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PHYSICAL THEORIES AND THEIR SOCIAL AND CULTURAL ENVIRONMENT

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The problem of whether the cultural and social environment of natural sciences, physics in particular, can influence the structure of their theories is considered. It is argued that the structure of particular theories is often influenced by the environment however not in a direct way but through the scientific community itself in the process of its struggle to gain autonomy and respectability within the outer environment. In particular, such an influence occurs whenever rival theories, empirically indistigiushable at a given time, are formulated, without regard as to whether the scientific field in question is in a Kuhnian »crisis« or not. The mechanism whereby the scientific community homogenizes itself against the outer environment, by making a utilitarian and pragmatistic selection among the rival theories is elaborated. The process is illustrated by the example of the wave-particle duality of light and matter.

INTRODUCTION

»It happened at a conference in London in the thirties. A Russian delegation turned up too late and the somewhat stiff Englishmen did not allow them any time to present their contributions. Therepon, they had these contributions printed in record time and distributed among the participants. One of their papers caused a sensation: it purported to show, in the spirit of historical materialism, that Newton's discovery of the law of gravitation was prompted by his endeavours of the time — the determination of longitude at sea, a problem that had arisen from the expansion of long distance sailing... It can indeed be documented that Newton took an early interest in geography (he edited an issue of Varenius' manual) and even industriously studied books on navigation. That such a thesis... could then impress the English, only evidences the sad neglect of Newton studies in England [at the time], as a result of which they were not prepared to take a more critical view of the matter.« (Rosenfeld, 1972)

Since then it has become increasingly evident that theories not only of social but also of natural sciences could be influenced by their social and cultural environment. The only thing on which no common consent has been reached is the way in which such an influence can take place. Actually, the ever growing complexity of theories as well as of their historical origins, on the one hand, and the poor susceptibility of the theories of natural phenomena to social demands, on the other, make the task too complicated to be solvable at the present stage of investigation. For example, whether the economic growth in Europe necessarily caused corpuscular theory of light to be developed before the wave theory of light, or whether it could have been the other way round, obviously cannot be answered without taking into account all the other scientific theories developed prior to those two. In doing so we blur the peculiarities among the proper influence of the environment on the theories and the mutual influence of the theories themselves, and, as a result, investigations become hopelessly complicated. Besides, such an approach, when started, cannot offer a much better insight into the problem until a pile of empirical studies of the actual development and growth of science is collected. Pending then new cases are likely to invalidate one set of assumptions after the other. For example, after only a decade, Kuhn's elaboration of normal versus revolutionary sciences turned out not to hold water. (Suppe, 1977) Lakatos' research progammes have undergone a similar criticism. (Ibid.) Feyerabend's »anything goes« contrivance suggestively indicates that actual theories development cannot be described by too simplistic a model. (Ibid.) On the other hand, Popper's and Sneed's logical critera for appraising new scientific hypotheses can hardly be adapted to include the actual practice of producing new theories.

In a word, all the models designed to describe the actual development of scientific theories throw but a partial light on the way in which their cultural and social environment influence them. However, an onlooker can object that apparently there are exceptions to this rule. For, Forman (1971), e. g. claims that there existed a direct influence of the cultural environment on the way in which quantum mechanics was formulated in the twenties. Actually there is a whole class of such exceptions, though not in the sense that Forman put forward. Namely, the class of empirically indistinguishable theories, on which I shall dwell bellow.

In order to make our statements clearer we shall first introduce some definitions. We consider scientists as mediators between theories and the environment. The environment itself we split into two parts: the inner and the outer, in the following sense. Scientists engaged in a particular field are heavily dependent and influenced by more or less distant peer (from colleagues in a team or department to referees in journals) recognition and approval. Thus all the scientists engaged in a particular scientific field represent a rather homogenous group. The group as a unit transfers social demands from the »outside world« to the individual scientists, however translated into specific inner values and rules. (»Although the thought collective is composed of individuals, it is not simply their sum«. (Fleck, 1980)) Such a group, together with its history of former achievements and its values and rules, we call the inner environment. The outside world together with its social and cultural values, and its political and economic systems, we call the outer environment. Empirically indistinguishable (at a given time) theories we call rival theories.

INNER VALUES AND OUTER DEMANDS

The paper mentioned in the opening quotation is the one presented by Boris Hessen to the International Congress of the History of Science, London, 1931, and published by Bukharin et al. (1931). It was a challenge at the time, but it was soon recognized that »[i]t is not enough to correlate a set of [scientific] ideas with one social group or class and believe therefore that a social basis [of a scientific development] has been established. [Historians] must examine the activities in which this group is engaged which in turn could make use of the ideas and techniques in question«. (Mendelsohn, 1977) For, theories of natural sciences have to be suited to describe and predict particular experimental data whatever prompted the appearance of the theories themselves. Besides, to come back to Newton, geography is not the only field in which he took an interest. »Newton left voluminous writings on theology, chronology, alchemy, and chemistry, in all of which he was profoundly learned«, (Collier's Encyclopedia, 1971, Vol. *17*, p. 470)

In other words, outer demands only prompt scientists to dwell on particular scientific problems, and do not determine which particular theories will emerge out of such an engagement. The latter process is carried out by the inner scientific environment, while the former one is the matter of the outer environment. Since there are still many scientists who would rather not recognize even this demarcation line, claiming that their scientific work is motivated by an abstract, asocial, individual curiosity, I shall review some recent sources in support to the claim. (Later on, it will turn out that such a belief in pure scientific »[c]uriosity justifies a kind of escapism in which the scientific worker refuses to recognize the part she or he is forced to play in class society« (Albury & Schwartz, 1982).)

Cini (1980) pointed out that theoretical particle physics embraced (in the late 1950's) a »utilitarian and pragmatic, but fragmentary, concept of science with the subsequent abandoning of its traditional aim of the unification of knowledge«. »[S]tressing that inclusion in [the Institute of Defense] consultative elite functioned as a mark of *scientific* eminence among US theorists, [Cini] suggested that this drastic shift in epistemic goals »was not a mechanical adaptation [to] an environment . . . but an active identification of [their] own interests.« (Forman, 1978) »Recently, the more detailed studies by Schweber [1985] and Pickering [1985] have confirmed the insights of Cini and his associates, presenting much evidence of the phenomenologic turn taken by elementary particle theory in the late 1950's with ontologic commitments rejected in favor of instrumentalist rules of dispersion and S-matrix theory. They find this reorientation was far more characteristic of America than Europe, and they give further arguments for regarding this turn as reflecting both militarization of the social purposes of physics in the US, and a particular mental posture fostered by the application of brain-grease military matters.« (Forman, 1987)

»The list of military astronomy-related projects is in fact rather long . . . [T]he researcher does not pose questions about its use . . . This kind of attitude is welcomed by the military, who may even encourage it. By distributing in a proper way the funds to research, they have no problem in influencing the choices of the scientists and privilege the fields of more interest to them. Sooner or later the 'free and pure' scientist will, consciously or unconsciously, follow the most promising research subjects (that is the most funded) which offer more opportunities for his career, and will start to find natural that basic research which is supported by the military.« (Vaghi, 1980)

»From 1945 onwards, successive [British] goverments (whether Labour or Conservative) drew the net of state-science interaction tighter, culminating when, under the 1970 Conservative goverment, the Rotschild Report, A Frame for Goverment Research and Development, challenged the Haldane principle [according to which Reseach Councils were independent of Departments of State] head on, and, over the vociferous protests of the scientific elite, was accepted as the future basis for the management of science.« (Rose & Rose, 1976)

The majority of U. S. corporations »discourage their scientists, sometimes forbid them, from publishing the results of their work in the learned journals or communicating them in any way to scientists outside the company preserve. More inhibiting, most corporations do not let their scientists devote more than a fraction of their time [5—10%] following up problem of their own choosing«. (Whyte, 1957)

These recent examples indicate that the outer community tends not only to purchase intelectual services in order to get solutions to particular technological demands but also to restrict and jeopardize the autonomy and freedom of scientific investigations even when once established. As s response to this threat particular scientific communities try to homogenize themselves as strongly as possible imposing on their members rather strict rules of behaviour and of inner language, paradigms and ideals they share, and expecting particular traits of personalities and even common prejudices. In this sense »[t]he theory that scientists follow only the internal rules of science would seem to reinforce their efforts to prevent subordination of their work to standards extrinsic to science and protect themselves from external political influence«. (Ezrahi, 1971) At first sight it realy seems to be a well devised tactic. Thus »[t]he Philip Report [in Australia] was produced as an essentially political response aimed at defending the autonomy of science from what was perceived as primarily a threat from 'outsider' bureaucrats: »Creative production of science depends on the autonomous operation of self-imposed values and controls. It is ultimately self-defeating for a society or goverment to erode the autonomy of the scientific community«.« (Jagtenberg, 1958) However, a closer examination of behaviour, inner rules, ideal personalities and prejudices which scientists »self-impose« on themselves, reveals that there is something wrong here. For, very often event these »values« are precisely those which the employers would like scientists to have, and precisely those which the public would expect to fit into advertised stereotypes of the ideal scientists. (Holton, 1978) For example, the well-known 'fresh-blood' prejudice:

»The 'fresh-blood' argument has served the scientific managers and government administrators well, since they have been able to exploit the widely held belief that you have to be young to be 'productive' in research. The level of political consciousness (and trade union organization) among the workforce was so low that this argument passed unchalenged [though i]t was almost transparently false. The myth of the superior originality of young researchers began in 1920's when there were a few . . . 'Wunderkinder' — Pauli, Heisenberg, and Dirac — involved in creating quantum mechanics . . . The actual situation is quite different . . . The average age of Noble Prize winning physicists, *when they did their prize winning work,* is 37 years old [*including* Pauli, Heisenberg, and Dirac]. In biology the average age is even higher, 39 old when they did their work.« (Albury & Schwartz, 1982) The above mentioned »exploitation« proceeds in the following way. In Great Britain in 1979, e. g., there were 2500 PhD scientists permanently out of work compared with 10 000 employed ones. After the budget cuts of 1980 even one-third of the promoted PhD scientists remain without a job. While preparing their PhD dissertation they receive a grant from the State and do their research in an institute or university for almost no additional money. When they finish their dissertations they are already »too old« to be »productive« and the younger »apprentices« take their places. »The tenured professors [and the goverment] continue to have a source of cheap labour for their labs and the whole revolving door is justified on the grounds that one needs 'fresh blood' to sustain a research programme.« (Albury & Schwartz, 1982)

As far as the other 'values', i.e. behaviour, language, and personality, are concerned, again they are shaped in such a way to 'protect' scientific communities but at the expense of the intelectual freedom of individual scientists.

Once a field of scientific research has been established at a university or an institute, scientists (who succeeded in getting positions there) tend to achieve their further career goals through the work conducted by their research students and apprentices who are trained in precisely the same disciplines as they were, since scientists are determined to »remain in that same area of research after they graduate as they have little alternative professional capability anyway«. (Jagtenberg, 1983) As a consequence of such a protective strategy scientific communities become »far more conservative than the wider community«. And while scientific communities tend to establish as much autonomy, independence, and self-determination as possible, scientists themselves do not strive for independence and self-determination at all. Such a 'vicious circle' is typical of any group which tries to ensure its continued maintenance and respectability in society by homogenization and standardization of its members and itself.

Let us have a closer look at the typical traits of 'homogenized' scientific personalities and behaviour, and the means by which scientific communities achieve the homogenization.

Cooley (1968) reports on the longitudinal study which involved 700 persons in groups from grade 5 through to PhD, and which goal was to find atributes and traits of personalities who are likely to become and remain scientists. The study shows that scientists-to-be are »markedly low on social interests, welfare interests, and political interests«, »uninterested in dealing with people in their day-to-day work«, and highly introverted. These results are in accordance with the ones which Roe (1952) obtained studing the careers of 64 leading, male, US-born, scientists. Most of the physicists and biologists disliked and avoided social occasions, were curious about a special area to the exclusion of all else, kept away from emotional situations, reported loneliness and the existence of very few friends etc. (in great contrast to social scientists). All these reports together with the one by Davis (1965) show also that scientists as well as scientists-to-be prefer logical, experimental, and simplicity-seeking ways of thinking over affective, intuitive, and ambiguity-tolerating. It does not seem likely that these features of 'scientific personalities' are necessary prerequisites for achieving new and significant scientific results, but it does seem likely that the scientific community will grossly encourage and support them. And this indeed can be abundantly illustrated.

Studying 79 eminent scientists, Hagstrom (1965) shows that any involvement in social action on the part of individual scientists invites the risks of ostracism and isolation by the scientific community. Holton (1978), on the basis of the 1960 Project TALENT Test-Inventory (refered by Cooley and Lohnes (1968)) of almost a half-milion young people in the U.S.A., from grade 9 to grade 12, who were followed-up five years, concludes that the scientific community »help« scientists-to-be to consider logical and experimental versus intuitive and ambiguity-tolerating as two mutually exclusive ways of thinking instead of tolerably accepting their co-existence. He also shows that any attempt of drawing the attention of scientists to any humanistic or cultural concerns meets with an immense amount of disbelief, resistance, or hostility.

Jagtenberg (1983) shows that »[s]cience is not an institution which permits the free expression of the individual. Most pertinently, scientists are subject to the social control of professionalism«.

»Knorr and Knorr [1978] have analysed scientific texts as media for *constructing* reality rather than »data which represent reality«. And Zenzen and I [Zenzen & Restivo, 1982] have analysed a set of papers in colloid chemistry as *persuasive* efforts. We do not claim that scientific papers are designed to persuade people that something that doesn't exist does exist; nor that persuasion is the only aim scientists pursue in writing papers. We do claim that scientists need to use a rhetoric of persuasion in order to draw attention to and legitimate their findings. Getting a paper which sets forth claims accepted for publication i nvolves choosing the »right« words, and deciding when and how to use analogies, mathematics, systematic theory, and other resources for communicating research.« (Restivo, 1983)

The last quotation brought us to the means which scientific communities use to homogenize their members: the inner rules and propaganda.

As for the inner rules, these are the rules (of »behaviour«) scientists *be* obeying while working in a team, writing scientific publications or joining scientific meetings, and can be stated as follows:

Rule 1 »Unlikely hypotheses are to be avoided at all cost, not least because the scientific community is far less tolerant or forgiving on that score than almost any other group.« (Holton, 1978)

Rule 2 »Statements that fall into areas with a large component of not easily verifiable or of falsibiable content are frowned upon, and issues dealing with . . .

long range prediction . . . of scientific findings . . . are not expected to be raised«. (Ibid.)

Rule 3 »Concepts which cannot be analysed in terms of simple arithmetic continua are not considered scientific«. (Whitley, 1977)

Rule 4 »The desired outcome is the simple, not the complex.« (Holton, 1978)

Rule 5 A work on a problem is »a problem-solving rather than a truth-seeking activity« (Laudan, 1977), and »[o]nly afterwards details are examined for their compatibility with the system.« (Fleck, 1980)

That the rules are applied and obeyed is taken care of by peer recognition. (Holton, 1978) In case it does not suffice to establish the homogenity of a particular scientific community, propaganda can be undertaken as a more direct means to this goal.

Niels Bohr defines propaganda in physics as follows:

»[I]n physics, we carry out propaganda. When we believe we have seen something more clearly than others, we try to spread out our new insight, and that is propaganda . . . I had to argue for two years with Heisenberg and Bloch before I could convince them that the new quantum theory depends altogether on correspondence . . . It was also hard to make them and others accept the notion of complementarity.« (Nielsen, 1963)

Propaganda as a means can be witnessed throughout the history of science:

»Sommerfeld may be considered as a propagandist: Some of his activities recall the original meaning of propaganda as s missionary attitude; more generally, we may regard as »instrument of propaganda« the sum of his publications, talk and lectures . . . , since all were more or less deliberate manipulations to augment his fame and to foster his school.« (Eckert, 1987)

Thomas Young adopted »strategies of propaganda« in order to propound the wave theory of light. »The successive versions of the wave theory developed by Young did no more than deal *ad hoc* with (some of) its predecessor's refutations« (Worall, 1976) (Cf. also Laudan, 1981)

Galileo adopted »strategies of propaganda« to win converts to Copernicanism (Kuhn, 1957). »Once it has been realized that close empirical fit is no virtue and that it must be relaxed in times of change, then style, elegance of expression, simplicity of presentation, tension of plot and narrative, and seductiveness of content become important features of our knowledge. They give life to what is said and help us to overcome the resistance of observational material. They *create* and maintain interest in a theory that has been partly removed from the observational plane and would be inferior to its rivals when judged by the customary standards. It is in this context that much of Galileo's work should be seen. This work has often been likened to *propaganda* [Koyré, 1939] —and propaganda it certainly is. But propaganda of this kind is not a marginal affair that may or may not be added to allegedly more substantial means of defence, and that should perhaps be avoided by the 'professionally honest scientists'. In the circumstances we are going to consider, *propaganda is of the essence.* It is of the essence because interest must be created at a time when usual methodological prescription have no point of attack; and because this interest must be maintained . . . until new reasons arrive.« (Feyerabend, 1978) And this is literally also applicable to the previous example of the theories of light.

In the »work [which] led Dalton to the epochal concepts of the chemical atom, atomic weight, the law of multiple proportions . . . each and every one of his steps was factually wrong or logically inconsistent.« (Holton, 1978)

To cut the long propaganda story short an optimist could advise us to adcpt *Encyclopaedia Britannica's* suggestion »that a given propagandist may look upon himself as an educator, may believe that he is uttering the purest truth, that he is emphasizing or distorting certain aspects of the truth only to make valid message more persuasive, and that the course of action that he recommends is in fact the best action that the reactor could take«. And that would be a soothing happy end of curious histories of scientific achievements. If there was only one truth of particular phenomena. Since it is not so, the story goes on.

For, if truths were unique, numerous theories of numerous scientific fields the society has a need for, would all have monopolistic positions and would not be truly competitive. Thus their propaganda would really be nothing but education. Since very often there are more than one empirically indistinguishable truths of particular phenomena available, i. e. several rival theories can be formulated on the very same phenomena, their propaganda reflects their factual and vociferous competitivness and rivalry, as the elaboration in the last section will show.

AN INNER ENVIRONMENT

What does the foregoing elaboration amounts to?

In general, it seems that the outer social and cultural environment stimulates particular scientific fields but bears no direct influence on the structure of particular theories within the fields. The structure of theories is nevertheless often influenced by the environment, however only the inner one, in the process of its strugle to gain autonomy and respectability in the society.

In particular, it seems that such an influence occurs whenever rival theories, empirically indistinguishable at the time, are formulated without regard as to whether the field in question is in a Kuhnian »crisis« or not, and I shall illustrate this process on the example of the wave-particle duality of light and matter.

However, before dwelling on the case it seems unavoidable to consider Forman's (1971) thesis which boils down to the claim that the German cultural and social environments »led physicists to ardently hope for, actively search for, and willingly embrace an acausal quantum mechanics«. Stated in more detail, »suddenly deprived, by a change in public values, of the approbation and prestige which they had enjoyed before and during World War I, the German physicists were impelled to alter their ideology and *even the content* *of their science*[1] [quantum mechanics] in order to recover a favorable public image. In particular, many resolved that one way or another, they must rid themselves of the albatros of causality«. (Forman, 1971)

Forman demonstrates the last statement »on« the following neophyte physicists: Exner, Nernst, Senftleben, Schrödinger, Weyl, von Mises, Schottky, and Reichenbach. However, »Forman himself suggests that Exner's rejection of causality was independent of the milieu and of little contemporary relevance«. (Hendry, 1980) Nernst reconverted to supporting the causality ideal within few months, then insisting that causality was not only »compatible [but] even necessary to the ideals of the milieu« (Ibid.), and »Senftleben did not reject causality either« (Ibid.). Schrödinger did not work in Germany at the time. He was a Viennese and worked at the University of Zurich from 1921 until 1927 when he was appointed as Max Planck's successor at the University of Berlin. But by then he had already published all his articles in which he developed his theory of wave mechanics, and the »quantum revolution« was over in 1927 anyhow. (Mehra, 1987) Besides, he too reconverted to causality. Weyl also worked in Zurich from World War I until 1928. Schottky and von Mises did support acausality but as indeterminism and not as lawlessness. However, what is more important is that neither Schottky nor von Mises had contributed to the quantum theories developed in these years. The same is true of Reichenbach who then taught philosophy of physics at the University of Berlin and contributed only to the interpretation of the already established quantum theory, in 1928. (Jammer, 1978)

In other words, if we consider the 'content of their science' (quantum mechanics) to mean the 'particular quantum formalism' elaborated at the time, then the thesis that such a content was influenced has been all but proved by Forman's analysis, and having in mind »heavy« mathematics underlying the quantum formalism this was to be expected. Besides, even if we agree to work realy hard to prove the thesis by finding out hidden and indirect influences of the environment we shall soon find ourselves struck by how few theoretical physicists Germany had at the time let alone the number which took an active part in creating quantum theories. In 1898 Wilhelm Wien wrote to Arnold Sommerfeld: »Theoretical physics in Germany is as good as finished. The resons for this are, in the first place, that physicists pursue pure experiment almost exclusively and are not interested in theory . . . This is shown externally by the fact that pure theoretical physics has only two chairs (Berlin and Göttingen) and so important a chair as Munich [which has been established for Boltzmann in 1890 and abandoned after Boltzmann's departure in 1894] has ceased to exist. At present, theoretical physics has no takers.« (Eckert, 1987) In the mid twenties we find the situation improved. In Munich were Wien, Sommerfeld, Albrecht Unsöld (born 1906) and Fritz London (1900?), in Tübingen was Alfred Landé (1988), in Leiden Paul Ehrenfest (1880), in Hamburg Wolfgang Pauli (1900) and W. Lenz (1900), in Frankfurt Cornelius Lanczos (1893) and Erwin Madelung (1880), in Göttingen Hertzberg, Norheim, Max Born (1928) Eugene Paul Wigner (1902), Werner Heisenberg (1901), Pascal Jordan (1902) and Walter Heitler (1904), and in Berlin Albert Einstein (1879),

[1] my emphasis

Max Planck (1858), Walther Hermann Nernst (1864), Arthúr Korn (1870), and James Franck (1882) (Schottky worked at Siemens, and Exner in Austria). An active part in creating the quantum formalism in the mid twenties was taken by Born, Heisenberg, Pauli, Landé, Jordan, Sommerfeld, Korn, Lanczos, Madelung, Lenz, and Unsöld.

Having in mind their age (especially before World War I) as well as the fact that they were just giving birth to a completely new science one cannot but wonder about the meaning of Forman's claim that »suddenly deprived, by a change in public values, of the approbation and prestige which they had enjoyed before and during World War I, the German physicists were impelled to *alter* [my emphasis] ... the content of their science«.

We can partly save Forman's analysis, as proposed by Hendry (1980), taking »attacks upon ... physics [in Germany in the twenties] from outside [this] discipline ... [as] attacks upon [its] *value,* rather than upon [its] *content«,* and accepting that in the »semi-popular addresses discussed by Forman ... physicists ... naturally used the language of the milieu, and justified the pursuit of their subject in terms that could be understood and appreciated by those who were questioning its cultural value« (Hendry, 1983).

However, understood in this way, it is hardly a language specific to a particularly German environment since it coincided with analogous languages which new-born quantum physicists used in Switzerland, Denmark, England, France and the USA.

Thus, it seems more appropriate to search for roots of acausality issue within the physicists' community and their theories. In doing so, we first have to clarify the very concept of acausality. Forman himself, admits that acausality is very often used as synonymous to indeterminism, and not to denote a specific quantum lawlessness. This narrow sense of acausality is, however, not what was at stake either in the creation of quantum mechanics or later on, for several reasons. First of all, from the very »quantum begining« it was almost obvious that quantum mechanics, as accepted in the late twenties, cannot be reduced to a deterministic theory. Or, stated in a professional language: »No existing so-called 'hidden variable theory' is a counter-example to von Neumann's [so-called 'impossibility'] proof.« (Bub, 1969) On the other hand, the so-called Bell-type experimental disprovals of hidden-variables have nothing to do with the existing hidden-variable *theories* as ever formulated by their propounders (e. g. David Bohm, N. Wiener, A. Siegel, J. H. Tutsch, etc), since they are conceived to disprove the *local* hidden variables which were only ever formulated by Bell himself.[2] And, as far as the more recent situation is concerned, it has been known for over two decades that classical Newtonian mechanics is not necessarily deterministic either (Tritton, 1986; Miles, 1984; Chirikov, 1979). »Modern theories of dynamical systems have very clearly demonstrated the unexpected fact that systems governed by the equations of Newtonian dynamics do not necessarily exibit the 'predicability' property. Indeed,

[2] Bell himself wonders: »why did people go on producing 'impossibility' proofs. after 1952 [when Bohm formulated his hidden variable theory], and as recently as 1978? When even Pauli, Rosenfeld, and Heisenberg, could produce no more devastating criticism of Bohm's version than to brand it as »metaphysical« and »ideological«?« (Bell, 1982)

very recent researches have shown that in wide classes of very simple systems satisfying those equations, predicability is impossible beyond a certain definite time horizon.« (Lightill, 1986)

What we are, therefore, left with, is acausality in its wider sense of lawlessness. But is it not a strange theory, the one without laws? Yes, indeed it is, and that is the point. Let me elaborate the claim in some detail.

CASE STUDY: THE WAVE-PARTICLE DUALITY OF LIGHT AND MATTER

By the end of the seventeenth century, when the corpusculuar interpretation of light was embraced, »Newton's doctrine of *verae cause* . . . was thought to exclude the postulation of any entity or process not strictly observable«, and it was held that »scientific theories must deal exclusively with entities which can be observed or measured«. (Laudan, 1981) Newton himself had not published a single of the numerous articles he had written upon aether. However, towards the middle of the eighteen century the situation changed. »In 1745, Bryan Robinson published his *Sir Isac Newton's account of the aether.* A year later Benjamin Willson's *Essay towards an explication of the phenomena of electricity, deduced from eather of Sir Isac Newton* appeared . . . By the 1760's, the scientific literature abounded with etheral explanations of heat, light, magnetism, and virtually every other physical process. (Laudan, 1981) As we would only expect, on the ground of the physical community's rules No. 1, 2 and 3, mentioned above, such »explanations« and their propounders were severely attacked by their colleagues, and more or less ostracised from the inner community. (Ibid.) Yet, Laudan (1984) claims that »many working scientist in the late 1700's and early 1800's [decided] to give up the view that we should seek to restrict our theories entirely to claims about observable entities and processes«, and that »this important shift in cognitive orientation was absolutely essential to the development of [many new] theories«, among them those based on the aether assumption, and, in particular, the wave theory of light, which was put forward by Thomas Young and Augustin Fresnel at the beginning of the 19th century. (Laudan, 1981) What, in my opinion, Laudan has not taken into account is the development of the theory of elasticity, the fluid mechanics and acoustics in the 18th century. These theories provided differential equations, that is a mathematical tool which could have been applied to any similar problem, and aether was of such a kind. Therefore, according to the afore stated rule No. 5, physicists were allowed to study aether formally and mathematically if for no other reason then to enable the community's decision on the rule No. 2 regarding any given etheral theory. For, if a theory can be elaborated mathematically then it can be considered scientific according to rule 3. In the »light« of these rules the light story can be presented as follows.

In 1803 Young reported to the Royal society on two experiments: a special kind of diffraction experiment and his famous 'double slit' experiment. Now, the diffraction experiment was an old one, performed for the first time in 1665 by Grimaldi, and later on by Newton, Hook, and others, and was successfully »explained« by corpuscularians by short-ranged forces emanated from the body and acting on the light corpuscles, i. e. deflecting them from their natural rectilinear path. (Worrall, 1976) The 'double slit' experiment was, on the other hand, so subtle an experiment that no one had succeeded in repaeting it, and, besides, it could have been »explained« in a 'corpuscularian's manner' as well. In order to convince the physicists' community that the wave proposal was worth considering, Young should have been able to offer a 'calculus' which would have predict the experimental outcomes. However, Young had never elaborated his proposals in a quantitative way. Too many community rules were thus violated and, as a result, he was attacked and his work ignored. Here, we should bear in mind, as the gist of the dispute, that »[n]either the corpuscular nor the wave programme had had anything in the way of *predictive empirical success* up to this time; both programmes were in the business of *post hoc* explanations.« (Worrall, 1976) Since corpuscularians were in power it was they who should have applied the 'rules' and ostracised opponents.[3]

Thus, it does not seem likely that Laudan (1981, 1984) is right when he considers the abundance of aether theories as a sign that scientists opened their minds. What does seem likely is that scientists have always considered that any entity which cannot be quantitatively elaborated *ought not* be their concern. As soon as a new theory can predict particular experimental results, no matter whether the theory assumes the existence of an entity which is not strictly and directly measured or not, they become interested in it provided the theory predicts some results which are not predicted by the existent theories. Further development of the wave story illustrates this well.

In the late 1810's Fresnel »*elaborated* [my emphasis] the wave theory of light and applied it to the explanation of diffraction [and interference] phenomena. Poisson, a confirmed corpuscularian and a member of a panel refereeing Fresnel's paper, observed that, according to the analysis of light which Fresnel was using, it would follow that the center of the shadow of a circular disk would exhibit a bright spot. This predicted result was highly unlikely; it contradicted both the corpuscular theory [actually it contradicted only corpuscularians' expectation, as their theory had not treated such a possibility previously], *and* the scientist's intuitive sense of what was 'natural'. Indeed, the fact that the wave theory possessed this bizzare consequence was seen, *prior* to performing the experiment, as a kind of *reductio ad absurdum* of it. But when the appropriate tests were performed, the wave theory was vindicated by a concordance between what it predicted and the observed facts.« (Laudan, 1981) Upon further investigation Fresnel found that aether as the carrier of light waves should exhibit properties analogous to the ones then known to be ascribable to an elastic media as a carier of transverse waves. And by that time Claude Navier and Luis Cauchy formulated the equations which

[3] An optimist would here remark that it could have been the other way round, as the wave interpretation has been known since even before the 1650, especially had Newton with his authority published his etheral elaborations of natural phenomena. The »other way«, however, had been unlikely to occur not because of casual personal preference, but because of the absence of the calculus for waves at the time when corpuscles had, at least as a possiblity, Newtonian mechanics at their disposal.

described waves in an elastic media, which were applied to light waves by Denis Poisson, Franz Ernst Neumann, and Oeorge Green. They however faced difficulties in finding such properties of aether which would ensure such boundary conditions of the equations as necessary to give appropriate transversal waves as solutions. To overcome these difficulties James MacCullagh concentrated on the transversal waves in no-matter-which-kind of aether and obtained the wave equation which later on James Clerk Maxwell obtained as well. In a word, by the mid 19th century the modern wave optics has been, in effect, established. That is, *prior* to the *experimentum crucis* performed by Leon Foucault in 1850. Namely, until the 1820's as we have already stressed, both the wave and the corpuscular theories provided explanations for the performed experiments *post hoc.* For the Fresnel-Poisson disk-shadow-with-a--bright-spot experiment which the undulationists *predicted,* the corpuscularians were still able to provide at least *post hoc* explanation (Worrall, 1976). But there was a point in which they contradicted each other in predictions *in advance.* According to the wave theory, in fact already according to Fresnel's elaborations in the late 1810's, velocity of light in a media denser than air should be smaller than its velocity in air, while according to the corpuscular theory it should be the other way round. Foucault's experiment in 1850 decided in favour of the wave theory almost 30 years after its acceptance by the physicists' community.

In the second half of the 19th century James Clerk Maxwell formulated a general theory of electromagnetism which included the wave theory of light thus interpreting the light wave as an electromagnetic wave, and which did not include any formal consideration of properties of aether. In his *Treatise* he mentions aether only once, on page 782, stating, in effect, that aether is neither in contradiction to nor plays any role in his theory.

In 1887 Albert Abraham Michelson and Eduard Morley performed the so-called Michelson-Morley experiment in order to determine the absolute velocity of the earth through aether and the result was null. Hendrik Antoon Lorentz introduced the so-called Lorentz contraction to explain the result in a way that would not contradict the aether hypothesis. In 1905 Albert Einstein formulated the special theory of relativity which covered both the null result and the Lorentz contraction and had no bearing on aether. Since it was immediately clear that Maxwell's theory of electromagnetism is not in contradiction to the new theory it looked as if the wave theory of light would stay intact. However, »[i]n Einstein's paper on light quanta of 1905 the corpuscular structure of light was developed from a formal analogy between gas fluctuations and radiation fluctuations«. (Darrigol, 1986) The corpusculuar structure of light could explain the photoelectric effect (first discovered in 1887 by Heinrich Hertz) for which Philipp Lenard's experiment in 1902 showed to be in opposition to Maxwell's theory, but at the same time it brought the old wave--particle dispute back to the stage.

Einstein himself did not like the idea of the corpuscular structure of light, i.e. the light quanta or photons, and he had already in 1909 shown that Maxwell's equations might yield pointlike singular solutions in additions to waves.

A new decisive experiment was needed. And was provided by Arthur Holly Compton in 1922. The direct scattering of photons by electrons, with a recoil of the electron has been recorded, and the corpuscular structure of light confirmed.

At the same time (1922—23) Luis de Broglie, inspired by Einstein's 1905 and 1909 papers, approached the problem from the opposite side. If light can exhibit corpuscular properties, could we not also expect of the proper particles to exhibit wave properties. He immediately obtained an impressive confirmation for the idea: the Bohr-Sommerfeld quantization could be interpreted as a resonance condition for waves along closed orbits and the assumption was confirmed experimentally in 1925 when a diffraction of electrons was recorded. On the other side he carried Einstein's 1909 idea further and in 1926 developed 'new optics of light quanta' as 'mobile singularities'.

In 1925 Heisenberg, Born, Jordan, and Pauli formulated the matrix quantum theory, and in the same year Paul Adrien Maurice Dirac developed his quantum algebra which he proved equivalent to Heisenberg-Born matrix formulation.

In 1926 Erwin Schrödinger created the wave mechanics and showed it to be equivalent to the previous two formulations.

At the same year Max Born interpreted Schrödinger's waves as probability amplitudes, but Schrödinger did not like it. For, he, together with Einstein, Planck, and de Broglie, did not like »all this quantum jumping« (Springerei). After Max Born put forward the interpretation, he wrote to him: »I have, however, the impression that you and the others, who essentially share your opinion, are too deeply under the spell of those concepts (like stationary states, quantum jumps, etc.), which have obtained civic rights in our thinking in the last dozen years; hence you can not do full justice to an attempt to breake away from this scheme of thought«. To Niels Bohr he wrote: »one should not, even if a hundred trials fail, give up the hope of arriving at the goal — I do not say by means of classical pictures, but by logically consistent conceptions — of the real structure of space-time process. It is extremely probable that this is possible«. (Mehra, 1987)

Along similar lines of reasoning, in a letter to Born in 1926, »Einstein had regarded the electromagnetic wave fields as a kind of »ghost field« whose waves served to guide the motion of corpuscular light quanta«. (Ibid.)

In 1926 Erwin Madelung proposed the hydrodynamic form of quantum mechanics, in which, he claimed, »the current problem on quanta has found its solution in a hydrodynamics of continuously distributed electricity«. (Jammer, 1974)

In 1927 Niels Bohr put forward the 'principle of complementarity' which states that quantum objects can not simultaneously exhibit both their wave and their particle aspects. This principle has been later supplemented by the claim that the matrix-wave formulation of quantum mechanics completely decribes not only ensembles but individual quantum objects as well. Such an interpretation of the bare matrix-wave quantum formalism is often called the Copenhagen interpretation.

Everyone of the last three theories had a serious flaw when compared with the minimal matrix-wave (Hilbert space) theory (whose »minimality« was, 40 years later, called the 'statistical interpretation'). In de Broglie's theory classical features of quantum objects cannot, out of principle, be observed. They are assumed to be hidden at least with regard to the existent kinds of measurements. However, the most important flaw of the theory was additional complications in formalism which were superfluous for any application at the time. The same is valid for Madelung's theory which in addition to this assumes the existence of aether. The Copenhagen interpretation, in the end, also ascribes some properties to quantum objects which cannot be proved. Namely, if the corpuscular aspect of an individual quantum object cannot be defined while the object is exhibiting its undulatory aspect then the claim that the object is nevertheless completely described by the minimal quantum theory remains extrinsic with regard to the theory if it cannot be proven within the theory as it cannot. This fact illuminates the sense in which the physicists referred to by Forman used to ascribe acausality to quantum mechanics when they did not take acausality as synonymous to indeterminism but in its proper meaning of lawlessness. Here I have in mind Born, Heisenberg, and Sommerfeld, i.e. the only physicists quoted by Forman (1971, pp. 105—107) who no sooner than, in 1927 gave the notion of acausality a tone of lawlessness. They obviously did not have in mind de Broglie's or Madelung's causal description of quantum objects but simply formally undescribed and in *this sense* lawless *individual* quantum objects, that is the Copenhagen interpretation. The clue of such lawlessness is the following.

When we prepare individual quantum objects (e.g. photons) by a preparational device (polarizer), one by one, then we are *not able* to *predict* whether we shall detect the prepared property on each particular object by a detection device (analyzer deflected at an angle with regard to polarizer) or not. What we *are able* to *predict* is the percentage of objects which are going to exhibit the prepared property (that is, we are able to predict the intensity of the light beam). Thus, decisive laws for statistical ensembles of quantum objects exist, they are empirically confirmed, and in *this sense* quantum mechanics is *causal*. There have been particular laws formulated by de Broglie, Madelung, their successors, and others (see below) for individual quantum objects but it does not seem likely that they can be conclusively confirmed by the existent measurements (though some experiments are currently under way in France and Italy), and in *this sense* quantum mechanics is *acausal*.

In 1927, according to the rules no. 2 and 4, de Broglie's and Madelung's causal (in both individual *and* statistical sense) quantum theories were excluded from the physicists' community soon after they were formulated: de Broglie and Madelung gave up pressed by the community (Jammer, 1974). According to rule 2 the Copenhagen interpretation should have been excluded as well. However, the minimal formulation, feared the physical community, would leave too much space and would perhaps attract physicists to investigate further instead of directing them to apply the new theory where needed. Thus, after considerable propagandist efforts on the part of Bohr and his collaborators, the Copenhagen interpretation was embraced.[4]

Today, there are again many quantum theories. In the meantime it has been ensured that this is just a business of producing new formalisms which will not provoke unproductive ontological disputes. Causal de Broglie's theory was reestablished in the 1950's and developed further, various phase space and fuzzy phase space theories were, mainly for application in chemistry, developed from the 1940's on, various stochastical theories have been developed mainly as a result of the development of the statistical electrodynamics, and some of them can be regarded as a continuation of Madelung's theory. (For references see Jammer (1974) and Pavičić (1982))

Taken all together it seems that there is no influence on the structure of scientific theories from the outer environment and that an influence is apparent in the inner environment whenever rival theories are concerned. Namely, the inner community chooses, acording to its rules, one of the rival theories as the official one. It largely affects further education and the free intellectual will of individual scientists, although they are usually not aware of this fact by virtue of such an education. Even when outer demands obviously change their scientific interest scientists are ready to accept this fact as their own decision or at best call it a »fashion«. Yet, such an influence does not affect the very formal structure of scientific theories. Namely, literally a theory of natural phenomena cannot be influenced except in the above described way when there are rival theories, and, on the other hand, today there is almost no field which is completely barred.

However, the overall dynamics of a scientific field obviously depends upon the number of scientists engaged. In other words, had we been more democratically educated and given possibility of free choice we would not have had our 'democratic Western society', and in this sense we can say that even the outer environment influences the structure of scientific theories.

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[4] John Steward Bell (who is convinced that quantum mecanics is complete in the abovementioned sense and who devised a special type of experiments to prove this) asked rhetorically: »But why . . . had Born not told me of this »pilot wave« [de Broglie's »ghost wave« from 1927]? If only to point out what is wrong with it? . . . Why is the pilot wave picture ignored in text books? Should it not be taught, not as the only way, but as an antidote to the prevailing complacency? To show that vagueness, subjectivity, and indeterminism, are not forced on us by experimental facts, but by deliberate theoretical choice.« (Bell, 1982) (Cf. also footnote № 2)

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MLADEN PAVIČIĆ

Fizikalne teorije i njihova društvena i kulturna okolina

Razmatrano je da li društvena i kulturna okolina prirodnih nauka, posebno fizike, može utjecati na strukturu njihovih teorija. Pokazuje se da na pojedine teorije okolina utječe, međutim, ne direktno, već preko znanstvene zajednice u procesu njene borbe za autonomiju i položaj u društvu. Posebno, takav utjecaj postoji kad god se pojave »suparničke« teorije koje su u nekom periodu empirijski nerazlučive, bez obzira na to da li je odnosno znanstveno područje u Kuhnovskoj »krizi« ili ne. Mehanizam pomoću kojeg se znanstvena zajednica homogenizira naspram šire društvene okoline, praveći utilitarističku i pragmatističku selekciju među suparničkim teorijama, je razrađen. Proces je ilustriran primjerom valno-čestične dualnosti svjetlosti i materije.