the organism and the environment. Neither does this theory believe the brain to be the mechanism realizing psychic reflection and control. In this theory, "spirit" is a separated, substantially interpreted, "immaterial" thing, independent from the brain. It is able to leave the body and only *uses* the brain in its autonomous activity as a telegraphist uses the telegraph set to enforce his autonomous will.

Because of the continuing, harmful influence of overtly or covertly spiritualistic and dualistic theories, in addition to vulgar materialistic and "ontologizing" distortions, it is important to reiterate the general methodological principles of the scientific study of psychic processes. A complex, many-sided approach is thought to be most needed in order to understand the *"particularity"* of these processes. Besides the study of the neuro-physiological mechanisms carrying them, we must analyse these processes from the viewpoint of their environmental, social determiners. From this, we must also explore their structural, dynamic characteristics, reflecting, controlling functions, active nature and their new, higher forms in man. We must be able to correctly employ the materialistic approach in the study of the psyche. We must understand that the relative independence and difference of psychic processes from the neuro-physiologic carrying processes, their reflecting nature and their sharing the characteristics of "ideas", can never account for their opposition to "material" processes at a general ontological level. Psychic processes, as typically complex, relatively independent processes with their own parameters and structure, particular laws and objective regularities, not forgetting their reflecting controlling function, are real processes in the only material reality in the same way as the neuro-physiologic processes carrying them, the environmental processes producing and defining them, or the social processes defining the changing forms and contents of human psychic functions.

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THE STRUCTURALIST VIEW ON EQUILIBRIUM THERMODYNAMICS

This paper has three purposes: (1) to analyse Kirchhoff's paradox in equilibrium thermodynamics, (2) to compare the various foundations of thermodynamics, and (3) using the method developed by J. D. Sneed, to give a reconstruction of Carathéodory-Landsberg's thermodynamics.[1]

1. *Introductory remarks*

1.1 Two foundations of thermodynamics

As is well known, Clausius, Kelvin, Rankine etc. reconciling Carnot's principle with the work of Joule and Mayer founded a new theory of heat in the early 1850's by means of the concepts of energy, entropy, and absolute (or Kelvin) temperature. The investigations in the kinetic theory of gases actually accomplished by Clausius and Kelvin to establish these concepts were not involved in the standard formulations of the postulates of the theory. It was not inaptly called "thermodynamics of cycles" by Tisza, because the applications of the method of Carnot cycles, the only technique that was as yet available, yielded abundant results without making unwarranted assumptions about the structure of matter. But in this artifice, all statements were in terms of reservoirs and other idealised macroscopic devices. This procedure, in which the system was hardly more than a "black box", became more a hindrance than a help to a direct study of physical systems. The first adequate method for this structural problem was developed by Gibbs (1902).

In Gibbs' theory, attention is focused on the system. Laying the theoretical foundations of his method, Gibbs built in the process an impressive theoretical structure. It is universally recognised that the publication of his papers on thermodynamics was an event of first importance in the history of chemistry, that in fact they founded a new branch of chemical science which, in the words of M. Le Chatelier, was becoming comparable in importance with that created by Lavoisier.

In contrast to Gibbs' theory, we speak of Carathéodory's theory of thermodynamics which is a mathematical formulation of the theoretical structure built by Clausius and Kelvin.[2] It sought to replace thermodynamic arguments based on Carnot cycles by a differential geometry dealing with Pfaffian differential forms. Both Gibbs and Carathéodory had a common purpose to eliminate the concepts that arose from the practice of heat engines but the idea of reversibility was adopted in their theories.

At first sight, the difference between the two theories is that Charathéodory's theory is an axiomatic approach and Gibbs' theory is a phenomenological one. But if we adopt Prigogine's classification of thermodynamical systems, then we can also say that Carathéodory's theory deals with isolated systems and Gibbs' theory deals with closed systems. Both theories are principally interested in reversible (or quasistatic) processes and they indirectly deal with irreversible processes. The reason for this is that a systematic investigation of irreversible processes was only begun by the pbulication of Onsager's paper (1931). Since that time, it has been the usual practice to divide thermodynamics into two disciplines: equilibrium and non-equilibrium thermodynamics. Although it has been suggested that Carathéodory's theory may eventually provide a better mathematical basis for a theory of non-equilibrium thermodynamics, for example by Eckart (1940), most of the recent theories are based on Gibbs' theory.

As for equilibrium thermodynamics, its central problems are: (1) the definition and proof of the existence of entropy and absolute temperature, and (2) the parametrization of equilibrium, that is, the development of a mathematical formalism by which equilibrium properties of thermodynamic systems can be characterised. Although both Carathéodory's and Gibbs' theories have some ideas and concepts in common, the first theory is concerned primarily with the axiomatization of (1) along the lines initiated by Clausius and Kelvin "without recourse to any hypothesis that cannot be verified experimentally", while the second one is directed to (2) using the concepts of energy, entropy, and absolute temperature as primitive ones.

In order to point out a characteristic feature in which Carathéodory's theory differs from that of Gibbs, it is convenient to compare the geometrical methods utilised by each. These methods are based on a "thermodynamic phase space". In Carathéodory's theory, the phase space is spanned by all the "independent state variables". The only criterion is that the variables must be directly measurable quantities. Beyond this requirement, however, no specific choice of the phase space is made; in particular, no mathematical distinction is made between extensive and intensive thermodynamic variables. This was first pointed out by Ehrenfest (1911), who recognised that the precise distinction between them requires additional axioms to those contained in Carathéodory's theory.

In this paper, we will consider a new modification of Carathéodory's approach to equilibrium thermodynamics developed by Landsberg (1956), (1961) and Buchdahl (1966). In this theory, the existence of an entropy function and an absolute temperature is inferred from the consideration of quasistatic adiabatic processes. Entropy is obtained through the integration of a Pfaffian differential equation and it is represented by a one parameter family of surfaces in the phase space. The monotonity or "one-way" property of entropy is deduced from the consideration of non-static adiabatic processes. Since Carathéodory's theory is quite insensitive to the specific choice of phase space, there is no way to obtain the characteristic potentials that are associated with particular thermodynamic spaces.

In contrast, in Gibbs' theory, a particular phase space - the Gibbs space - is spanned by all the independent extensive variables. The exact mathematical foundations of Gibbs' geometrisation of his thermodynamics was accomplished by Blaschke (1923), who showed that the geometry of the Gibbs space is an affine defferential geometry. In this theory all the thermodynamic properties of the system are supposed to be contained in a fundamental equation representing entropy (or energy) as a function of the additive invariants. Geometrically, this equation is represented as a surface in the Gibbs space; it is called the primitive or phase surface. The phases of thermodynamic systems are each represented by a primitive surface. A system of given chemical composition exists potentially in a number of phases, each of which is specified by a primitive fundamental equation. The actual distribution of the additive invariants of a system over phases is determined by the entropy maximum principle. Gibbs' criteria of stability may be obtained from an analysis of the curvature of the primitive surfaces.

1.2 The problem of equilibrium; Kirchoff's paradox

Among the most important concepts of thermodynamics is that of equilibrium, an elusive concept - in the words of Tisza[3] -, since there is no purely empirical method to establish whether or not a given system has actually reached equilibrium. For it might well be the case that an experimental investigation of a given system does not reveal any measurable changes during the time required to carry out the experiments; but this is not to say that a change might not have been observed if the investigation had extended over a greater interval of time. It is, however, experimentally observed that isolated systems generally tend to evolve spontaneously toward simple terminal states. Moreover, it is natural that a physical theory should first attempt to treat the properties associated with the simplest states of a system rather than to attack all possible complicated states in a completely general way. But, of course, a fundamental question is still with us: "What are the simplest states of a system?" Since the observation of "whether the state of interest is static and quiescent" cannot be considered as the absolute empirical criterion, we have to look for another one for greater simplicity. A very general formal criterion of simplicity is the possibility of describing the system in these states - the so-called equilibrium states - in terms of a finite number of variables. In order to rephrase the description of equilibrium in a manner that will provide a basis for further theoretical development, it is necessary to postulate the existence of equilibrium states and to assume that it is always possible to give a finite number of independent parameters which define the equilibrium states of a given system uniquely. These parameters or variables, which enter into the specification of an equilibrium state, are called state or thermodynamic variables. Of course, unless equilibrium obtains, these variables are not defined, in the sense that experiment does not yield unique values which might be assigned to them. Accordingly, it is meaningless to speak of a "non-equilibrium state": a system not in equilibrium is simply in no state at all.

By regarding the state variables of a system as coordinate axes of a finite dimensional space, one can picture an (equilibrium) state of a system as represented by a point in the appropriate phase space. Further specification of thermodynamic variables leads to the different foundations of thermodynamics.

We now call attention to a methodological problem which was first objected by Kirchhoff to Planck's early writings on thermodynamics. We will call this problem Kirchoff's paradox.

In any theory of equilibrium thermodynamics, the determination of the conditions of equilibrium is made in the cases of isolated systems. But there are two possibilities: (1) the system is in equilibrium, or (2) it is not in equilibrium. In case (1) it is no longer possible to determine the conditions of equilibrium. In case (2) there is the problem of defining a thermodynamic potential for non-equilibrium states, but it is not a matter for equilibrium thermodynamics. That is, how are we to make a statement like "the entropy of an isolated system tends toward a maximum", whereas the entropy concept can be applied only for systems in equilibrium? To resolve this difficulty Carathéodory introduced the ingenious idea of "composite systems", which was later carried over into Gibbsian thermodynamics. However, the systematic incorporation of this concept into the foundations of equilibrium thermodynamics necessitated a considerable revision of the classical conceptual framework. In order to achieve this in a logically satisfactory manner, it was necessary to introduce some devices endowed with extraordinary properties. These devices are called partitions or walls. They are essential for what might be called the "basic trick" in thermodynamic arguments. It is only by virtue of this basic trick that we can convert the nón-equilibrium situation to an equilibrium situation and, in particular, we can talk about the entropy of non-equilibrium states of a system.

The assumption of the existence of walls or partitions is fundamental for equilibrium thermodynamics. They separate a given system from its surroundings and provide its boundary conditions, or divide the isolated system into a number of subsystems. It is by means of manipulations of the walls that the thermodynamic parameters of the system are altered and certain processes are initiated. Thus, in this approach, the notion of specific thermodynamical processes is deduced from the notion of specific walls. Of course, the converse reasoning is equally good. If the notion of a certain type of process is given, one can easily define the appropriate type of wall.

As it was pointed out by Landsberg (1956), if a system in equilibrium is thermodynamically fully defined in terms of a finite number of independent variables, then a wall can be considered as one which forces one new relation on all the variables involved. Thus, from the mathematical point of view, a wall and a condition of equilibrium which establishes itself across it - i.e. the relation of "thermodynamic interaction" - can be characterised so that there shall exist a unique function of the variables of the systems separated by it such that the value of this function for the final equilibrium states of subsystems is zero. If the sequents of state variables of two subsystems are denoted by $\bar{x}_1$, $\bar{x}_2$ and the number of independent variables for these systems are n_1, n_2 respectively, then the required function, say $F_{12}(\bar{x}_1,\bar{x}_2)$, is a function of $n_1 + n_2$ independent variables. We can regard this function as having physical meaning only for certain values of the $n_1 + n_2$ parameters - those values in fact which occur in the equilibrium conditions $F_{ij}(\bar{x}_i, \bar{x}_j) = 0$. For any two systems, this relation is defined experimentally. According to Carathéodory, "In every particular case it is necessary to *define* what is meant by the various expressions used. This is done experimentally by establishing the form of an equation of type /$F_{ij}(\bar{x}_i, \bar{x}_j) = 0$/ that describes the thermodynamic properties of the wall under consideration. ... The discovery fo all relations of this type constitutes one of the principal problems of experimental thermodynamics."[4]

1.3 The notion of composite systems

Let attention be then turned to the problem of the definition of walls and of composite systems in axiomatic approaches to thermodynamics.

As we want to carry along with the idea of a physical system the assumption that it can be modified within certain defined limits, i.e. a system has to be regarded together with its domain of permissible changes or, in other words, of permissible processes which can take place in this system, we need the notion of processes as primitive. Thus, the next methodological problem is the classification of the possible processes. The simplest solution is to introduce some basic types of processes as primitives. Another solution is to define the different types of processes in terms of given types of walls. In this case, of course, the notion of walls is considered primitive.

The processes arising by manipulations of the walls are generally associated with a redistribution of some quantity among various systems or among various portions of a single system. Thus, a formal classification of different types of walls would be based accordingly on the property of the walls in allowing or disallowing such redistributions. It is clear that this classification depends on which quantities are chosen. The general definition of the walls would be based on the concept of energy. Energy can be transferred to several modes of a system. For example, it can be transferred to a mechanical mode, an electrical mode, or a chemical mode of a system etc., such fluxes of energy being called mechanical work, electrical work, and chemical work etc. The different types of energy transfer can be typified by the terms treated fully in the standard theories of physics references. Thermodynamics, from this point of view, is concerned with the energy transfer to the hidden atomic modes of motion which is called heat. At this point, however, there is the problem of founding thermodynamics without knowing a heat term.

First, we will consider the formulation of Carathéodory's axiomatic approach given by Lansberg and Buchdahl. Let us refer to this theory as C.L.T. Carathéodory's theory specifies only two types of walls more closely: adiabatic and diatermic walls. That is, in this theory, the interactions between two systems are dichotomized into two types that are known, respectively, as *pure work interaction* and *pure heat interactions*. The essential characteristic of the presentation given by Carathéodory is "that the concepts 'adiabatic' and 'adiabatically indulated' are defined with the aid of physical properties and are not, as is usual, reduced to the concept of energy"[5]. However, the definition of adiabatic walls in terms of "impermeability of heat" implies that the concept of heat has been defined. Hence the definition of the "amount of heat" in terms of adiabatic changes (or processes) involves apparently logical circularity. To avoid this circularity Landsberg introduces the notion of the adiabatic process in the restrected sense. The essence in this artifice is introducing the notion of restricted adiabatic process as a primitive one. The definition of this concept is not an explicit definition, it is rather used to demonstrate that this concept may operationally be defined. His main object of introducing restricted adiabatic changes is to prove the existence of an "internal energy" function in terms of mechanical quantities only. Then, having this knowledge, it may generally be supposed that there exist forms of energy other than mechanical, and we have full knowledge of the properties of all forms of energy, except just one, namely the form called heat. Thus, when Landsberg introduces the general notion of energy and states the First Law in full, he has to use the concept of adiabatic processes as another primitive one. Consequently, the underlying theory of C.L.T. consists of the "theory of adiabatically interacting systems" referred to as T.A.I.S. For simplicity, we will use the concept of adiabatic processes for the structuralist reconstruction of T.A.I.S. and, by the above remarks, we will refer to it as non-theoretical.

The starting point of T.A.I.S. is assuming the existence of empirical methods to determine if a given system is in equilibrium. That is, we assume that a given system in equilibrium is macroscopically fully defined by means of a finite number, say n, of independent physical quantities known from other parts of physics. As a matter of convenience, bearing the aim of simplicity constantly in mind, a further restriction will be placed for the time being on the kind of systems to be contemplated, which relates to the character of its coordinates. Explicitly, all independent state variables of this sort of systems, called *standard-systems, except for one*, must depend on the external shape of the given system. The quantity that determines the external shape will be called *deformation variable*. As a consequence of this restriction, a standard system cannot have any internal partitions which adiabatically isolate parts of it from each other. From the mathematical point of view, this amounts to saying that there exists a function from the set of all states of the system to the n-dimensional Euclidean space, where $n \geq 2$. The condition $n \geq 2$ for the dimension of Euclidean space is necessary, because in the case $n = 1$ the equilibrium would be reduced to a special kind of equilibrium which is discussed in another branch of physics.

In T.A.I.S., in addition to the notion of (equilibrium) states and of independent variables, we will use the general notion of processes as a non-theoretical primitive one. Moreover, it is possible to single out a special class of processes, namely *quasistatic processes*, which satisfy the additional requirement that the work done in a quasistatic process is done precisely by the forces which hold the system in equilibrium. Since a quasistatic process has, as a sequence of (equilibrium) states, a reasonably clear intuitive definition, the concept of quasistatic processes may be regarded as a primitive non-theoretical one. Hence in T.A.I.S. the concepts of (equilibrium) states, state variables, permissible processes, adiabatic processes, quasistatic processes, and work are regarded as primitive non-theoretical ones. That is to say, T.A.I.S. is a special theory in the sense that it is exposed without the use of theoretical terms. As the underlying theory of C.L.T., this theory has the other feature that there is no need to introduce constraints, i.e. the notion of composite systems.

The Carathéodory-Landsberg theory is considered as a theoretization of T.A.I.S., where the new component is the internal energy function. For the formulations of the constraints we will use the concept of diatermic walls as a non-theoretical primitive. In C.L.T., the primary object of using composite systems is to proceed to the concepts of absolute temperature and entropy. The mathematical basis of this procedure is the Frobenius Integration Theorem.

Let us now consider the use of composite systems in Gibbsian thermodynamics. The motivation for the use of composite systems in this theory is the same as in Carathéodory's theory. Composite systems, together with the thermodynamic processes of the redistribution of additive invariants, are used to determine the condition of equilibrium. That is, in order to obtain the conditions of equilibrium, it is necessary to introduce partitions. In this theory, however, the classes of walls are increased so that other criteria of equilibrium can be obtained in addition to those of thermal equilibrium.

We will refer to the formulation of "neo-Gibbsian thermodynamics" given by Moulines (1975) as known. In contrast to C.L.T., in his formulation of Gibbsian thermodynamics, denoted by S.E.T., Moulines introduces two primitive state concepts: states in the general sense and equilibrium states. Beside them, in S.E.T., there are the following primitive functions: energy, entropy, volume, mole numbers, and pressure. In addition, for the formulation of the most important thermodynamic contraints, Moulines introduces the notion of a combination operation as a primitive dyadic operation on states. From this primitive operation on states, he derives a defined operation on the corresponding systems. Recall, a system is identified with all its states. The combination operation is the formal counterpart to the physical coupling operation. Thus, in S.E.T. the concept of composite systems is defined in terms of a combination operation and intensive functions. Instead of assuming the existence of different types of walls, in this theory, the notion of "Y-equilibrium" is defined, where Y is an abstract intensive function.

One of the most remarkable differences between S.E.T. and C.L.T. is that S.E.T. concerns only one process. Hence all the primitive functions are defined on the set of states.

In order to proceed to the formulation of the Second Law, Moulines introduces the precised form of the so-called "fundamental equation", i.e. he assumes the existence of an unspecified "functional correlation" between entropy and the other primitive functions. The functional correlation means that the values of the entropy in the equilibrium states depend on the values of energy, volume, etc. This dependence is expressed by means of a given function f^S whose particular form can be different for different kinds of systems, but it is always the same for a given system. Moulines calls the function f^S which is defined on tuples of real numbers an "entropy determination".

Finally, in S.E.T., amongst the primitive concepts those of state, volume, mole numbers, and pressure are non-theoretical and those of equilibrium state, combination operation, energy, and entropy are theoretical. It should be noted, however, that Moulines has not raised the question of whether or not the function f^S is a theoretical one. The axiom D3-(4) which is expressed by means of the function f^S is considered as an empirical law; but the axiom D3-(5) which is also expressed by means of it is considered "as an intertheoretical relation (Tisza's 'corresponding principle'), a bridge between thermodynamics and mechanics. Had we an appropriate conceptual apparatus for reconstructing this sort of inter-theoretical principles, D3-(4))and D3-(5) would certainly not fall under the same logical category". Then he says: "... D3-(4), as stated in the core, does not specify anything. It is a general frame law. In a way analogous to Newton's second law in classical particle mechanics, this thermodynamic frame law is not 'falsifiable'. For a given simple system, we try to apply the theory with a specific f^S; if we do not get the desired results, we just try another form of entropy determination."[6] But this seems to mean that the function f^S is considered to be a theoretical one.

2. *Formalization of T.A.I.S.*

After the foregoing discussion of T.A.I.S., it should be clear that all primitive terms are non-theoretical ones. The reconstruction of T.A.I.S. given here is based on the formalization of "First Law Thermodynamics" given by Day (1977). We will assume that the basic concepts of the theory of differentiable manifolds, particularly the Frobenius theorem, are known to the reader. For discussion of these topics one can consult Von Westenholtz (1978) or Boothby (1975).

2.1 Definition

$TAIS_{pp}(x)$ iff there exists an E, P, P^{ad}, P^{qs}, L and W such that

(1) $x = \langle E, P, P^{ad}, P^{qs}, L, W\rangle$;

(2) E is a connected C^1-manifold of dimension $n+1\geq 2$, with a single chart where the coordinate functions are denoted by $x_0, x_1, \ldots, x_n$;

(3) P is a non-empty set;

(4) P^{ad} and P^{qs} are non-empty subsets of the set P;

(5) L is a function from P into $E \times E$;

(6) W is a function from P into the set Re of real numbers.

The intended interpretations of the primitives are as follows: E is the set of all (equilibrium) states. The coordinate functions are to be interpreted as representing the $n+1\geq 2$ independent state variables which are appropriate to the system of interest. Note that there is only one non-deformation coordinate which will be denoted by x_0. The image of E is usually called the phase space of the system. P is the set of all possible processes which the system can undergo. P^{ad} and P^{qs} are the subsets of all adiabatic and quasistatic processes, respectively. The function L is to be interpreted as an assignment f initial and final states to the processes. We will use the notations $L(p) = \langle p^i, p^f \rangle$, or $L(p) = \langle e_1, e_2 \rangle$ for all $p \in P$. (Of course, the values may be expressed in the local coordinates as well.) For notational convenience, we will use the expression $\overline{L(p)}$ to denote the ordered pair which is obtained by reversing the members of the ordered pair $L(p)$. Finally, any process $p \in P$, for which $\overline{L(p)} = L(p)$ will be called *cyclic process*. For any $p \in P$, $W(p)$ is the real number which represents the net work done by the system in the process p with respect to some fixed units of measurement.

The set of all partial possible models (and of all possible models) of T.A.I.S. is the set $\underline{TAIS}_{pp}$ /$=\underline{TAIS}_p$/ $= \{x : TAIS_{pp}(x)\}$.

2.2 Definition

TAIS(x) iff there exists an E, P, P^{ad}, P^{qs}, L, W, and DW such that

(1) $x = \langle E, P, P^{ad}, P^{qs}, L, W \rangle$;

(2) $TAIS_{pp}(x)$;

(3) $DW = \sum_{1 \le i \le n} a_i dx_i$ is a non-singular Pfaffian differential form on E where $a_i = a_i(x_o, x_1, \dots, x_n)$;

(4) For any $e_1, e_2 \in E$, there exists an $p \in P^{ad}$ such that either $L(p) = \langle e_1, e_2 \rangle$ or $L(p) = \langle e_2, e_1 \rangle$;

(5) For any $p, P' \in P^{ad}$, if $L(p) = L(p')$ then $W(p) = W(p')$;

(6) For any $e_1, e_2, e_3 \in E$ and for any $p', p'' \in P^{ad}$, if $L(p') = \langle e_1, e_2 \rangle$ and $L(p'') = \langle e_2, e_3 \rangle$ then there exists a $p \in P^{ad}$ such that $L(p) = \langle e_1, e_3 \rangle$ and $W(p) = W(p') + W(p'')$;

(7) There exists a bijection C between the set P^{qs} and the set D_{pw}(/0, 1/, E) of the piecewise differentiable curves in E such that $p^i = C_p(0)$ and $p^f = C_p(1)$, for $p \in P^{qs}$;

(8) For any $p \in P^{qs}$, $W(p) = \int_{C_p} DW$.

According to the intended interpretation, the particular form of DW given in the coordinate system $x_o, x_1, \ldots, x_n$ contains all physical properties of the system of interest. The differentiable functions a_i, $1 \leq i \leq n$, which in fact contain the empirical information necessary for the application of theory, are sometimes as the generalized forces corresponding to the derformation variables. "The functions a_i can be determined experimentally by measuring at every state of the system those forces that must be applied from the outside to maintain equilibrium."[7] Axiom (4) stipulates that for any two states of a given system there must exist an adiabatic process which connects these two states, but the direction of the process is unspecified. Axiom (5) stipulates that for any two adiabatic processes which have the same initial and final states the net work done by the system in one of these processes is equal to the net work done by the system in the other process. Axiom (6) stipulates that, if there exist adiabatic processes such that the final state of one process is the initial state of another process, then there exists an adiabatic process which connects the initial state of the former process with the final state of the latter process and the net work done by the system in this process is equal to the sum of the net works done by the system in the first two processes. Axiom (7) stipulates that every quasistatic process is represented by a piecewise differentiable curve, and conversely. Axiom (8) together with the intended interpretation of the Pfaffian DW may be considered as an exact mathematical formulation of the expression "the net work done by the system in a quasistatic process p".

The set of all models of T.A.I.S. is <u>TAIS</u> = {x : TAIS(x)}.

Let us now consider the theorems and definitions which will be derived from *2.2 Definition*.

The proofs of the following theorems may be found in Day (1977).

2.3 Theorem

For any $p_1, p_2, p_3 \in P^{ad}$, if $p_1^f = p_2^i$ and $L(p_3) =$ $= \langle p_1^i, p_2^f \rangle$ then $W(p_3) = W(p_1) + W(p_2)$.

2.4 Theorem

For each *cyclic* process $p \in P^{ad}$, $W(p) = 0$.

2.5 Theorem

For any $p_1, p_2 \in P^{ad}$ if $L(p_1) = L(p_2)$ then $W(p_1) = -W(p_2)$.

The first theorem is the usual statement of the First Law of Thermodynamics.[8] In this theory, however, it is derived from two weaker requirements (explicitly, from axioms (5) and (6)) which contain all the essential conditions necessary to the foundations of thermodynamics. The latter two theorems are simple consequences of the first one.

Now, in order to formulate the well-known fundamental properties of T.A.I.S., we will give a definition of internal energy function for the system (E,P).

2.6 Definition

Ief(E, P) is the set of all functions U whose domain is E taking on real numbers as values such that for any $p \in p^{ad}$, $U(p^i) - U(p^f) = W(p)$.

2.7 Theorem

The set Ief(E, P) is not empty.

2.8 Theorem

For any $U_1, U_2 \in Re^E$ if $U_1 \in$ Ief(E, P) then $U_2 \in$ Ief(E, P) if and only if there exists a $c \in Re$ such that for all $e \in E$, $U_1(e) = U_2(e) + c$.

According to *2.8 Theorem*, an internal energy function of the system (E, P) is unique up to an additive constant. The proofs of these theorems are also in Day (1977).

Next, we will give a definition of heat function for the system (E, P) which associates with each process $p \in P$ the net heat absorbed by the system in that process.

2.9 Definition

Heat (E, P) is the set of all functions Q whose domain is P taking on real numbers as values such that for all $p \in P$, $Q(p) = U(p^f) - U(p^i) + W(p)$.

2.10 Theorem

Heat $(E, P) = \{Q\}$.

2.11 Theorem

For any $p \in P^{ad}$, $Q(p) = 0$.

2.12 Theorem

For any *cyclic* process $p \in P$, $Q(p) = W(p)$.

The proofs of these theorems may also be found in Day (1977).

According to the first of these theorems there exists one and only one heat function for a given system. The second theorem upon interpretation says that the net heat absorbed by the system (E, P) in any adisbatic process is zero. Finally, the third theorem may be considered as expressing the impossibility of a perpetual motion machine of first kind.

In order to formulate the basic set-theoretic predicate of C.L.T. we need an important auxiliary concept.

2.13 Definition

AIP(E, P) is the set of all points $e \in E$ for which there exist points $e' \in E$ arbitrarily close to e that are inaccessible by adiabatic processes with initial state $e \in E$.

The points in the set AIP(E, P) are called Adiabatically Inaccessible Points of the system (E, P).[9]

3. *Formalization of C.L.T.*

As in the case of T.A.I.S., we will assume that it is clear from the considerations made in the introduction that all of the primitive terms of C.L.T. are nontheoretical ones. Thus the frame of C.L.T. is also a special one because the set of possible models for the fundamental predicate of the theory is identical with the set of its partial possible models. In this case, of course, the constraints are imposed upon non-theoretical components.

3.1 Definition

$CLT_{pp}(x)$ iff there exists an E, P, P^{ad}, P^{qs}, L, W and U such that

(1) $x = \langle E, P, P^{ad}, P^{qs}, L, W, U \rangle$;

(2) TAIS(E, P, P^{ad}, P^{qs}, L, W where $DW = \sum_{i=1}^{n} a_i dx_i$;

(3) $x_o = U \in Ief(E, P)$ is of class C^1;

(4) $DQ = DU + dW$ is a non-singular Pfaffian.

The set of all partial possible models (and of all possible models) of C.L.T. is the set

$$\mathit{CLT}_{pp}\ (= \mathit{CLT}_p) = \{x : CLT_{pp}(x)\}.$$

3.2 Definition

CLT(x) iff

(1) $CLT_{pp}(x)$;

(2) For any $p \in P^{qs}$, $Q(p) = \int_{C_p} DQ$ where $Q \in Heat(E, P)$;

(3) AIP(E) = E.

The set of all models of C.L.T. is the set $\mathit{CLT} = \{x : CLT(x)\}$.

The axiom (3) in *3.1 Def.* requires that the n + 1 independent state variables consist of the internal energy and n deformation coordinates. Axiom (2) in *3.2 Def.* may be considered as a precised form of the expression "the amount of heat gained by the system (E, P) in a quasistatic process P". Axiom (3) in *3.2 Def.* is *Carathéodory's principle*. It simply says that all points in E are adiabatically inaccessible points. According to this axiom, which may be considered as the Second Law of Thermodynamics, axiom (7) in *2.2 Def.*, and *2.11 Theorem*: for each point $e \in E$, there exist points

$e' \in E$ arbitrarily close to e that are inaccessible from e along curves for which $DW = 0$.

The following theorem, which may be called *Carathéodory's theorem*, is an application of Frobenius's integrability theorem. It says that the integrability condition for the Pfaffian equation $DQ = 0$ and Carathéodory's principle are equivalent.

3.3 Theorem

For any x, E and P if $CLT(x)$, $pr_1(x) = E$ and $pr_2(x) = P$ then the following statements are equivalent:

(i) There exist two functions $h, s \in C^1(E, Re)$ such that $DQ = h\, ds$.

(ii) $AIP(E, P) = E$.

The proof of this theorem may be found in Von Westenholtz (1978) p. 237.

According to this theorem, Carathéodory's principle is a necessary and sufficient condition for a function $s \in C^1(E, Re)$ to exist such that $ds \neq 0$ on E and the hypersurfaces of the type

$N =_{df} \{e \in E : s(e) = \text{const.} \text{ and } ds(e) \neq 0\}$

are integral surfaces for DQ. By the intended interpretation, any continuous sequence of states, no two members of which are quasistatically accessible from each other, therefore generates a family of non-intersecting hypersurfaces in E, called the *adiabatic* hypersurfaces, or simply the adiabatics of the system (E, P).

The new function $s \in C^1(E, Re)$, determined by the Second Law of Thermodynamics and the particular form of DQ, is called *empirical entropy function* of the system.

3.4 Definition

$Ent(E, P)$ is the set of all functions $s \in C^1(E, P)$ for which there exists a function $h \in C^1(E, Re)$ such that $DQ = h\, ds$.

It is well known from the proof of the Carathéodory theorem that an empirical entropy function is uniquely determined up to arbitrary monotonically increasing or decreasing transformations of class C^1; i.e. if $g \in C^1(Re)$ and $s \in Ent(E, P)$ then $g \circ s \in Ent(E, P)$.

A basic property of the empirical entropy function is that it has the same value for all states which are accessible from each other by quasistatic adiabatic processes.

The following theorem says that an entropy function can be used instead of the internal energy function as one of the independent state variables.

3.5 Theorem

For any x, E, P, U, and s if CLT(x) and $pr_1(x) = E$, $pr_2(x) = P$, $U = x_0$, and $s \in Ent(E, P)$ then

$$f_s \circ f_U^{-1} \quad \text{and} \quad f_U \circ f_s^{-1}$$

are C^1-diffeomorphisms between $f_U(E)$ and $f_s(E)$, where the coordiane functions of the chart f_U are $x_0 = U$, $x_i = pr_i \circ f_U$ $(1 \leq i \leq n)$ and of the chart f_s are $x_0 = s$, $x_i = pr_i \circ f_s$ $(1 \leq i \leq n)$.

Proof: It is enough to prove that $\partial s/\partial U \neq 0$ for any $e \in E$. Assume that $s \in Ent(E, P)$, i.e. $DQ = h\, ds$. Then by point 4. of *3.1 Def.*

$$DQ = dU + \sum_{i=1}^{n} a_i dx_i = h\, ds.$$

Since
$$ds = \frac{\partial s}{\partial U} dU + \sum_{i=1}^{n} \frac{\partial s}{\partial x_i} dx_i \,,$$

we have
$$DQ = h \frac{\partial s}{\partial U} dU + h \sum_{i=1}^{n} \frac{\partial s}{\partial x_i} dx_i \,.$$

Hence
$$h \frac{\partial s}{\partial U} = 1, \quad \text{that} \quad \partial s/\partial U \neq 0 \,.$$

To formulate the general constraints of C.L.T. precisely we will need the following definitions.

3.6 Definition

$$\bar{E} = \langle E_{i_1}, E_{i_2}, \ldots, E_{i_k}, \ldots \rangle \qquad \underline{E} = \cup \bar{E}$$

$$\bar{P} = \langle P_{i_1}, P_{i_2}, \ldots, P_{i_k}, \ldots \rangle \qquad \underline{P} = \cup \bar{P}$$

$$\bar{L} = \langle L_{i_1}, L_{i_2}, \ldots, L_{i_k}, \ldots \rangle \qquad \underline{L} = \cup \bar{L}$$

$$\bar{W} = \langle W_{i_1}, W_{i_2}, \ldots, W_{i_k}, \ldots \rangle \qquad \underline{W} = \cup \bar{W}$$

$$\bar{Q} = \langle Q_{i_1}, Q_{i_2}, \ldots, Q_{i_k}, \ldots \rangle \qquad \underline{Q} = \cup \bar{Q}$$

$$\bar{U} = \langle U_{i_1}, U_{i_2}, \ldots, U_{i_k}, \ldots \rangle \qquad \underline{U} = \cup \bar{U}$$

where E_{i_k}, P_{i_k}, $P^{ad}_{i_k}$ etc. are the components associated with application i_kth of C.L.T.

First, we will formulate the identity constraints for functions L, W, Q, and U. These constraints reflect the intuitive idea that these functions measure an "intrinsic property" of the system, i.e. they require that the different concrete functions L_{i_1}, L_{i_2}, ... etc. proceeding out of the abstract functions $\underline{L}$, $\underline{W}$, etc. yield the same value for a process, respectively.

3.7 Constraint

$\underline{L}$ is a function from $\underline{P}$ into $\underline{E} \times \underline{E}$;
$\underline{W}$ is a function from $\underline{P}$ into Re;
$\underline{Q}$ is a function from $\underline{P}$ into Re;
$\underline{U}$ is a function from $\underline{E}$ into Re

When there is no risk of confusion, we will omit the underlining and use the same letter for the concrete and abstract functions.

To formulate the "diatermic interaction" between two standard systems we will need the notion of a distermic coupling relation between them. Let us denote this relation by $R^{dt} \subseteq \underline{P} \times \underline{P}$. It is, of course, a primitive relation likewise the combination operation $\oplus$ used in Moulines (1975). The physical interpretation of the relation R^{dt}, which may be considred as the formal counterpart to the idea of the "diatermic interaction", is the following: For any two processes P_1 and P_2, which belong to distinct systems, if $\langle p_1, p_2\rangle \in R^{dt}$ then then at the initial states p_1^i, p_2^i there must be a diatermic interaction between the two systems (E_1, P_2) and (E_2, P_2) for which the states p_1^i and p_2^i pertain, and there is a function $F_{12} : E_1 \times E_2 \rightarrow Re$ such that $F_{12}(p_1^f, p_2^f) = 0$. The particular form of F_{12} is determined by a set of primary empirical conventions. Furthermore, this relationship shall be unique in the following sense. If the local coordinates of p_1^f and p_2^f are $x_o, x_1, \ldots, x_m$ and $y_o, y_1, \ldots, y_n$, respectively, and if all but one of the x's and y's in the equation $F_{12}(x_o, \ldots, x_m, y_o, \ldots, y_n) = 0$ are known, the unknown coordinate shall be determined uniquely by this equation.

As is well known, the main aim of claiming the Zeroth Law of Thermodynamics is to proceed to the idea of empirical temperature. However, the usual statement of this law does not imply the continuity property of the entropy function. It was pointed out by Lenker (1979) that we have to place additional restrictions on the set of relations F_{ij} and the state spaces so that continuous empirical temperature functions do exist.

First, we will define the following notions:

3.8 Definition

For any x_1, x_2, E_1, E_2, P_1, and P_2 if $CLT(x_1)$, $pr_1(x_1) = E_1$, $pr_2(x_1) = P_1$, $CLT(x_2)$,= $pr_1(x_2) = E_1$, and $pr_2(x_2) = P_2$ then

E_{12}^{dt} is the set of all points $\langle e_1, e_2\rangle \in E_1 \times E_2$ for which there exist $p_1 \in P_1$ and $p_2 \in P_2$ such that $\langle p_1, p_2\rangle \in R^{dt}$, $e_1 = p_1^f$ and $e_2 = p_2^f$;

P_{12}^{dt} is the set of all pairs $\langle p_1, p_2\rangle \in P_1 \times P_2$, for which $\langle p_1, p_2\rangle \in R^{dt}$.

3.9 Constraint

For any x_1, E_1, and e_1 if $CLT(x_1)$, $pr_1(x_1) = E_1$, and $e_1 \in E_1$ then there exists an x_2, E_2, and e_2 such that $CLT(x_2)$, $pr_1(x_2) = E_2$, $e_2 \in E_2$ and $\langle e_1, e_2\rangle \in E_{12}^{dt}$.

That is, in the case of standard systems, equilibrium can exist only on condition that one or more relations of the form $F_{ij} = 0$ are satisfied.

The following theorems are simple consequences of *3.8 Def.* and *3.9 Constraint*.

3.10 Theorem

If e is a state of the system composed by E_1 and E_2 with independent state variables n_1 and n_2, respectively, then the composed system in state e has $n_1 + n_2 + 1$ independent state variables with $n_1 + n_2$ deformation coordinates.

3.11 Theorem

For any e, e′ ∈ E, there exists a process $p \in P^{dt}$ such that $L(p) = \langle e, e'\rangle$ or $L(p) = \langle e', e\rangle$; where P^{dt} is the set of those processes of the system (E, P) for which there exists a system (E′, P′) and a process $p' \in P'$ with $\langle p, p'\rangle \in R^{dt}$.

The next two constraints posed upon the relations F_{ij} are usually called the Zeroth Law of Thermodynamics.[10]

3.12 Constraint

For any x_i, E_i, and e_i $(i = 1, 2)$ if $CLT(x_i)$, $pr_1(x_i) = E_i$, $e_i \in E_i$ and $\langle e_1, e_2\rangle \in E_{12}^{dt}$ then $\langle e_2, e_1\rangle \in E_{12}^{dt}$.

3.13 Constraint

For any x_i, E_i, and e_i $(i = 1, 2, 3)$ if $CLT(x_i)$, $e_i \in E_i$, $\langle e_1, e_2 \rangle \in E^{dt}_{12}$ and $\langle e_2, e_3 \rangle \in E^{dt}_{23}$ then $\langle e_1, e_3 \rangle \in E^{dt}_{13}$.

First, we will concentrate the identical copies of a given standard system. The set of relations F_{ij} is thus reduced to a single relation F. It follows from the last two constraints that F induces an equivalence relation E^{dt} on $E \times E$ if we define $\langle e_1, e_2 \rangle \in E^{dt}$ exactly when $F(e_1, e_2) = 0$.

3.14 Theorem

E^{dt} is an equivalence relation on E.

Denote the equivalence class of $e \in E$ by $/e/$, and the quotient space by E/E^{dt}.

3.15 Constraint

For any $e \in E$, $/e/$ is closed and connected.

Let E^+ be the subset of E which satisfies the condition that, for any $e_1, e_2 \in E^+$, $\langle e_1, e_2 \rangle \notin E^{dt}$.

3.16 Constraint

E^+ is not connected.

We are now in a position to apply a result due to Eilenberg (1941).

3.17 Theorem

There exist exactly two inverse near orders on E.

The intuitive idea behind this procedure is the following: the concept of thermal equilibrium induces a near order relation on E, and an empirical temperature function is simply a real-valued near order-preserving function defined on E.

Extension to more than one systems. Let us assume that the state spaces E_i $(i = 1, 2, 3)$ of three arbitrary systems are endowed with equivalence relations E_i^{dt} and near orders, denoted by H_i, respectively.

Now, we can define a subset $E_{123} \subseteq E_1 \times E_2 \times E_3$ by the condition that

$$\langle e_1, e_2, e_3 \rangle \in E_{123} \quad \text{iff} \quad F_{ij}(e_i, e_j) = 0$$

where $i,j = 1, 2, 3,\ i \neq j$ for any $e_1 \in E_1,\ e_2 \in E_2,$ $e_3 \in E_3$.

Moreover, we can define a near order H_{123} on E_{123} componentwise if the equivalence relation E_{123}^{dt} on E_{123} generated by E_i^{dt} is also defined componentwise.

Finally, we can define two functions.

3.18 Definition

For any $e_1 \in E_1$, $e_2 \in E_2$ and $e_3 \in E_3$, let the function

$$Y_1 : E_1/E_1^{dt} \rightarrow E_2/E_2^{dt}$$

be defined by the assignment $Y_1(/e_1/) = /e_2/$ if $F(e_1,e_2) = 0$, and the function

$$Y_2 : E_2/E_2^{dt} \rightarrow E_3/E_3^{dt}$$

be defined by the assignment $Y_2(/e_2/) = /e_3/$ if $F_{23}(e_2,e_3) = 0$.

The next two constraints complete the sufficient conditions for the Zeroth Law of Thermodynamics to imply the existence of a continuous empirical temperature function.

3.19 Constraint

The functions Y_1 and Y_2 are near order-preserving functions.

3.20 Constraint

E_{123}/E_{123}^{dt} possesses a countable dense subset.

3.21 Theorem

There exists a real-valued continuous near order-preserving map $\bar{t}$ with domain E_{123}/E_{123}^{dt} .

Thus $t = \bar{t} \circ G$ is a real-valued continuous near order-preserving map defined on E_{123}, where $G(x) = /x/$, for any $x \in E_{123}$.

Where are finally ready to define the empirical temperature functions. Since they are all defined similarly, we exhibit t_1 by $t_1(e_1) = t(\langle e_1, e_2, e_3\rangle)$, i.e. an empirical temperature function of the system (E_1, P_1) is a restriction of the function t to E_1.

3.22 Definition

For any x, E, P if $CLT(x)$, $pr_1(x) = E$ and $pr_2(x) = P$ then Temp(E, P) is the set of all empirical temperature functions of the system (E, P).

It is important to note that temperature functions exist only for systems which are free from adiabatic partitions and vacuous spaces. The particular form of an empirical temperature function depends on the particular form of the empirical functions F_{ij} used to determine the condition of thermal equilibrium. The existence of a unique empirical tempereture function for a given system has not been proved, and in fact no such function exists. It is obvious that any monotonically increasing or decreasing transformation of a given temperature function may equally be considered as a new temperature function of the system of interest. Of course, requirements of continuity of differentiability on the transformation function and on the empirical functions F_{ij} are understood.

The following theorem says that for any $t \in Temp(E,P)$ if t is given by the local coordinates $U, x_1, \ldots, x_n$ then it must in fact depend on at least one of the x's.

3.23 Theorem

For any x, E, P, if $CLT(x)$, $pr_1(x) = E$, and $pr_2(x) = P$ then for any $t \in Temp(E, P)$ there exists a coordinate function x_i $(1 \leq i \leq n)$ such that $\partial t/\partial x_i \neq 0$.

Proof: Let $e_o, e \in E$ and $p \in P^{ad}$ such that $e \neq e_o$, $L(p) = \langle e_o, e \rangle$ and the local coordinates of e_o, e are the following

$$\bar{x}_o = \langle U, x_1, \ldots, x_i, \ldots, x_n \rangle \quad \text{and}$$

$$\bar{x} = \langle U, x_1, \ldots, x_i + dx_i, \ldots, x_n \rangle, \quad \text{respectively;}$$

i.e. $dU = 0$, and $dx_j = 0$ if $j \neq i$. By *2.11 Theorem*, $Q(p) = 0$ and since $Q(p) = \int_{C_p} DQ = a_i\, dx_i$ by *3.2. Def.*, we have

$$a_i(U, x_1, \ldots, x_n) = 0 .$$

That is $U, x_1, \ldots, x_n$ would not be independent, but this is a contradiction.

The consequence of this theorem is that any empirical temperature function can be introduced in expressions involving x_i by virtue of the given dependency relation.

As is well known, the proof of *the existence of absolute temperature and entropy function* is one of the main results of C.L.T. Next, we will proceed to this task.

Let us consider the thermal equilibrium of the systems (E_1, P_1) and (E_2, P_2). Let $x_o, \ldots, x_m$ and $y_o, \ldots, y_n$ be the independent state variables corresponding to these systems, respectively, where $x_o = s_1 \in Ent(E_1, P_1)$, $x_1 = t_1 \in Term(E_1, P_1)$, $y_o = s_2 \in Ent(E_2, P_2)$, and $y_1 = t_2 \in Temp(E_2, P_2)$. The thermal equilibrium imposes the relation $t_1(e_1) = t_2(e_2)$, for all $e_1 \in E_1$, $e_2 \in E_2$. That is, if we use t for the common empirical temperature, then the independent variables are

$$t,\ s_1,\ s_2,\ x_2,\ \dots\ ,\ x_m,\ y_2,\ \dots\ ,\ y_n$$

where t denotes the common empirical temperature. Since there exists an integrating factor to each system separately, and the combined system is also a standard system, using $DQ = DQ_1 + DQ_2$ for the Pfaffian of the combined system, one finds

$$(1)\qquad h\,ds = h_1\,ds_1 + h_2\,ds_2$$

omitting suffices for the combined system. On the other hand, from the functional relation $s = s(t, s_1, s_2, x_2, \dots , x_m, y_2, \dots , y_n)$, the derivate ds can be expressed in terms of the $m + n + 1$ independent coordinates:

$$(2)\qquad ds = \frac{\partial s}{\partial t}dt + \frac{\partial s}{\partial s_1}ds_1 + \frac{\partial s}{\partial s_2}ds_2 + \sum_{i=2}^{m} \frac{\partial s}{\partial x_i}dx_i + \sum_{j=2}^{n} \frac{\partial s}{\partial Y_j}dy_j\ .$$

From (1) and (2)

$$(3)\qquad \frac{\partial s}{\partial t} = \frac{\partial s}{\partial x_i} = \frac{\partial s}{\partial x_j} = 0 \qquad (2 \leq i \leq m, \quad 2 \leq j \leq n)$$

and

$$(4)\qquad \frac{\partial s}{\partial s_k} = \frac{h}{h_k} \qquad\qquad (k = 1,\ 2)$$

By (1) and (3), $s = s(s_1, s_2)$, consequently, from the equation (4) follows that

$$(5)\qquad \frac{\partial^2 s}{\partial s_k \partial t} = 0, \quad \text{that is} \quad \frac{\partial}{\partial t}\left(\frac{h^k}{h}\right) = 0 \qquad (k = 1,\ 2)\ .$$

Performing the differentiation explicitely, we find that

$$(6)\qquad \frac{1}{h_1}\frac{\partial h^1}{\partial t} = \frac{1}{h_2}\frac{\partial n_2}{t} = \frac{1}{h}\frac{\partial h}{\partial t}\ .$$

Since $h_1 = h_1(t, s_1, x_2, \dots , x_m)$ and $h_2 = h_2(t, s_2, y_2, \dots , y_n)$ it follows that each expression (6) is a function of t only, g(t) say. Thus, g(t) is a universal function of the chosen empirical temperature function. It is easily seen that the sign of the integrating factor h does not affect the specific properties of the function g(t). So far, there is no need to make special assumptions concerning the sign of h. By integration, we obtain

$$(7) \qquad h_k(t, s_k, \bar{z}_k) = B_k(s_k, \bar{z}_k) \exp\{ \int_{t_o}^{t} g(t)dt\} \quad (k = 1, 2)$$

where $\bar{z}_1 = \langle x_2, \ldots, x_m \rangle$ and $\bar{z}_{22} = \langle y_2, \ldots, y_n \rangle$.

The value of this integration depends on the value of t_o. Consequently, to fix the value of the integrating factor we need a new constraint:

3.24 Constraint

The standard empirical temperature t_o is the same for all physical systems.

Thus, the value of the integrating factor h_k at this temperature occours as a constant of integration, and has been denoted by

$$(8) \qquad h_k(t_o, s_k, \bar{z}_k) = B_k(s_k, \bar{z}_k), \qquad (k = 1, 2) .$$

The possibility of using a single empirical entropy function for the combined system can be justified by an argument based on (1) and (7) but we will not repeat here.

To gain the existence of an absolute temperature function we have to require not only t_o but also a constant C is given for all standard systems.

3.25 Constraint

The value of the constant of integration derived in (7) is the same, C say, for all physical systems.

Now, we can write for all systems

$$DQ = h \, ds = T \, dS$$

where, by definition

3.26 Definition

$$T(t) = C \exp\{ \int_{t_0}^{t} g(t)dt \} \quad \text{and}$$

$$S(s) = C^{-1} \int_{s_0}^{s} B(s, \bar{z})ds$$

and s_0 refers to a standard value of s for the particular system under discussion. The function T is called the *absolute temperature*, and depends only on t_0 and the empirical temperature t, while S is called the *entropy* which depends on the variables $\bar{z}$, the empirical entropy s and the value s_0. To fix the value of S we will require the following

3.27 Constraint

For all physical systems, the value s_0 has the property $S(s_0) = 0$.

The name absolute temperature comes from the fact that one and the same function T, depending only on t_0 and the empirical temperature function t, serves as an integrating denominator for the Pfaffian DQ of all standard systems whose temperatures are measured on a common scale t. The function T can be also considered as a transformation from one temperature function to another.

As it can be justified that the function g(t) can not change sign under the given assumptions, we will assume the following

3.28 Constraint

By adjustment of the sign of the empirical temperature function if necessary, g(t) has been arranged to be positive for all systems.

The following theorem is a simple consequence of this requirement.

3.29 Theorem

The absolute temperature is a strictly increasing or a strictly decreasing function of the empirical temperature, when the remaining variables are kept fixed.

Proof: Since g(t) is real, T and dT/dt both have the sign of the constant C.

The next theorems say that T and S have certain essential invariance properties.

3.30 Theorem

For any x, E, P, t_1, and t_2 if CLT(x), $pr_1(x) = E$, $pr_2(x) = P$, and $t_1, t_2 \in Temp(E, P)$ then $T(t_1) = T(t_2)$.

Proof: See Landsberg (1961), p. 64

3.31 Theorem

T and S are independent of the choice of the integrating denominator for DQ.

Proof: See Landsberg (1961), pp. 64-65

3.32 Theorem

T and S are independent of the choice of the empirical temperature functions.

Proof: See Landsberg (1961), pp. 65-66

Against the properties of invariance just discussed must be set other properties of entropy and absolute temperature, which depend on certain arbitrary choices. One of these have been already done in *3.28 Constr.*, i.e. the sign of the empirical temperature. As it was already noted, the choice of sign for the integrating factors h has no effect on the absolute temperature T, but it does effect the sign of the entropy S: The absolute temperature function is defined only to within a constant factor since we can always write $(c^{-1}T)d(cS)$ in place of TdS, where $c \in Re$. We will require that the sign of c be chosen so that $d(cS)/ds > 0$, whilst factors c^{-1} and c may simply be left understood in T and S, respectively. That is, the sign of S depends on the sign of the integrating denominator for DQ.

3.33 Constraint

The sign of the integrating denominator for DQ must be chosen that $dS/ds > 0$.

Finally, the sign of the constant C determines the sign of T and of dT/dt.

3.34 Constraint

The sign of C must be positive.

Now, let us turn to the "one-way" property of entropy in adiabatic processes. According to *2.2 Def.* (*4*), *2.13 Def.* and *3.2 Def.* (*3*), two points in E which correspond to different entropy values can be linked by a non-static adiabatic process. Consider now the entropy values attainable by arbitrary adiabatic processes which commence at a given state $e_o \in E$. Let the function S_o be defined for all processes $p \in P^{ad}$, for which $p^i = e_o$, by $S_o(p) = S(p^f)$. The range of S_o is an interval of Re by *2.1 Def.* (*2*). It follows from *3.2 Def.* (*3*) that $S_o(e_o)$ is an endpoint of this interval, i.e. the value of the function S_o *can either not increase or not decrease in adiabatic processes with initial state* e_o. Of course, this property is independent of the particular choice of the initial point e_o. It remains to be decided if the sense in which the entropy can change from a given initial state is the same for all initial states of a given system. However, this cannot be derived from the assumptions made so far.

3.35 Constraint

The monotonity property of the function S_o in adiabatic processes is the same for all points in E.

The following theorem says that this sense is the same for all standard systems.

3.36 Theorem

For any (E_1, P), (E_2, P_2), p_1, p_2, s_1, and s_2 if $p_1 \in P_1^{ad}$, $p_2 \in P_2^{ad}$, $s_1 \in Ent(E_1, P_1)$, and $s_2 \in Ent(E_2, P_2)$ then

$$\text{sign } S_{p_1^i}(p_1^f) = \text{sign } S_{p_2^i}(p_2^f) \ .$$

Proof: See Landsberg (1961), p. 82

To determine the sense in which the entropy of a system changes in adiabatic processes, we have to define the entropy of composed systems. The functions T and S have been defined only for standard systems. If the systems (E_1, P_1) and (E_2, P_2) are in thermal equilibrium, then from the expressions

$$h \, ds = h_1 \, ds_1 + h_2 \, ds_2$$

$$DQ = TdS$$

we have

$$DQ = T \, dS_1 + T \, dS_2 = T \, d(S_1 + S_2) \ .$$

That is

$$dS = dS_1 + dS_2 \ ;$$

and this implies

$$S = \int_{s_o}^{s} B(s, \bar{z}) \, ds = \int_{s_o}^{s} \left[B_1(s_1, \bar{z}_1) ds_1 + B_2(s_2, \bar{z}_2) ds_2 \right]$$

$$= S_1 + S_2 + A \ ,$$

where the constant A, which depends only on the standard entropy value s_o, is zero by *3.27 Constraint*. Thus, the entropy of the composed system in the given equilibrium state is the sum of the entropies of the individual systems. If the part systems are each in equilibrium, but their temperatures are different, then the composed system is not a standard one. That is, this system has no unique temperature function, nor has it an entropy function. However, an appropriate entropy function can be defined to be the sum of the entropies of the part-systems. Hence, the increment of the entropy of a composed system consisting of two adiabatic part-systems is

$$dS = dS_1 + dS_2$$

bearing in mind that the equation $DQ = TdS$ does not hold in this case, since the composed system is not a standard one.

If an absolute temperature T is introduced as a weighted average of T_1 and T_2 then we can assign an appropriate standard system to the given composed one. The Pfaffian of the new system is defined by

$$DQ = T_1 \frac{dS_1}{dS} + T_2 \frac{dS_2}{dS} dS$$

Now, if the initial temperature difference $T_2 - T_1 > 0$ is very small, then the change of entropy of the composed system in a quasistatic non-adiabatic process which establishes a temperature equality is

$$(*) \qquad dS = DQ(1/T_1 - 1/T_2) > 0$$

i.e. it is positive by *2.9 Def., 3.2 Def.* (2), and *3.34 Constr.*

Then, under the same initial states, instead of using a quasistatic non-adiabatic process, we use a non-static adiabatic process in which the variables entering into the expression for DW are again kept fixed to attain temperature equality between the two systems. Of course, the final states in these two experiments will in general not be identical. But it would be reasonable to expect them to be very similar, with a very small distance between them. For the non-adiabatic element in the first process, and the non-static element in the second process, are then both very slight. It follows that the sign of the entropy changes involved is the same in both processes. Thus, by *3.35 Constr.* and (*), the entropy of a system (which may contain adiabatic partitions and vacuous spaces) in a given state cannot be adiabatically decreased. This statement is usually called the *principle of the increase of entropy*.

4. Concluding remarks

First of all, the formalization of C.L.T. given here is not concerned with the "complete" equilibrium thermodynamics, because it does not account for the so-called Third Law of Thermodynamics.

Instead, the main points are

1. to reconstruct the relationship between the First Law and the Second Law, and it has been achieved through the definition of TAIS and of CLT;

2. to formulate the sufficient conditions for the existence of empirical temperature functions; it has been achieved through Constraints 3.9, 3.12, 3.13, 3.15, 3.16, 3.19, and 3.20;

3. to formulate the sufficient conditions for the existence of absolute temperature function and entropy function, which has been achieved through Constraints 3.24, 3.25, 3.27, and 3.28;

4. to derive the principle of increase of entropy for a given type of systems characterised by Constraints 3.33, and 3.34 from Charathéodory's principle and Constraint 3.35.

The relationship between SET and CLT is not a reduction relation or a specialization relation. These are different theory-element cores. The diatermic coupling relation in CLT seems to be similar to the combination operation ⊕ used in SET. However, the physical interpretation of R^{dt} and that of ⊕ are very different. Moulines requires that ⊕ be a dyadic operation on states, i.e. $z_1 \oplus z_2$ is also a state for any states z_1 and z_2. In addition, a combined state of two systems is considered in which there is a possibility of a truly physical interaction of a certain, unspecified kind between the two systems. By (D.8) in Moulines (1975), the composite system $Z_1 \bar{\odot} Z_2$, whose members are the combined states of the systems Z_1 and Z_2, cannot be considered as a subset of $Z_1 \times Z_2$. We have to note, moreover, that Moulines fails to require that $Z_1 \bar{\odot} Z_2$ be a possible model of S.E.T., or even a "continuous process", while this is involved in the requirements (C.3), (C.4), (C.7) and (C.8).

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NOTES

1. We will assume that the reader is familiar with the structuralist approach as expounded in Sneed (1971) and in Balzar and Sneed (1977-78).
2. Carathéodory (1909)
3. Tisza (1966), p.36
4. Carathéodory (1909), p.232
5. *ibid.*, p. 230
6. Moulines (1975), p. 114
7. Carathéodory (1909), p. 238
8. Carathéodory (1909), p. 239; Landsberg (1961), pp. 25, 31; Buchdahl (1966), p. 40
9. Landsberg (1961), pp. 50-55, 77-79
10. Carathéodory (1909), pp. 243-4; Landsberg (1961), p. 13; Buchdahl (1966), pp. 27-30.

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