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A CONCEPTUAL ANALYSIS OF THE PROBLEM OF THE RELATIONSHIPS BETWEEN BASIC RESEARCH AND APPLIED WORK IN PSYCHOLOGY: THE EXAMPLES OF EXPERIMENTAL AND APPLIED BEHAVIOUR ANALYSIS *

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Using Wittgensteinian methodology of conceptual analysis we show how the problem of the relationships between basic and applied psychological research has been considered in an inappropriate way, as a direct extrapolation of scientific knowledge for the solution of behavioural problems. The results of the conceptual analysis carried out allow the reformulation of the problem, recognising that: a) in the case of psychology, at least three ways of knowing can be identified; b) these are relatively independent of one another by virtue of their conditioning factors and their objectives; c) they are continuously interrelated through everyday language. The three ways of knowing are described, before moving on to an analysis of the example provided by The Experimental Analysis of Behaviour and Applied Behaviour Analysis. This analysis shows how two thematic fields, reproducing the logic of the direct extrapolation of scientific knowledge for developing a behavioural technology, constitute closed, isolated units, with almost negligible mutual communication. Some solutions to this problem are suggested in the light of its reformulation.

Mediante la metodología del análisis conceptual wingensteiniano se muestra cómo el problema de las relaciones investigación básica- aplicaciones en psicología se ha planteado de forma inadecuada, como una cuestión de extrapolación directa del conocimiento científico para la solución de problemas conductuales. Los resultados del análisis conceptual realizado permiten reformular el problema reconociendo que: a) en el caso de la psicología pueden identificarse al menos tres modos de conocimiento; b) relativamente autónomos unos de otros por sus condicionantes y objetivos; c) que se interrelacionan entre sí continuamente mediante el lenguaje cotidiano. Se caracterizan los tres modos de conocimiento identificados para pasar a analizar el ejemplo proporcionado por el Análisis Experimental de la Conducta y el Análisis Conductual Aplicado. Dicho análisis muestra cómo ambos campos temáticos, que reproducen la lógica de la extrapolación directa del conocimiento científico para desarrollar una tecnología conductual, constituyen islotes cerrados en sí mismos, que establecen comunicación apenas nominal. Se sugieren algunos modos de remediar este problema a la luz de la reformulación efectuada del mismo.

Among those working in behaviour modification, there exists the basic assumption that this thematic field developed as a logical extension of the data and theories of experimental psychology in general, or of the theory of learning and animal conditioning in particular (e.g., Franks, 1991). From the outset it has been considered, in the fields of behaviour modification and of psychology as a basic science, that the relationship between the two thematic fields is unidirectional: professionals and researchers interested in applications are more or less passive receivers of a basic *knowledge* about relationships *of a general kind* among variables, which allows the explanation, with varying degrees of success, of human behaviour. If this were the case, and *irrefutably so*, the present work would make no sense, for everything would have been said on the matter and there would be no point in going on. In order to solve the problem of the divorce between basic research and application it would be enough just to leave it where it was, and fall back as a possible solution on, for example, the formal analysis of the theories that support beha-

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viour modification following the canons of the theory of science, as do O'Donohue and Krasner (1995 a, 1995 b).

However, things cannot be so deceptively simple. Other, perhaps more fruitful points of view are also possible. In fact, we shall start out from the basis that there are no such things as a basic and/or academic psychology and an applied psychology; that these are no more than names that often, through the magic of language, appear to refer to irrevocably and perfectly established facts that are, nevertheless, merely arbitrary creations. We shall assume from the outset that the history of relationships in psychology between basic research and applications is nothing more than the (historical) chronicle of a (secretly) foreseeable divorce that must be traced from the very moment of their crystallisation as *projects* of science and technology, respectively, up to the present time, in which our disciplines have neither their own model of how to approach scientific, technological and applied work, nor an object of study, nor what Kantor called a meta-system (Kantor, 1959). In order to develop our arguments on the basis of these premises, we shall use the methodology of conceptual analysis, since we consider the nature of the problem of the basic research-applications relationship to be, like so many others in psychology, conceptual.

Consider the opening paragraph. The expressions used in it are not neutral. Terms such as "basic research", "applied work", "knowledge", "explain", "professional(s)", "knowledge of a general kind", do not convey precise meanings, because the context in which they are used is loaded with a sense that we must strip away in order to correctly identify conceptual questions that usually remain hidden *by our familiarity with the use of these terms*. We shall try to discover what the meaning of these expressions might be in the context of traditional discourse about the relationships between basic research and applications.

The terms "basic research" and "applied work" appear to make reference to two autonomous and independent fields of activity, one of which (basic research) constitutes the ultimate and irreducible segment of a sector of reality, which in our case is human and animal behaviour, whilst the other (applied work) consists in the application of *knowledge* to the solution of some relatively well-defined problem. "Professional(s)", another common term, refers to the existence of a clearly-delimited thematic field of work to which any speaker can meaningfully refer, in this case the field of the applications of psychology. Our language appears to take for granted that two clearly-defined thematic fields exist, so that we need two different nouns to refer to them. In fact, to talk about the products of basic research knowledge, we use the expression "knowledge of a general kind", stressing the abstract and generic nature of this thematic field. Paradoxically, in spite of the different categorisations established by our language, these two thematic fields are supposed to be closely coordinated, with one (basic research) constituting the matrix *knowledge* that *legitimates* the other (applied work). And this coordination is based on extrapolation, on the direct appropriation of *knowledge* from basic research by applied work.

We might now ask: What assumptions underlie such uses of these terms in this context? What conclusions *are we obliged* to draw by the context of use just described? Let us look at some possible ones:

1. *There exist a psychology as scientific knowledge and a psychology as the application of that knowledge and/or as professional practice* ***that are perfectly constituted and articulated***. This assumption, explicitly stated in textbooks (e.g., Fernández Trespalacios, 1997; see Chap. 1) does not do justice to the reality we encounter *at the current historical moment* of psychology. The multiplicity of methodological conceptions and traditions in both thematic fields constitutes the rule more than the exception, in the background being the problem of the undefined nature of psychology's object of study. Such an assumption ignores the *historical* fact that psychology, at the very moment of its constitution as a science, found itself trapped in a twofold problem. On the one hand was the need to study the rational processes of knowledge, a problem approached within the academic environment and at the time of its inception as an independent scientific discipline, and on the other, scarcely articulated and/or delimited heterogeneous social demands, arising in some cases as problems at the margin of other perfectly articulated professions such as medicine or teaching, or as problems arising from new needs, such as those presented by engineering or the selection of specialized personnel. Thus, two fields developed, one corresponding to an ill-defined academic problem, without object of study or agreed meta-system, and the other to the contexts of problems socially defined as such that demanded a solution (Ribes Iñesta, 1989). Given this historical situation, a divorce between the two thematic fields is practically inevitable and irreversible. Not only, *at this point in history* do we lack two well-defined thematic fields, for on top of this there are no conceptual or methodological links between them, except for the flavour given by the use of concepts and procedures that have become deta-

ched from their origin. And it is doubtful, therefore, that there exists professional practice in the traditional sense of “professional”, unless we are to identify it, as has been the case, with specific practice contexts and with what psychologists do (Ribes Iñesta, 1982 a), a poor option conceptually, organisationally and socially speaking, given the problems it generates.

2. *Ontologically, it is possible to speak of the existence of an object, process or faculty, “knowledge”, that is independent of its specific circumstance and easily transferable in a transfer process considered as a set of point-by-point correspondences.* This assumption is logically possible if we start out from openly dualistic premises. However, the dualism implicit in the conception of knowledge as a universal ability raises various problems that are insoluble from its perspective. In the first place, if knowledge is universal, by definition it cannot possess an historical dimension that is easily demonstrable (see Fleck, 1935; Turbayne, 1970, 1991). Secondly, *knowledge* is often identified with *product*, which in the case of scientific knowledge may be a set of data, a statement such as a law, an explanatory model, etc. However, on being identified with specific products, we lose sight of the process, the operations carried out in context to *elaborate* these products. Furthermore, the dualistic conception of knowledge is contradictory when it treats it as a set of stored products and, at the same time, conceives of them representationally, that is, as more or less vivid reflections of an objective reality external to the subject knowing. And this contradiction is avoided, apparently, on locating on an internal level the operations leading to the production of representations, a leap in the dark that is observed in recent constructionist versions of cognitive psychology, and which in no way saves the dualistic conceptions of knowledge from their inherent difficulties (Robles Rodríguez, 1996). Thirdly, knowledge appears to be strongly contextualised, even if it involves such abstract and academically prestigious fields as calculus, as Lave (1988) has recently demonstrated.

If we assume that knowledge possesses an historical dimension and is exercised as operations, as an activity in context, on referring to it we cannot speak with any sense if we treat it as a product or as a materialised and/or reified object or thing, that is, decontextualised and therefore synchronous. “Knowledge” should be a term reserved to refer to a concrete product or event, such as a law-type statement, or an expression such as “I knew that Juan came yesterday.” In contrast, *to know*, in its double sense as a verb of action and a verb of relation (Ribes Iñesta, 1990 a), should be reserved for two uses. As a verb of action it should refer to the concrete activities carried out in a certain context that gives meaning to these actions as *knowing*; e.g., on referring to the exploratory behaviour of a infant on becoming acquainted with a vase for the first time, or the computer operations necessary for activating a computerised laboratory. As a verb of relation, it should be employed to refer to those circumstances in which we relate a set of actions (of knowing or not) with special circumstances and events; e.g., when we make a theoretical interpretation of the “getting to know” process of the boy that is handling a vase, we relate his concrete responses to the vase with the context in which these responses take place (in the presence of his mother who has given him the vase, and who uses expressions such as: “What a clever boy!”, “Isn’t it nice and smooth?”, “Look how white it is!”, etc.).

In this sense, we cannot consider *teaching* as a simple process of correspondence and/or transfer. Knowledge is not transferred in the same way as, for example, we transfer money to a bank, we transmit an e-mail message or we make a phone call. Actions and the coordination of multiple actions with special circumstances and events take place within spatial coordinates, but they are not spatial objects, given the fact of their being transitory in time, and their relational and/or interactive nature. Rather, what are taught or learned are ways of proceeding or interacting, both conceptual and empirical, *contextualised in a conceptual tradition and a socio-historical moment*, and which are taught in the actual process of carrying out those actions, such as operating a computer, consulting a book, implementing an assessment or intervention procedure, drawing conclusions from a set of data, etc.

3. As a corollary to the previous point, *knowledge is neutral and is devoid of ideology as long as it is decontextualised and ahistorical*. The products of knowledge and knowing as action and as relation, on possessing an intrinsic historical dimension and a context, cannot be neutral, that is, removed from the conditions in which they appear and are carried out. If they are neutral, on being taken out of context, the products of knowledge could not generate any tension since they constitute a (representational) reflection of a reality that is objective and/or external to the subject knowing. Moreover, scientific knowledge as a product is merchandise like any other, subject to the rules of capital (Talento and Ribes Iñesta,

1980). The products of knowledge and knowing as action and relational operation cannot be bereft of ideology, understanding ideology in a wide sense, as cultural practices transmitted by use and custom. Lakoff and Johnson (1980), have shown the ideology underlying the use of terms and expressions of emotion (e.g., "to be happy is to be up; to be sad is to be down") and specific terms such as "argument" ("an argument is a war"). In this sense, ideology implies belief, ways of life. To use the expression of Ortega y Gasset (1986), "*we have ideas, we are beliefs*". Thus, it can be said that we live a metaphor, and we actually do live it, if we consider that an argument is like a war, following the example of Lakoff and Johnson: we do not really talk to our fellow arguer, we try to convince him that he is mistaken, bringing up one reason after another, raising our voice, using subtle rhetorical resources, and so on. All with the aim of showing him how wrong he is and finally coming out victorious. In this sense, we have been dominated by the metaphor on taking it as the literal truth, as the world itself as it presents itself to us (Turbayne, 1970).

This problem becomes especially important when we consider the question of the nature of the specialized technical languages used in science. In the specific case of psychology, understood in its double sense as basic and applied science, the technical terms available are taken directly from everyday language (Ribes Iñesta, 1990 b). However, this process of appropriation of psychological terms and expressions from ordinary language has historically remained *implicit*. That is, it has been taken for granted that, e.g., because the term "perception" exists, e.g., there exists a process or processes identified by this name, the use of this term thus being legitimated as part of the technical vocabulary. Hence the importance of being aware of the problem, since the logical and ethical implications of this tacit ideology continue to operate even when we are unaware of it, generating important tensions even in fields of psychology that have apparently developed a truly technical vocabulary from the very interior of the discipline, as is the case of The Experimental Analysis of Behaviour (TEAB). For example, in questionnaire-based research among the members of the editorial boards of several journals in the TEAB field (such as the *Journal of The Experimental Analysis of Behaviour*, the *Journal of Applied Behaviour Analysis* and *The Analysis of Verbal Behaviour*) on the use of the terms "reinforcer" and "discriminative stimulus", Schlinger, Blakely, Fillhard and Poling (1991) found that researchers working in human behaviour used the two technical terms in a way inconsistent with their original formal definitions when they overlooked the temporal parameters (contiguity). These results point to the possibility, not considered by their authors, that these technical terms are being employed in a merely formal way when researchers deal with specific characteristics of human behaviour which, though well recognised by them, are not taken into account by the use and type of technical terminology available. A second example, though in the context of psychopathology (another apparently well-defined language), is provided by Berrios and Fuentenebro de Diego (1996) in their historico-conceptual analysis of the symptom *delusion*. These authors show how the diverse conceptions of delusion rest on the use of an everyday term that is employed differently according to the native language of each of the authors that have worked on the description and explanation of this symptom, with diverse and complex implications that, nevertheless, continue to anchor its study to rationalistic conceptions of delusion as a perfectly structured belief, with true value and replete with information on its etiology.

It would appear, then, that we have identified a tremendous conceptual confusion that actually constitutes the problem of the relationships between basic research and applied work. Our next task is to reformulate this problem. In order to do so, we shall first identify what its instrument might be, the conductive thread of it all, so that, as Turbayne would say, we can tear off the mask and see the face of the problem. This instrument is none other than everyday language understood as social practice among individuals in context. Our position with respect to the above is well stated by Ribes Iñesta (1990 c):

> "Science constitutes a specific *way* of knowing, a way characterised by a way of proceeding in the formulation, systematisation and legitimation of concepts. Nevertheless, even if the *content* of science is peculiar to the analytical and thus abstract nature of its concepts, it permits, insofar as its language is based on ordinary language, that other ways of knowing (religious, artistic, political, technological) *appropriate* this content and transform, deform and adapt it to other social uses. It is in this way that ordinary language becomes established as a *means* of appropriation of *content* by diverse social *ways* of knowing. There exists a trade route between the contents of science and other ways of knowing maintained *by means of* the articulatory social instrument constituted by ordinary language" (Ribes Iñesta, 1990 c, pp. 24-25; original author's italics).

If this is to provide a reasonable starting point, we therefore need, in order to try and reappraise the problem of the relationship between basic research and applications, among other things, to describe the everyday activity of individuals in contextualised interaction that carry out their activities in laboratories and clinical settings. With an initial analysis such as this we assume that, *in the way the problem of the relationship between basic research and applications is put, the problem does not make sense*. What we do believe to make sense as our discipline currently stands is to describe how and in what conditions there is an everyday interchange of knowledge contents between ways of knowing that are in principle divergent due to their relatively autonomous nature, resulting from their different conditioning factors and basic objectives.

Arising from the above conceptual analysis are the theses that we have formalised in the following way:

1. The relationship between basic research and applications in the field of behaviour modification has been conceived as a unidirectional and direct extrapolation of both procedures and of empirical relationships isolated in the laboratory (Ribes Iñesta, 1977). This extrapolation is possible, among other reasons, if we start from the assumption that there exists a psychology as a consensus-based scientific discipline, and an applied psychology properly delimited by an indisputable social commission. These two basic conditions are not fulfilled at the current time in the historical development of psychology.
2. This type of extrapolation and/or generalisation is *logically possible* as long as we assume *in an implicit way* that knowledge is universal; that is, that it exists in a way that is decontextualised from practice and the contexts in which it is deployed, constituting a kind of faculty or unitary processing device, or however we wish to call it (Lave, 1988). In other words, it has been assumed historically and in an implicit way that only one type of representationally-conceived type of knowledge –the rational one, of course– is possible and legitimate.
3. To conceive of knowledge in such a dualistic way generates tensions of all kinds, within and between the different ways of knowing that are to be coordinated, since this conception does not recognise (nor permits their recognition) the different practical contexts of interaction of individuals that earn a living working in the two conflicting fields. These contexts mediate/organise activity within each way of knowing and the relationships between them. A dualistic conception of knowledge is blind to ideology and to the historical dimension of knowledge (Ribes Iñesta, 1990 c), and therefore does not recognise that it is itself impregnated with ideology.
4. Knowledge should be considered in terms of three different senses or uses: a) knowledge as a product, for which we reserve the term *knowledge*; b) knowledge as a verb of action, for referring to *knowledge acquisition actions or operations*, in contexts identified as being concerned with obtaining knowledge; and c) knowledge as referring to relationships, for which we reserve the verb *to know* employed as a term of relation (Ribes Iñesta, 1990 a), this meaning being synonymous with that of *way of knowing.*
5. Taking as a starting point Ribes Iñesta's proposal (1989), we assume that: a) more than one type of knowledge is possible, b) each one of which is contextually determined, and therefore conditional upon diverse social interests and criteria, c) so that each has its own validation criteria, and d) hence, they all constitute different degrees of empirical generality, making them relatively independent of one another. Therefore, the relationships between these ways of knowing cannot be characterised as linear interchanges; it is simply a problem that it makes no sense to approach from this particular perspective.
6. As a corollary to point 5, each way of knowing should be analyzed, then, in its own terms, that is, understood as an autonomous way of knowing. It is not legitimate to analyze them from exclusively formal perspectives; rather, they should be analyzed as practices that by definition take place and develop in sociocultural contexts.
7. Therefore, it is not legitimate to consider what basic research can teach applications and vice versa. What should be looked into is how knowledge and knowledge procedures are transferred from one way of knowing to another at a particular historical point in the development of psychology, describing these transmission and/or appropriation processes and subsequently considering which is the best way of interchanging knowledge between different ways of knowing; that is, what way or ways of knowing interchange we consider to be the best or most useful at the current time.

Below we analyze the field of Applied Behaviour Analysis (ABA), following the steps set down in the theses outlined above. That is: a) to attempt a tentative description of the activities of individuals working in basic research and in applications, considering that this description is applicable in a general way to psychology as a scientific and applied way of knowing; b) to extract from this description a series of continuous dimensions between the different ways of knowing; and c) to apply these dimensions to the analysis of the relationships TEAB-ABA.

EVERYDAY LIFE IN THE LABORATORY: THE SCIENTIFIC WAY OF KNOWING

Science is usually described as an activity that is essentially different from other human activities. An activity that is intrinsically *intellectual, rational, abstract, that seeks to discover the facts written in the book of Nature or, in other words, reality as it really is*. Hence, the traditional presentation of science as the activity of developing a hypothesis and its corresponding set of deductions, and the subsequent verification and testing of these that it is made through experiment. The following extract from an introductory psychology handbook is a good description of this view of science:

> "The scientific method is the so-called *hypothetical-deductive method*, in which, after observing some data, hypotheses are produced for explaining them; from these hypotheses, conclusions are deduced, which must be tested through inductive reduction as a result of experiment. Thus, the characteristic feature of the scientific method is that it begins with experience (observation of data) and ends with experience (testing through induction, which is why it is called inductive reduction). This contrasts the scientific method with other ways of knowing that, although starting out from experience, do not return to it at the end, but rather to some kind of abstraction or something that is merely intelligible, not empirically observable; something that may be believable or intelligible, but not empirically testable through inductive reduction. The scientific method is also in contrast to other methods that, although being based on observation, do not test their hypotheses, even though these may be empirical" (Fernández Trespalacios, 1997, p. 66; original author's italics).

Continuing with the traditional description of science, we can observe how it is conceived as a dispassionate activity, which tries to rid itself of all the obstacles and prejudices that distance the scientist from the reality he or she is seeking to discover. The only acceptable prejudice is that of the scientific theory that guides the scientist's steps in the selection of the analytical unit and of the data considered worthy of close attention. However, this classical and rational conception of scientific activity, in the first place, commits the sin of tautology (Latour and Woolgar, 1986), through the merely justificatory introduction of terms with a formal flavour such as "hypothetical-deductive", "hypothesis" or "inductive reduction." Secondly, it is too simple to be right. The scientist is not dispassionate, decontextualised, like the archetypal solitary professor totally absorbed in his experiments and theories. A careful reading of transcripts (Latour and Woolgar, 1986) of everyday conversations between those working in a neuroendocrinological laboratory reveals that, in some sense, the traditional notion of the scientific way of knowing is correct. It is indeed characterised by being analytical, abstract, enunciative and descriptive, eliminating references to the specific circumstances in which the studied event takes place (Kantor, 1953; Ribes Iñesta, 1989). Where the traditional conception of the scientific way of knowing falls down is in the *formal* nature attributed to it. The abstract, analytical character of science comes not from the formal and special nature of its operations, *but from its objectives and the specialized linguistic description by those working in it of the knowledge operations carried out*. A linguistic description that is crystallised in the background of a *conceptual tradition* that selects and discards data and relevant constructs in an often implicit and apparently synchronous way.

What factors might be involved in this process of construction and/or selection of facts? Without going into excessive detail, we can group them in three large categories: a) historical factors (individual, group, of the specific thematic field, of the group or groups of thinking styles used as a reference for the individual researcher); b) macro and micro-social contextual factors; and c) behavioural factors, referring to the types of interaction involved and the relationship between them.

We start out from the basis that all scientific facts are *constructed, not given*. Etymologically, the term "fact" is derived from the Latin *factum, facere* (to make, to fabricate), as Latour and Woolgar (1986) rightly point out. But this fabrication, this making, takes place in the course of a set of activities that are eminently social. And the object of these social activities, in the case of the scientific mode of knowledge, is the production of facts in the form of written texts and records, articles, books, etc. (Latour and Woolgar, 1986). Being constructed, scientific facts cannot be of just any kind whatsoever, or produced with total freedom. Researchers' conceptual frames of reference, determined by their initiation in a field and the dominant styles of thinking in their community, will be those that determine what type of fact is finally produced and no other (Fleck, 1935). *This conceptual frame of reference is the world for the rese-*

archer and the community to which he or she belongs. At first, when the researcher begins work in a field, his or her "vision of the fact" is, as it were, diffuse, blurred, such as when a myopic person puts on someone else's glasses. However, after successive contacts with the studied event, guided by the *all-pervading* style of thinking of his/her community, the new fact becomes decontextualised from the process through which it comes to light: it seems as though it had always been that way, and that we had been looking for precisely this from the start. We forget the long, slow process of gestation we have had to follow to produce a fact that will finally be published. In other words, *we have lost all connection with the historical referents of the process of producing the fact* (Fleck, 1935; Turbayne, 1970; Latour and Woolgar, 1986). As Turbayne would say, we have become dominated by our metaphor (Turbayne, 1970) or, in other words, the historical referents of the fact end up being those of the community –with its own style of thinking– in which it was born.

The fact thus established persists not only because of its disconnection from the historical referents of the process of its gestation, from being well-worn through continuous use as currency in the interactions between individuals making up a thinking-style group, and because of the pressure exercised by the thinking-style group during its genesis. To change this fact for another or take it down from the pedestal of truth is somewhat more difficult than it might seem. As Latour and Woolgar write (1986), established scientific facts constitute a set of statements whose review and questioning is *judged to be* very costly. Thus, the process of reviewing a fact is subject to multiple pressures: those from the context external to the thematic field in which one works and those imposed by the thinking-style group itself through the interactions between individuals working in the thematic field. There is no lack of historical examples with regard to this matter. In his description of the historical development of the Wasserman reaction, Fleck (1935) showed how the initial experiments were based on mistaken assumptions. Nevertheless, Wasserman and his collaborators interpreted their data as supporting their initial hypotheses, despite the fact that some of them openly contradicted them (for example, the case of the negative reactions obtained in brain tissue taken from patients with progressive general paralysis). According to Turbayne (1970), Newton started out from the assumption that he did not construct hypotheses. However, his *Principia Mathematica* constitute an outstanding example of hypothetical-deductive construction. What we wish to underline in saying that "...their questioning is judged to be very costly" is the difficulty, not to say impossibility, of erring from our point of view. This means, then, that we are somehow *inside* a belief, which constitutes the very way in which we live (Wittgenstein, 1979; Ortega and Gasset, 1986). Thus, even if the questioning or review of a previously-established fact were to be easy, it could be an enormously long and difficult process. This amounts to saying that we cannot allow ourselves to doubt that such a fact is true. Indeed, *it is often the case that we do not realise that we cannot allow ourselves to doubt that such a fact is true* (Wittgenstein, 1979). This means that the fact has been *reified*, transformed into something with an objective reality external to its own process of production. As Latour and Woolgar state in the work cited above:

> "The result of the *construction* of a fact is that it appears that no-one has constructed it; the result of rhetorical *persuasion* in the agonistic field is that the participants are convinced that they have not been convinced; the result of *materialisation* is that people can swear that material considerations are only smaller components of the "thinking process"; the result of investments in credibility is that the participants can claim that neither beliefs nor the economy have anything to do with the solidity of science; as far as circumstances are concerned, they simply disappear from the reports, so that it is better to leave them for political analysis and not for the appreciation of the solid and simple world of facts! (Latour and Woolgar, 1986, p. 268 of Spanish translation; original authors' italics).

And when the fact has been produced and is considered incontrovertible, and when the scientists that produced it present it as something that was always there and has finally been discovered, this fact decontextualised of the circumstances of its production process is ready for consumption by colleagues from the same thematic field, by colleagues from different fields of basic or applied research, by manufacturers, by the public in general with an interest in science, etc. But this fact ready to be consumed remains decontextualised from the historical referents of its production process. From the very moment it appears as an objective fragment of the reality that is out there, the distortion of the fact when consumed by its potential users *is already assured*. It is as though this fact was not an abstraction, but the world itself. It would seem that the production of the fact had taken place through a mysterious process, a mystery amplified and maintained by the formal terminology with which scientists themselves describe their activities. In fact, in scientists' descriptions of their activity to lay people they tend to omit all reference to the circumstances in which science takes place (Latour and Woolgar, 1986), thus safeguarding the prestige of science as superior knowledge incomprehensible to all but the initiated –though the ideology in which science is wrapped up is in reality interchange between lay people and scientists.

WAYS OF LIFE IN CLINICAL SETTINGS: THE TECHNOLOGICAL AND PRACTICAL WAYS OF KNOWING

Before going on, it should be stressed that the applied way of knowing is not a unitary way. We can distinguish two applied ways of knowing, following Ribes Iñesta's (1989) proposal. In the first place, we have the technological way of knowing, characterised by its synthetic nature of scientific knowledge and its basically operative and predictive character. This way of knowing is guided in an implicit or explicit way by a theory that allows the bringing together of various types of scientific knowledge, these first having been selected. On analyzing the work of Fleck, and referring to the specific case of medicine, Schäfer and Schnelle (1980) provided a good description of the technological way of knowing, its inherent peculiarities and the factors conditioning its development:

> "Fleck sees two particularities in medicine (...). The first consists in that in medicine knowledge is oriented not to regularity, to "normal" manifestations, but precisely to that which strays from the norm, to states of illness of the organism. Therefore, the formulation of regularities in the phenomena of illness, the delimitation of nosological entities, is only possible through a high degree of abstraction based on the observation of each individual case. Thus, conceptualisations in medicine are often of *a statistical type*. The second particularity resides in the fact that the knowledge goal of medicine is not, primarily, the extension of knowledge in itself, but rather a much more pragmatic one: dominion over such pathological states. The conceptions, models and principles, that is to say, all that is involved in the theoretical clarification of the observations of illness, is subject to a *permanent* and immediate need for success. Therefore, abstract statements are often seen to be insufficient in medicine (...). The nosological entities established are, to a large extent, fictitious, since between the knowledge in books and real observations there is an enormous gulf (...). The enormous range of particularities in the specific states of illness obliges the constant variation of medical conceptions (...), often, new types of problem, new pathological conditions, are unable, due to the need for success, the demand for a cure, to be inscribed in terms of the forms and pseudoforms established for that illness. This obliges, then, the formulation of new definitions of illness. But the direction in which this development moves depends not only on the new observations carried out (...). *It can only have its roots in the previous development of medicine, so that the new definitions of the illness grow from their historical antecedents"* (Schäfer and Schnelle, 1980, pp. 18-19 of Spanish translation; italics in final phrase are our own).

In this long quotation we can see how the technological way of knowing is basically subjected to the conditioning factor of success and/or effectiveness *in statistical terms*. And it is this condition that marks the difference, the point of inflection or discontinuity between the scientific and technological ways of knowing. In contrast to the scientific way, whose objective is description, the objectives of the technological way are prediction and control, that is, effectiveness in the solution of a problem. This discontinuity in terms of objectives and the related conditioning factors makes these two ways of knowing relatively autonomous and independent of one another.

However, there is a sort of continuity between the two ways of knowing in two senses: a) the very fact of the synthesis of knowledge produced by the scientific way of knowing, which implies that the technological way is to a certain extent dependent on the other; and b) as it should be noted, in our description of the scientific way of knowing in the previous section, we wrote that "*...this fact ready to be consumed remains decontextualised from the historical referents of its production process*." That is, knowledge produced via the technological way itself contains a particular degree of distortion, of bias, that is difficult to determine. And in the very process of synthesis further distortion will occur, basically determined by the often implicit character of the theory that guides the process of synthesis of the knowledge selected to be produced. The implicit nature of this results above all from the pressure exerted on the technological way of knowing so that it is effective in the solution of the problems with which it is charged. Moreover, though, a further form of pressure is often constituted by the very lack of definition with which the problems proposed are formulated, problems that are clarified by the technologist, whose conceptual frame of reference often remains implicit. As Fleck (1935) showed on analyzing the historical development of the concept of syphilis, the conceptions of the time that arose with respect to the Wasserman reaction were rooted in previous ideas, dating from medieval and Renaissance times, about the impure blood of syphilitics, a form of moral stigma, denoting sin, which was often identified with certain nations, such as France (it was no coincidence that one of the medical terms coined to refer to syphilis was "morbus gallicus"). In fact, what Wasserman and his collaborators were looking for was precisely what would now be called a specific biological marker of a disorder, which could have apparent value as a diagnostic proof. Nevertheless, this connection with past notions of syphilis remained implicit, on the one hand because the social commission given by the corresponding Ministry to Wasserman and his team (officials of that Ministry) was not clarified, and on the other due to the pressure that existed in view of the scientific rivalry with France caused by the Franco-Prussian War.

The example of the Wasserman reaction analysed by Fleck illustrates the implicit persistence of a style of thinking, in this case, efficient causal thinking. In modern terms, Wasserman and his team were looking to isolate a specific biological marker with high diagnostic precision for syphilis. This research objective was inadvertently based on previous concepts of syphilis. Similar causal notions of what today we call mental disorders arose in the medieval and Renaissance periods, developing up to the present day. Nowadays, the cause of these problems is no longer demons or spirits, but biological markers, contingencies of reinforcement, cognitive biases and similar factors. Notice that what appears to have changed is the nature of the cause, ***but not the causal scheme itself***. In the case of (biological) psychiatry, Ross (1995) carried out an analysis of the implicit assumptions in articles published in the *American Journal of Psychiatry*. All of these assumptions can be described as based on a linear and mechanical causal model.

The implicit nature of this "distortion of the distortion" can also be detected in other more recent cases that we shall discuss in some detail. In the case of ABA, Hayes, Rincover and Solnick (1980) carried out a bibliometric analysis of the first ten volumes of the *Journal of Applied Behaviour Analysis* in order to see whether the published literature fitted the defining dimensions of ABA proposed by Baer, Wolf and Risley (1968) in their inaugural article. What they detected was precisely the opposite: ABA was facing what they called *technical drift*, to the detriment of the conceptual reflection necessary for properly situating the problems to be analysed and of empirical and technological-developmental effort. This tendency was detected in the meteoric speed with which specific technical solutions were (and continue to be) proposed for a limited and topographically-defined set of problems. Another interesting example from the context of behaviour therapy is provided by intervention programmes for obsessive-compulsive disorder. Experimentally validated and fully operative programmes have progressed very slowly, and scarcely go beyond the traditional techniques of exposition and prevention of response, as demonstrated in recent handbooks (Foa and Wilson, 1991; Steketee, 1993). Analysing the conceptual history of the descriptive psychopathology of obsessions and compulsions, Berrios (1995) finds that the current definitions of both symptoms, on which intervention programmes proposed for behaviour modification are based, had already crystallised and were fully operative in the nineteenth century; it does not appear that recent advances impelled by cognitive models (e.g., that of Tallis, 1995) have remedied this situation, since they maintain intact the traditional definitions of both symptoms, translating them operationally into another technical language. It is implicitly assumed that the language of descriptive psychopathology is transparent or neutral, an unfortunate assumption, historically and conceptually speaking (Berrios, 1984).

The other way of knowing or way of life in the clinical context is the practical way, exercised daily by clinical psychologists in their work environment. This way of knowing could be described as being bound up with the specific circumstance in which it takes place, so that it does not have the aim of generalisation. Also, it is basically narrative in nature, and is reproduced in a routine way, which gives it a certain repetitive character. Its fundamental objective is the solution of *individual* problems, in the sense that its aim is to modify the unique circumstances of individuals or particular groups with unrepeatable interactive histories. This is something any clinical psychologist knows. The users of their services can request help in a variety of ways. Not all clients are motivated in the same way to see a psychologist: the couple who both think they are in the right and come more or less to demonstrate to one another that there is no possible solution; the adolescent that comes because his parents oblige him to... and so on, in a wide variety of different situations (1).

As it can be seen, the practical way of knowing is different from the technological one in a fundamental sense: it is concerned with the prediction and control of behaviour, *but in particular cases*. In this sense the clinical psychologist employs the technology developed by the technologist, but in a way that is flexible and adapted to the particular circumstances with which s/he is dealing. The use of technology by the clinical psychologist can by definition never be the norm, and in fact never is, not even in cases of shoddy or amateurish practice, or in those in which the novice tries to apply in the most orthodox way a specific technique. The clinical psychologist is conditioned *in an immediate way* by at least two factors: a) the particular case s/he is trying to solve; and b) the particular style of thinking s/he has absorbed during his/her initiation as a clinical psychologist, a complex hotchpotch of those prevailing in the scientific and technical reference group and in the society in which s/he lives. In other words, the clinical psychologist is an expert in the general sense, in the sense that s/he shares with the user of his/her services an ideology in terms of social practice that is manifested in the everyday language to which we refer above.

There is also an important divergence between the practical way of knowing and the technological one. In contrast to the technologist, whose problem is to maximise the effectiveness of his/her knowledge or, in other words, his/her percentage of successes, the clinical psychologist is interested not only in effectiveness, but also in the percentage of failures, which the technologist does not need to explain. In either case, however, the clinical psychologist is not only asked for success, but also has to *be seen to be successful*. It is for this reason that we can describe the everyday practice of the clinical psychologist in the consulting room as one that is itself *ideologised* (Ribes Iñesta, 1990 d), although it is not recognised as such. What do we mean by the expressions “individual case” and “has to be seen to be successful”? “Individual case” means that the problem about which s/he is consulted *is that of an individual*. Thus, the user or patient becomes the target of the clinical intervention, an intervention that is external to the client, administered, of course, by an expert in the problem that *afflicts the client*. But the client’s problem, the trust s/he deposits in the clinical psychologist and the judgement made by the latter are by no means divorced from the scientific-social ideology that prevails at any given socio-historical moment. Nevertheless, this ideology remains implicit and is exercised daily as a way of life, as a belief, within the clinical context. In the specific case of Freudian psychoanalysis, Pérez Álvarez (1992) carried out an historical analysis that shows us how its birth and its practice were at all times intimately bound up, entwined with life in Vienna around the turn of the century (2), just at the time when there began to crystallise what Béjar (1993 a) refers to as a “psychological culture”.

By the expression “*has to be seen to be successful*” we wish to refer to none other than those characteristics all clinical psychologists *should display*: empathy, attentiveness and sympathy, calmness and naturalness, good appearance, a correct and considerate treatment of the client with respect to his/her values, etc. At this point we should like to describe clinical practice, still in terms of its ideological character and, following Pérez Álvarez (1996), introducing two interesting anthropological concepts: a) ceremony (therapeutic) as the context in which it is carried out, and b) rhetoric. It is within the framework of these two anthropological concepts that clinical practice is developed as an ideologised activity.

Ceremony is an interesting contextual concept in that all forms of psychological treatment involve a certain preparatory ceremony; moreover, it could be proposed as an interesting hypothesis that human behaviour has an inherently ceremonial dimension (Wittgenstein, 1976). All that comes afterwards is conditional upon these ceremonial operations. For example, one of the initial steps in all cognitive therapy is what Beck (1995) calls “socialisation in the cognitive model.” This ceremony involves a series of operations well-known to behaviour therapists: familiarising the client with written recordings of thoughts or other behaviours, conceptualising the problem in terms of the cognitive model, labelling of underlying cognitive distortions, prescription of tasks to be done at home and insistence on their importance for the process of improvement, etc. A set of operations that will condition the rest of the process. And in this operative context, rhetoric will enter as an exercise unnoticed by the clinical psychologist; indeed, few clinical psychologists would describe their everyday activity as rhetorical, perhaps because of both the possible pejorative connotations of the term and the prevailing technical

(1) *As a collateral reflection that we shall not develop here, it is curious that, given such a great diversity of situations, the existence of an actual profession (clinical psychology) can be conceived, in the sense of the term “profession”, that is, with well-defined frontiers with respect to other professions, and a social commission that is made clearly explicit and is clearly delimited from others.*

(2) *It would be interesting to analyse the interactions between medicine and the nascent psychology, as well as their parallel development and the absorption by clinical psychology of aspects of work that were previously the exclusive domain of doctors, initially internists in the first asylums in Britain, and later becoming converted into psychiatrists. The emergence of psychiatry as a medical speciality and the development and crystallisation of clinical psychology might be seen as parallel processes of the division of labour.*

ideology in the field of behaviour modification, in the sense that the techniques employed appear to be the only effective ingredient of any psychological treatment. However, techniques of any kind remain the ceremonial wrapping in which rhetoric is deployed as a linguistic exercise. Whether we like it or not, the clinical psychologist is rhetorical, although hardly aware of it, and must look as though s/he is effective through, among other things, the exercise of rhetoric, persuasion *with* the client, an exercise that may be noble, but may also be wretched, given the contributions of both players in this dialectical game. Laín Entralgo (1987) is keen to emphasise the great likeness between the practice of psychotherapy and the rhetorical exercises of the Sophists in classical Greece. In a similar way, it is curious to find the figure of the clinical psychologist compared with that of the confessor, in the context of an emotivist and individualist culture (see Béjar, 1988, 1993 b).

As Pérez Álvarez (1996) points out, the rhetorical person becomes a kind of *sceptic*. This is logical if we bear in mind that through rhetoric one has to persuade, to convince in diverse, unique situations –and this is very close to the everyday practice of clinical psychologists in their consulting room. In fact, in a way, the clinical psychologist is a sceptic, but a sceptic who *believes without knowing s/he believes* (Wittgenstein, 1979). This believing without knowing one believes underlies a linguistic practice peculiar to all clinical psychologists, which can be observed in public and private clinical contexts: the use on the part of clinical psychologists from various doctrinal affiliations of a kind of common vocabulary, an "Esperanto", as it were, which, without being technical, seems to have its origin in the vocabularies of diverse schools of psychotherapy that are in principle diametrically opposed. For example, a psychoanalyst or a behaviour therapist may use the term "reinforcement" to refer to an observable consequence administered to a given response. However, the term is incorrectly used, as it does not refer to the increase in frequency of that response, which is a condition *sine qua non* for defining reinforcement as an operation. The term "unconscious" is another example; it is often heard in conversations between two clinical psychologists discussing a case, and it sometimes seems to refer to something that happens in an automatic way, other times it is found correctly used in the psychoanalytical context, and on other occasions it is unclear to what it refers. This common, everyday jargon, which is nothing more or less than the everyday rhetoric of the consulting room, where doctrinal diversity tends to be the norm, becomes an instrument through which the clinical psychologist, without knowing it, lives in the metaphor (to use Turbayne's expression), but also "sells" that metaphor in a believable (i.e., "scientific") way to the client.

Due to the routine nature of their work, that is, rooted in the everyday, bound up with the specific circumstances of each individual case, clinical psychologists are often unaware of their connections with the scientific and technological ways of knowing or with the ideology underlying these and their own professional practice. The more or less sophisticated clinical psychologist will use a theoretical model, not as an approximate description of a problem, but *as the literal truth*, without questioning him/herself whether the model s/he is using as a categorisation tool is conceptually congruent with the raw data s/he is observing and seeks to systematise. This is logical due, among other things, to the fact that the technologist has developed techniques for promoting behavioural change, but has not defined any criteria that allow its application and/or modification in individual cases. These criteria are defined by the clinical psychologist as s/he goes along. In fact, the clinical psychologist selects the information of interest when, for example, identifying symptoms, unaware that s/he is assuming, among other things, that psychopathology is transparent (Berrios and Chen, 1993) and, moreover, ideologically neutral, that is, devoid of normative judgement criteria. For the clinical psychologist, the psychopathology is simply there, and what s/he has to do is identify it and give it a name/concept. Clinical psychologists are so anchored to the specific circumstances of their everyday work that they are scarcely able to consider its connections with other ways of knowing, nor with the prevailing ideology. And in this sense the clinical psychologist is dominated by the metaphor, *among other reasons*, because the technical vocabularies of the scientific and technological ways of knowing have developed in other circumstances and for other uses quite different from those of the practical way of knowing. Technical language is often used in the context of this way of knowing, as the wrapping for persuasive discourse, based on the social criteria of the evaluation of science and of professional practice as an intellectual profession (Talento and Ribes Iñesta, 1980).

CONTINUITY BETWEEN WAYS OF KNOWING

From this brief description of the three possible ways of knowing in the context of psychology, we have isolated, by way of synthesis, two continuous dimensions of their relationships:

1. Everyday language as a means of appropriation of knowledge and knowledge operations between ways

of knowing. In the case of psychology, this dimension is critical given the origin in everyday language of its vocabulary of technical terms (Ribes Iñesta, 1990 a). Moreover, psychology has the curious privilege of being (or seeking to be) both a science and the object of science at the same time. Thus, at the same time as a term such as "to think" is used to describe a certain interaction, the use itself of the word requires its own explanation. In other words, we have to determine the origin in everyday language of our scientific, technological and practical knowledge (Lee, 1988).

2. In the successive transfer operations, knowledge and knowledge operations suffer a progressive distortion that is all the more accentuated the more a particular way of knowing is anchored to specific circumstances. This distortion is in fact the decontextualisation of a particular piece of knowledge or knowledge operation, which with use become reified. This reification is facilitated by the relatively autonomous nature of the different ways of knowing, and by the absence, in the spaces between ways of knowing, of defined criteria that permit the translation of knowledge and knowledge operations from one way of knowing to another. These criteria have to be developed *ad hoc*, and in the majority of cases are implicit and ideologically impregnated. From this follows the notion of direct extrapolation. Among other things, the direct extrapolation of scientific knowledge and knowledge operations to the technological and practical ways of knowing constitutes a practice of ideological self-justification of these ways of knowing. In other words, a sort of quasi-scientific, rather than scientific ideology. It should also be borne in mind that, given the absence of explicitly-defined criteria that permit the transfer of knowledge and knowledge operations from one way of knowing to another, the interrelations between them are cut, contributing to the ways of knowing becoming isolated, converted into islands, into networks of quotations closed in on themselves, whose main objective is their own survival. The development of such explicitly-defined criteria requires basic research of a domain type (Fuentes Ortega, 1993) or, put more simply, the return of psychology to mundane matters, to everyday life.

Against this background the scientific, technological and practical ways of knowing interact constantly. To a certain extent, the technological and practical ways of knowing depend on the scientific way, as a database for synthesis in the case of the technological way, and as a context of ideological justification in the case of the practical way. In turn, the practical way of knowing delimits the frontiers of technological knowledge, identifying exceptions that can be transformed into problems pertinent to its own processes of theoretical and empirical inquiry, a delimiting role that is also played by the technological way of knowing with respect to the scientific way.

Operationalising these continuous dimensions between the three ways of knowing, what criteria should be satisfied so that this interrelation can be found? The three criteria proposed by Ribes Iñesta (1982 b, pp. 102-103) are: a) the existence of scientific knowledge and technological knowledge that provides the theoretical and methodological foundation for practical knowledge; b) a common language that permits the analytical assessment of technological application; and c) explicit social criteria with regard to the characteristics and conditions of application of the available technology. As it has been seen up to now, academic and applied psychology and the professional practices derived from them do not fulfil any of these criteria. We shall now test this affirmation, analysing, in the light of these criteria, the relationship between TEAB and ABA, as an excellent example from psychology of the development of a technology of behavioural change based on basic research. This is an interesting example since the two fields appear to be related, resulting from the use of a common technical vocabulary and a set of relatively well-standardised procedures. Moreover, ABA appears to have derived logically as an extrapolation of TEAB outside of the laboratory. In the light of what has been argued up to now, we shall show that this conclusion is, at the very least, imprecise.

AN ANALYSIS OF THE INTERRELATIONS TEAB-ABA: INTIMATE RELATIONSHIP OR MARRIAGE OF CONVENIENCE?

The theoretical and conceptual structure of TEAB

Does TEAB currently fulfil its role as a database for the development of a scientifically-based technology of behavioural change? Or, in other words, is there knowledge to be synthesised? The answer, although there are details that lack of space prevents us from dealing with here, is, on the whole, negative. TEAB constitutes more a promise of empirical and theoretical development with regard to human behaviour than a reality. Some authors have pointed out the inherent difficulties in its theoretical structure (Kantor, 1970; Ribes Iñesta and López Valadez, 1985). Though not the only ones, concepts such as reinforcement, and knowledge operations such as the obtaining of response rates as a basic measure and the selection of repetitive and easily quantifiable discrete responses take up almost exclusively the attention of researchers. Assumptions such as that of continuity among species (Hayes and Hayes, 1992) continue to operate within the TEAB community; despite the strong quantitative increase in basic research with humans detected by Hyten and Reilly (1992), the strategy of research has not changed substantially, given the great importance still attached to the use and design of animal models with the purpose of isolating general parameters whose generality and relevance for human behaviour will be confirmed (or not) later in a second research phase. The error is often made of identifying all mental terms of everyday vocabulary with verbs of action (e.g., Chiesa, 1994; Verplanck, 1996), in a way coherent with the above-mentioned methodological routines of TEAB.

In spite of these inadequacies, TEAB can be considered to constitute a good database for the development of ABA. Let us see whether this is the case, in the light of the three criteria formulated by Ribes Iñesta (1982 b), so that we can speak of harmonious TEAB-ABA relations.

To begin with, we should ask ourselves whether TEAB has determined the origin in everyday language of its knowledge. Skinner realised early on the theoretical need to take on this task, in conceptual and empirical terms, in his article on operationism (Skinner, 1945). Skinner knew it was necessary to identify the variables that determine the use of the mental concepts of everyday language, as a scientific enterprise of the first order that could contribute, among other things, to explaining the behaviour of scientists. In this sense, instead of rejecting mental expressions without directly empirically observable correlates, the procedure characteristic of methodological behaviourism, it is legitimate to ask oneself about the variables that control the use of these expressions. For example, instead of rejecting psychoanalytical vocabulary on the grounds of its inaccessibility to an observer of the constructs it contains, the behaviour analyst should consider the psychoanalyst's verbal behaviour as an object of study in its own right, isolating the contexts and variables that control the use of expressions such as "Oedipus Complex" or "resistance." This attractive research programme, whose parallels with the work of Wittgenstein have not gone unnoticed by behaviour analysts (Day, 1969), has made hardly any impact on TEAB. With the exception of Day's work (see its appreciation by Moore, 1991), only recently (Leigland, 1989, 1996) has this thematic field been taken up once more in publications. As Leigland (1996) notes, Skinner abandoned the systematic exploration of the use of the mental terms of everyday language in favour of TEAB. Skinner himself (1945) remarked that this undertaking was of only historical interest, and that the objective of the nascent science of behaviour was to develop its own technical vocabulary and a strong empirical corpus. The abandonment of the analysis of the use of the mental terms of everyday language had, historically, disastrous consequences for the development of the experimental analysis of human behaviour:

1. Paradoxically, the analysis of verbal behaviour as a legitimate object of study was abandoned. Only recently have we seen a renewed interest in this thematic field with the explosion of research on equivalence relationships between stimuli, rule-governed behaviour, and the appearance of the journal *The Analysis of Verbal Behaviour*, even if (and this is symptomatic) this journal is still published irregularly, and its production is quantitatively far inferior to that of other specialised journals in the field of TEAB.
2. Research effort was concentrated on the experimental analysis of animal behaviour, a methodologically more obvious and theoretically more fruitful enterprise, since it allowed the development of the technical language and knowledge operations of TEAB.
3. It fell into what Hayes (1992) called *the observer's perspective*, characteristic of theoretical systems interested in prediction and control; that is, TEAB became a kind of technological undertaking where the demonstration of control over a kind of behaviour and the subsequent prediction of its future occurrence become the ultimate criteria of the goodness of fit of a scientific principle. The consequences of the observer's perspective, beyond control by the events of interest, are observed in the identification of reality with the knowledge operations carried out by the researcher. For example, in their recent review of human and animal research for applied purposes in the area of reinforcement schedules, Lattal and Neef (1996) affirm that, though lacking the precision attained in the laboratory and the formal prescriptions of procedure details, reinforcement schedules are operative in real environments. Even if these authors show exemplary caution in their statement, they fail to identify the control variables of their use of the term "reinforcement schedules": are they an analogical model of an observation of reality, or do they constitute an operation identifiable in reality as such? Do they constitute simple structural descriptions of observed real covariations? With what level of reality are they identified?

4. The identification of the events under observation with the knowledge operations used involves important problems on trying to extrapolate these operations from the context in which they were designed to the study of complex human behaviour. It is sufficient to consider the example of the area of rule-governed behaviour. Schlinger (1990) argues against those researchers that continue to identify a rule with a discriminative stimulus, proposing that the term "rule" be reserved for those verbal stimuli that specify contingencies and thus alter behavioural functions. So, then: what type of functional alteration is produced, and what contingencies should be specified? The use of the terminology of TEAB in this area is severely limited, since, if Wittgenstein (1967) was correct, the term "to think" and its related expressions are employed multivocally in very different contexts and, functionally speaking, we can find ourselves dealing with different kinds of events. Thus, do the studies reviewed by Catania, Shimoff and Matthews (1989), Chase and Bjarnadottir (1992) and Verplanck (1992), constitute examples of rule-governed behaviour? And, if so, are they of the same type, functionally speaking, or can we isolate different types, some identified with thinking and others with other behaviours? It is not possible, at the present time, to give a clear answer to the question. The limitations of the technical terminology of TEAB, in the sense of its difficulties for dealing with qualitative details of the organization of human behaviour, can be observed in the mentioned study by Schlinger, Blakely, Fillhard and Poling (1991).

Thus, TEAB appears up to now to have failed to elucidate the strategic concepts contained in so-called "everyday psychology", that is, the mental terms and expressions of everyday language, which constitute the origin of its knowledge, in the sense that they are the very events it is sought to understand, describe, control and predict (Lee, 1988). It would appear difficult, then, for TEAB to be able to explain properly the development and maintenance of ideology in the sense of cultural practice transmitted through use and custom, and even less to provide the database that would permit the technologist to develop significant behaviour modification techniques. With this last statement we intend to refer to the fact that in TEAB it is common practice to identify a particular knowledge operation (e.g., a reinforcement schedules) with an observed event that it is sought to predict and control. The criterion that controls this practice is precisely success in the prediction and control of a kind of behaviour. However, the parameters isolated by basic research may lose relevance in other contexts or settings (*setting factors*, as Kantor [1924] called them), where each *event* (not instance of stimulus and response) is unique, and not necessarily repetitive.

It is only recently that the TEAB community has begun to become aware of this problem. Thus, researchers appear to use the term "equivalence relations" in a variety of ways: a) as an experimental procedure directly extrapolatable to reality, in the case of restricted situations such as learning to read, or of specific theoretical problems, such as the generative nature of human language; b) as a special behavioural phenomenon; c) as an experimental procedure that, despite the impossibility of direct extrapolation, is increasing knowledge about certain properties of complex human verbal and non-verbal behaviour, thus becoming transformed into a conceptual tool for the generation of applied implications that are counter-intuitive to the traditional logic of the TEAB and ABA fields. The question remains open, with no solution foreseeable in the short term.

THE THEORETICAL AND CONCEPTUAL STRUCTURE OF ABA

Baer, Wolf and Risley (1968) isolated seven key dimensions that characterise ABA: a) applied, b) behavioural, c) analytical, d) technological, e) conceptually systematic, f) effective, and g) generalisable. These dimensions were conceived as a way of assessing a study as applied. Baer, Wolf and Risley were aware that research outside of the controlled environment of the laboratory could not be assessed according to the current criteria for assessing it. These authors situated the ultimate criterion for distinguishing between basic and applied research in the differences in emphasis on the focus of experimental control of the relevant variables, and in the selection of behaviours relevant to the study (treated as dependent variables). Thus, basic research may potentially select any behaviour for its study and any variable on which this behaviour may be dependent. In contrast, applied research is restricted to the analysis of *socially important* behaviours in the contexts in which these occur, together with the relevant variables of which these socially selected behaviours are a function. But: what does the expression *socially important behaviours* mean? How are these behaviours selected? Are they really selected for their social importance, or for methodological convenience? Who decides which behaviours are socially important and which are not? The answer to these questions is crucial for determining whether ABA constitutes an authentic technology of behaviour based on a relevant database. The keys to these answers can be found in the original article by Baer, Wolf and Risley, as well as in their reappraisal of ABA almost twenty years later (Baer, Wolf and Risley, 1987).

As regards the question: what does the expression *socially important behaviours* mean?, we should reply to it bearing in mind that the criteria of social relevance may be fixed simultaneously by at least two social groups: the users likely to benefit from research and application activity, and the groups that offer such services. The group of potential users is obviously interested in solving a problem, and the group of professionals has a double interest: on the one hand in the study of the variables that contribute to the genesis and the maintenance of the problem, and on the other in its solution. The two social communities share as a cultural practice everyday language. Also, each community has developed its own solution procedures, based on its own criteria for defining a given behaviour as problematic. And these sets of procedures, independently of their effectiveness, are functional forms of behaviour in the contexts in which they are exercised. It is at the heart of these contextualised practices that the criteria of social judgement that define a given behaviour as problematic are generated, maintained and disseminated.

If the above is true, we greatly fear that ABA has not dealt correctly with the problem of the social definition of its target behaviours. Given that TEAB abandoned for a long period, as a legitimate scientific enterprise, the elucidation of the control variables of everyday language, especially of the language of mental operations, ABA could not have had better conditions in which to analyse this question. Let us consider the extent of this argument.

In their original article of 1968, Baer, Wolf and Risley, on defining the *behavioural* dimension of ABA, set as an ultimate assessment criterion that which a given subject can do effectively, drawing a strict distinction between what the subject does and what s/he says. As an obvious consequence, they rejected as a valid measure what the subject says about his/her non-verbal behaviour, unless this verbal report could be independently validated. This assessment criterion can be considered an obvious and prudent methodological requirement given the scarcity of available data on verbal behaviour, which at that time consisted basically in the demonstration of sensitivity to reinforcement of discrete, repetitive verbal and easily quantifiable *topographies* (Greenspoon, 1950; Wilson and Verplanck, 1956), or of larger functional units, such as statements of opinion (Verplanck, 1955). However, the entrenchment of applied behaviour analysts in this methodological precaution due to the need to demonstrate cleanly the effectiveness of their procedures, in a period in the history of ABA characterised by its militancy against the medical model prevailing in clinical contexts, was to have disastrous consequences.

In the first place, ABA was restricted to the analysis and modification of a limited group of target behaviours in institutional settings in which it could guarantee greater control and availability of subjects (Hopkins, 1987), a limitation to some extent understandable since the initial target populations of ABA were under government control in large institutions. These target behaviours were considered socially important according to their immediate effects on the subjects emitting them, it thus being considered that ABA fulfilled its first (applied) dimension defined by Baer, Wolf and Risley in their seminal article. However, it is reasonable to assume that due to their own methodological demands of obtaining reliable measures of repetitive physical events (response rates), and the rejection of verbal behaviour as a criterion for assessing change, they eliminated from ABA's action context complex behaviours such as those referred to as "neurotic." These methodological requirements continue to be present in published research, as can be observed in the resort to the use of analogies from physics, such as *behavioural momentum*, applicable in quantitative research concerned with the analysis of the temporal distribution of response rates (e.g., Mace, Mauro, Boyajian and Eckert, 1997), or the recent renewal of interest in procedures of response effort, defined originally as the force of pressure necessary to work an operandum in the animal laboratory (Friman and Poling, 1995). It is somewhat paradoxical to find this emphasis on response rate (which, among other things, often impedes strict comparison between basic and applied studies, as exemplified in the mentioned review by Friman and Poling on response effort), when it is recognised that frequency measures often do not constitute the best measures of behavioural change in applied settings (Baer, 1986).

Secondly, with regard to the selection of the target behaviours of ABA, apart from the methodological bias discussed, it would seem that ideological biases were inadvertently introduced. For example, the behaviour of repeatedly banging one's head against the wall observed in retarded subjects in institutions, or the delusional speech of institutionalised psychotics, are examples of dramatic behaviours that clearly affect not only the implementation of therapeutic and/or rehabilitative programmes and the health and quality of life of the subject that emits them; they also seriously perturb the climate of the institution in which they are patients. If we reduce the frequency of delusional speech behaviours in a psychotic subject in the context of his/her hospital, we can achieve a situation, among other effects, in which the subject can hold reasonable conversations with other patients and with staff. But: what is the objective of achieving a greater frequency of reasonable conversations? What is a reasonable conversation in this context? Describing clearly the subjective symptoms experienced by the subject in a clinical interview? Talking about the weather with another patient or with a member of staff on a boring shift? Discussing the current political situation? It is a well-known fact that these interventions are generalisable only to the limited context in which the intervention is implemented, while increasing the tranquillity of the institution. If we take as a reference adult subjects categorised as "neurotic", and put into historical perspective the procedures employed for the solution of their psychological problems, we frequently find that these procedures, supposedly experimentally designed and validated, are clearly –though unconsciously and implicitly– inspired in historical-cultural traditions (Pérez Álvarez, 1991). Only recently have those working in basic and applied analysis recognised the pressing need to take into account *setting factors* in order to develop a true behavioural technology (Baer, Wolf and Risley, 1987; Wahler and Fox, 1981), with the beginning of the experimental analysis of contextual control (e.g., Bush, Sidman and de Rose, 1989; Steele and Hayes, 1991), sometimes limited to questions of a quantitative and structural nature about the phenomenon of equivalence relations (Sidman, 1994; see pp. 475-531).

Thirdly, in the initial stages of its development, ABA emerged as a critical alternative, along with other fields, to the medical model prevailing in clinical contexts. Its contribution of new methodologies and different conceptual points of view on human behaviour were thought to be sufficient, together with the proof of their effectiveness, to achieve its generalisation as *the* technology of behavioural change necessary for the solution of a wide range of health problems. However, when ABA arrived in the professional arena, it found already solidly-established practices, which it simply confronted. Just as TEAB abandoned the analysis of the variables of control of verbal behaviour, at no time did it make any effort of historical-conceptual analysis of the technical terms of psychopathology and the traditionally-established therapies. Such analyses might have allowed the development of a new theoretical and technological corpus, as well as the identification and empirical and conceptual foundation of behavioural change procedures that were effective, but developed on the basis of common sense (and thus scarcely evaluated), at the same time as the reconstruction of a set of technical vocabularies such as that of psychopathology, founded on a consensus-based definition of the object of study of behavioural sciences. Thus, the virulence of the confrontation between irredeemably opposed doctrinal positions was inevitable. Finally, ABA (and behaviour modification in general), has ended up revolving around the old psychopathological concepts, which, redefined operationally in successive editions of the DSM system, are undergoing a new process of change in which symptoms are selected and discarded according to whether or not they can be adapted to the operationalisation process. Thus, for example, although ABA seeks to analyze and modify autolesive behaviours, the functional meaning of these behaviours continues to revolve around classificatory categories such as "autism" or "mental retardation", or descriptive categories such as "hallucination", in an uncritical way, without it being noticed that the conceptual load of these terms remains intact. In this way, ABA has not in fact become a brilliant contributor to the analysis and classification of behaviour judged as abnormal, nor has it contributed especially to the elucidation of this judgement process.

Fourthly, we should even consider whether criteria d) and e) listed by Baer, Wolf and Risley in their 1968 article are fulfilled by ABA in a general way. With regard to criterion d) (technological), Hopkins (1987) recognises that it is often the case that not all of the variables that may be mediating the effectiveness of a procedure are specified. Ribes (1977) provides the example of the time-out procedure in a study in which the aversive nature of this procedure did not appear to be responsible for its effectiveness. Similarly, and halfway between dimensions d) and e) (conceptually systematic), Schlinger, Blakely, Fillhard and Poling (1991) comment that referring to a functional relationship as discriminative or reinforcing in the case of verbal human subjects may lead to our losing sight of the operation of other specifically human variables that have not been adequately analysed. Other examples can be quoted –without pretending to be exhaustive–, that call into question the conceptually systematic character of ABA due to the inconsistent use of technical terminology (Carr, 1996; Woods, 1987). In fact, Baer (1986) recognises that ABA undertook its task using terms with an analytico-behavioural flavour, but "free of the laboratory", a situation that was contributed to, paradoxically, by the assessment of the social validity of the effects of behavioural techniques.

RECAPITULATION: TEAB-ABA INTERRELATIONSHIPS

The time has come to respond to the question of whether TEAB and ABA interact with one another in some way beyond the terminological and methodological façade, beyond verbal appearances and the flavour of their technical terminology. The fact of whether TEAB and ABA interact with one another transcends the mere verification of that fact. On this interaction depends the very fact of our having an authentic technology of behavioural change. To answer the question, let us return to a consideration of the three criteria proposed by Ribes Iñesta (1982 b), which we already saw in the third section of this paper.

a) *The existence of scientific knowledge and technological knowledge that provides a theoretical and methodological foundation for practical knowledge*. This criterion is not fulfilled in a strict sense by either TEAB or ABA. In the case of TEAB, due to the scarcity of data and theoretical work on human behaviour, which has only begun to appear over the last two decades at most. Moreover, this work is conditioned by the very theoretical structure of TEAB, which poses questions relevant to research, of a quantitative nature, and focused on the analysis of phenomena observed in the laboratory, claiming to provide appropriate and sufficient descriptions of behaviour in its real settings. In the case of ABA, the functional relationships isolated in the laboratory have not been translated into parametric research and systematic technology. Its efforts have concentrated almost exclusively on the assessment of the effectiveness of the procedures used themselves, perhaps based on the notion that the basic knowledge available on the effects of diverse knowledge operations constitutes the necessary and sufficient justification for embarking on a *pragmatic* enterprise that restricts itself to translating knowledge operations (only some of them) to a different environment and in showing their effects *a posteriori*. This translation should never be confused with the *synthesis* of knowledge, which does not in any way involve the use of identical knowledge operations between ways of knowing that are incommensurable. Rather, synthesis, as we see it, constitutes a process of *derivation* of implications starting out from a database, which may allow: a) the complete and global characterisation of a problem, not only with regard to the variables involved in an immediate way, but also to its ideological determinants; b) the modification and use of culturally pre-existing behavioural change procedures, with the object of achieving new effects or the same ones where desirable; and c) the development of new procedures of behavioural change.

b) *A common language that permits the analytical assessment of technological application*. Such a common language does not exist, as some authors, such as Baer (1986) have concluded, or is at least equivocal, according to others, such as Carr (1996), Schlinger, Blakely, Fillhard and Poling (1991) or Woods (1987). However, in addition to this common language, it is necessary to bear in mind the different contextual determinants of the scientific, technological and practical enterprises, which give different meanings to this language, transforming and/or distorting it according to their own needs. It should be remembered that the knowledge operations and knowledge characteristic of each way of knowing can undergo distortions on losing sight of the historical referents of their genesis, distortions that are more and more pronounced the more a particular way of knowing is bound up with a specific circumstance. We are also aware that the different technical languages refer to different types of event: a) the events being studied (descriptive languages); b) the knowledge operations used (methodological languages); and c) the data finally produced (data languages). Each technical language, we repeat, is under the control of its own contextual determinants, which it is necessary to describe and analyse. As far as we know, such a task has as yet scarcely been begun (Dougher, 1993). Only when suitable data are obtained, based on premises that are appropriate to human behaviour, can we begin to assess the extent to which the technology of behavioural change fulfils this criterion.

c) *Explicit social criteria with regard to the characteristics and conditions of application of the available technology*. Such criteria have been identified by applied behaviour analysts with the empirical determination of criteria of social validity on the part of the users of professional services, or “subjective evaluation” (Wolf, 1978). They were initially discouraged on grounds of methodological prudence (Baer, Wolf and Risley, 1968). However, the explicitation of these social criteria, an enterprise appropriate to ABA, does not appear to us to have yet been properly undertaken. In the first place, the strategy of the assessment of the social validity of a behavioural intervention involves inherent ideological dangers, already analysed elsewhere (Ribes, 1982 b; Talento and Ribes, 1980). Secondly, even on the basis that the strategy of social validity is appropriate, a difficulty arises when we consider its subjective nature. What is "subjective"? What does this term mean in this context? As employed by Wolf (1978) and Baer, Wolf and Risley (1968), the term appears to refer to responses of verbal topography that are neither repetitive nor easily quantifiable, employed in relatively particular circumstances, that is, unique. If this is the case, we have to ask ourselves how it is possible to determine empirically the criteria of social validity. Are they generally applicable to any technological undertaking because they always refer to specific behavioural topographies with similar effects (i.e., to an operant class)? And even then, if we bear in mind TEAB research itself, different subjects may emit different verbal responses to the same contingency. Thus, how can we find points of agreement between several rules self-generated by different individuals? Which of these rules do we select as the appropriate criteria of social validity? Or should we take them all into account? We fear that these authors have fallen into important contradictions with regard to the logic of TEAB itself, since such rules and/or criteria are functional under contextual control. Therefore, these verbal responses cannot *per se* constitute appropriately explicitated social criteria if their contexts of emission are not taken into account. In other words, what becomes essential is the explicitation of the ideology that sustains these verbal practices, both on the part of the users of professional services and on the part of the professionals that provide these services. No solution to this problem is foreseeable in the short term whilst ABA continues to be anchored to the logic imposed upon it by operant conditioning.

In conclusion, we believe that we cannot speak of an interrelationship between TEAB and ABA. At the present time, the relationship between the two thematic fields seems more like a marriage of convenience that sustains a rhetorical practice of survival: quasi-scientific ideology. Their history is nothing more than the chronicle of a foreseeable divorce.

CONCLUSION: WHAT KIND OF METAPHYSICISTS DO WE WANT TO BE?

The conclusions of this work (or rather, the possible ways forward) are focused on aspects we consider to be negative in the scientific and technological projects that constitute basic and applied psychology, respectively, at the current socio-historical time. Pointing out the negative aspects in the current development of the so-called (by themselves) basic and applied psychologies in the specific cases of TEAB and ABA constitutes a starting point for considering the possible future development of these two projects, which we summarise in a series of points:

1. Basic and applied researchers, as well as "professionals" should situate their everyday practice in its historical context. All scientific and/or professional practice is carried out at a given socio-historical moment. However, in undergraduate and postgraduate programmes the history of psychology, as it is taught, amounts to nothing more than a historiography that does not aid the identification of conceptual specimens, nor its assessment as a corpus of data as important as the empirical data *per se*. Furthermore, there is a need for the learning of techniques of conceptual analysis. If university teaching and professional practice become converted into ignoble rhetorical exercises, it is often due to the lack of a critical perspective with regard to the technical discourse itself. Conceptual analysis becomes more relevant in the so-called Humanities, which constitute the subject and object of analysis simultaneously. This allows the avoidance of a situation of uncritical acceptance of conceptual specimens that are transmitted with astonishing ease in formal education and training through constant use. This historico-conceptual analysis, on the basis that everyday language is the instrument of the transfer of knowledge between ways of knowing, and the irreducible foundation of psychological knowledge, should consider: a) the relevant technical vocabularies; b) everyday mental and/or psychological language; and c) the articulations between the two languages.
2. There should be explicit recognition, rather than a denial, as tends to be the norm, of the qualitative differences between the diverse ways of knowing identified in the case of basic and applied projects in psychology. The recognition of these differences prevents falling into simplifications on conceptualising the relationships between ways of knowing, and allows the consideration of new forms of relationship that have up to now been obscured. Such recognition does not imply considering each way of knowing as an island closed in on itself. Quite the contrary, in fact. Although they constitute universes of different empirical generality with different objectives and contextual determinants, they start out from a common matrix characterised by a model of how

to carry out science, technology and practice, a clearly *delimited* object of study, and a minimum meta-system. None of these requirements is fulfilled at present by the scientific and technological projects that make up basic and applied psychology.

3. Up to now, and concentrating on the case of the interrelationship between TEAB and ABA that we have analyzed as an example, efforts to avoid their divorce have consisted in lamenting it, in simplified presentations for professionals of the findings of basic research, or in an insistence on practical training. When the conceptual dimension of the problem has been approached, it has been done so on the basis that the differences between basic science and applications are questions of degree and of work context. In the case of psychology, as far as we are aware, there has been no attempt at the empirical analysis of how individuals adopt and reproduce a set of linguistic and instrumental practices, nor even of the ways in which psychologists actually work, which could follow, for example, Latour and Woolgar's (1986) procedure in their empirical analysis of the interactions between those working in a neuroendocrinological laboratory. As far as psychology is concerned, to our knowledge there is only Skinner's retrospective report on how he carried out his laboratory work (Skinner, 1956).

With the above in mind, we feel it would be interesting to embark on a wide-ranging research project which, based on the historico-conceptual analysis referred to in the first section of this paper, takes into account the following areas of work: a) the empirical analysis of each way of knowing in its own context; b) the empirical analysis of the articulations and/or correspondence between the languages of each way of knowing. This research project would be both transversal and longitudinal. The transversal research would be oriented to the analysis of practices in immediate contexts, whilst the longitudinal research would allow us to discover the process of acquisition and change of systems of practice or *ways of life*. Such empirical analysis might permit the development of a model for undertaking scientific, technological and practical tasks –something that is lacking at the moment–, as well as the determination of the relevant variables that affect their undertaking.

This work constitutes an attempt to adopt a critical perspective with regard to the author's own professional activity, so that it is the object of its own analysis. It has emerged as a product at a given point of his personal and professional development, and therefore does not constitute a complete and final product. At the current time in our disciplinary projects, it is no longer a question of choosing between different schools of thought. The issue is not whether we are behaviourists, cognitivists, psychoanalysts or whatever other name may be given to what we do, but rather which type of metaphysicist we are, according to Turbayne's (1970) description:

> "Those that fall victim to the metaphor accept a way of classifying, grouping or locating facts as the only one that exists for classifying, grouping or situating them. The victim not only has a particular view of the world, but also considers that his is the only possible view, or rather, confuses a special perspective on the world with the world itself. He is, then, without knowing it, a metaphysicist. He has confused the mask with the face. This victim, a metaphysicist *malgré lui*, should be distinguished from the other metaphysicist who is aware that his classification of the facts is arbitrary and could have been different" (Turbayne, 1970; p. 42 of the Spanish translation, 1974).

Which of these two kinds of metaphysicist do we want to be? Which of these two metaphysicists is the reader?

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