Nails, (finger) nails and hammers: scattered notes on Clinical Psychology today [*]

by Massimo Grasso[**]

About ten years ago, towards the end of 1997, at the invitation of the director of the journal "Psicologia Clinica", of which I was then editorial coordinator, I was writing some comments on the past and future of Clinical Psychology for the closing issue of the journal (Grasso, 1997). Today, like yesterday, the same director, who is also the inspiration for the new adventure of which these pages are part, has been kind enough to ask me for my thoughts on the destiny of the discipline. At that time an experience was coming to an end,: today a new experience is beginning because, as he himself says, he would like to "try his luck again". He certainly has courage: I'll give that to him.

I have returned to that piece and found decidedly pessimistic conclusions: in part I think due to the fact that I was perhaps feeling slightly frustrated at the conclusion of a cultural project that over a long period had absorbed too much of my own and of many colleagues' effort and competence; and in part, I still think, due to a realistic, and therefore necessarily depressive view of the facts.

At that time the most significant aspect, the one that had led to the closure of the journal, was the small number of readers, and therefore of interlocutors: because of the journal's wishy-washy editorial policy or due to lack of interest in the issues dealt with? Probably for both these reasons. And what was in store did not seem very different to me.

I was looking towards a future, in short, which did not seem very bright for Clinical Psychology, but as I said on that occasion, my gaze was basically calm, not *worried*. In hindsight, I realise that the lack of worry derived fundamentally from my personal decision to remain involved "up to the elbows", trying to maintain a not-detached or external attitude to the problems and difficulties often debated in Clinical Psychology; trying to measure up to the difficulties, alone or in collaboration with others, from inside the discipline; trying to feel part of a process, to commit myself to that infinite series of *work in progress*, which is, after all, the cultural debate within a discipline.

In short, with or without a journal, as I said returning to the subject a few years later (Grasso, 2001), clinical psychologists, and those who write with them, do not stop existing, working, and - at least some - questioning themselves about their work. Those who want to become clinical psychologists do not stop learning, and making demands for training. To be honest, the adjectives I used on that occasion were different: in fact I spoke about an "increasingly strong, diversified and informed" demand for training. I am no longer so convinced of this, but I do not want to anticipate an idea that I will try to develop later.

In 1997, however, weighing up the results of the journal's cultural life, I was able to enter in the *credits* column several elements that I found significant. For instance, the proposal to record in the journal from both the theoretical and pragmatic points of view, the overcoming of the split between the knowing phase and the intervention phase in psychology, as well as the progressive definition of an integrated model of research, training and intervention. As I stressed then and repeat today, this is a great step forward for the way of thinking and working of many clinical psychologists.

This process seemed to me to involve a progressive liberation from the servitude to a "medical" model of intervention, which triggered misleading theoretical perspectives for Clinical Psychology and obvious short-sightedness at the level of procedure. In parallel, it involved an equally progressive affirmation of a purely "psychological" model, with the power to provide a solid theoretical and methodological anchorage to the multiple facets of psychological action.

Working on the definition of a theory of technique in psychology has meant, from this viewpoint, trying to escape from the logic of the dominant techniques to deal with the purely psychological factor of the complexity of contexts.

---

[*] The rendering stands for the pun used by the author in both the title and the text. This pun is on the double meaning of the English word nail that is correctly translated in *chiodi* [pointing out the object] and *unghie* [pointing out the part of the finger]. (Translator's note).

[**] Full Professor of Clinical Psychology, Faculty of Psychology, "La Sapienza" University of Rome, Italy.

In the *debits* column, on the other hand, I entered the huge obstacles encountered in trying to counter, on equal terms, the strong power of cultural penetration and the social importance of other models of intervention, and consequently, of training in clinical psychology, with the proposal made by our journal: not only in the academic but also, as can easily be understood, in the non-academic domain. This translated into an enormous effort to occupy cultural and institutional spaces, and to establish productive alliances.

It also seemed to me that there were a lot of problems to solve for those willing to do so, and many possible critical areas to be dealt with. I will mention just three of them, which still seem relevant.

1. the need to contextualize techniques, which implies *also* recuperating some techniques as such;
2. the wisdom of a theoretical-methodological depth-study into the psychological relationship between clinical psychology and psychotherapy;
3. the need to rethink the issue of evaluation, treated on all sides in a largely unsatisfactory way, in my view.

From today's perspective, I would add to this what I call the increasing ambiguity and uncertainty of education/training.

The feeling at that time was of the fatigue of working with others on a unified theory of clinical psychology, and of an imperceptible, uncontrollable sliding towards a coexistence that would be a-theoretical and potentially a-conflictual, being without debate, without different models and different representations of clinical psychology itself: to counteract this prospect I felt a process of integration was necessary, in certain respects highly stimulating. However, I am becoming more and more convinced that very few seemed and still seem interested in such a demanding task. The alternative appeared to be for different cultures and models, at times diametrically opposed, to proceed in parallel without creating too any problems for each other.

In the conclusion to my notes at that time I quoted one of Maslow's remarks that appears in one of his well-known essays: "I suppose it is tempting, if the only thing you've got is a hammer, to treat everything as if it were a nail"[1]. The risk I saw then was, as it were, each one with *his* hammer, driving in *his* nails. With the blessing of all, and of each one.

What is the situation that we are facing now? Maslow's conjecture seems to have been translated, from my point of view, from pure hypothesis to convincing oracle.

Let us have a look at the definition of clinical psychology taken from the regulations of the college of university teachers of clinical psychology, in the section entitled "*Ambiti di competenza disciplinare*"[2].("Domains of disciplinary competence").

It is a very important document. In the last few years, clinical psychologists working in universities, with no distinction of academic title, faculty, clinical work or research, have been engaged in producing a document that constitutes a sort of constitution of the discipline, outlining its distinguishing features and distinctive peculiarities.

If one scans the list of points characterizing the domains of disciplinary competence, one can find a clear affirmation of the aspect of a-conflictual coexistence of different cultures from both the theoretical and the pragmatic point of view, as mentioned earlier. Points *5.)* and *6.)* in particular, in which *models* and *methods* are specifically described, seem illustrative. Let us look at them more closely:

Models. Clinical psychology's tradition of research and intervention benefits from a *plurality of models*. These models are grounded on *different epistemological and theoretical-methodological premises*, and feature *inalienable differences* in clinical strategies and in research, both in constant scientific and cultural evolution.[3]

Methods. The methods of clinical psychology are codified in operative protocols, recognised and validated by the various traditions of study, research and clinical application The different diagnostic, evaluational, and therapeutic procedures, while using psychobiological or sociopsychological methods, are described as 'psychological' due to

[1] Maslow A. H. (1969), The Psychology of Science (pagg. 15-16): "I suppose it is tempting, if the only tool you have is a hammer, to treat everything as if it were a nail".
[2] Cfr. www.collegiopsiclinicauniv.it
[3] My italics

the means used and the effects pursued. Among the methods present in Clinical Psychology, an intervention tool that assumes particular importance is the *subjective system of clinical psychology.* This is an emotional, cognitive and relational system constructed through specific training and clinical activity.[4]
…

The document therefore states that, on the one hand, clinical psychology benefits from a plurality of models arising from epistemological and theoretical-methodological premises which represent inalienable differences, and that these differences tend to reappear in clinical and research strategies. I think I can say that this mainly refers to the aspects that founded clinical psychology. Therefore these differences are not only not considered "problematic", but are seen as conveyors of development at a scientific and cultural level. On the other hand, it highlights, as the central tool for clinical psychology practice, the subjective system of the clinical psychologist: i.e. the "system" that is constructed through training and clinical work. Since both training and clinical work must necessarily be based on a model, and since the models are inalienably different from each other, this means that the inevitable result will be new generations of clinical psychologists marked by the epistemological and theoretical-methodological differences mentioned above.
As can be seen, the word "integration" is not used at all. The outcome is therefore that the university clinical psychologists, after a wide-ranging internal debate of admirable level, have reached the conclusion not only that it is not possible to aim for integration, but that basically it is not even to be wished.
At this point, the alternatives concerning the results of such an approach, can only be two:
The first: the affirmation (due to a series of factors: historical, socio-political, cultural, economic, of academic power, etc.), or the struggle for the affirmation of one model, or of an oligarchy of models, sanctioning its supremacy over the others and guaranteeing its hegemony.
The second: each one proceeds within his own model of reference, seeking, by means of stringent debate – but only at this level – a means of integration and of progress compatible with the characteristics of the model itself, and trying to regularly stimulate a shared dimension among the different perspectives, if possible on neutral ground, where with equal dignity (a sort of scientific par condicio), information and updates on results obtained and possible developments are exchanged, or at least made public. It is this second alternative that seems to be the option being pursued by the college with conviction and continuity5.
And one does not necessarily rule out the other.
If this is the authoritative opinion that the university teachers of clinical psychology have expressed – that we university teachers of clinical psychology have expressed - then it must be taken into account. I mean, and I say this above all to myself, also in view of this new scientific editorial adventure.
It would seem to me to be more appropriate in this sense to talk not so much about clinical psychology as about clinical psychologies. Although, on looking closer, under the concept of plurality of models one can discern, without much difficulty, the reference to the plurality of psychotherapeutic models. This is confirmed by seeing what the above-mentioned document containing the regulations of the college of university teachers of clinical psychology says about the definition of clinical psychology, the first point to be considered in the "Domains of disciplinary competence" mentioned earlier, in point 1.):

Definition. Clinical psychology is a sector of psychology whose aims are the explanation, the understanding , the interpretation and the reorganization of dysfunctional or pathological individual or interpersonal mental processes, together with their behavioural and psychobiological correlates. Clinical psychology is identifiable with psychological methods aimed at consultation, diagnosis, therapy or at any rate intervention on the individual or group psychological structure and organization, in its problematic aspects of suffering and maladjustment and in

[4] My italics
[5] On this see the two conferences organized by the college in the last two years: the first in 2004 (2-4 aprile), at Naples and Ischia, entitled "*La psicoterapia: sviluppi ed innovazioni*"("*Psychotherapy: developments and innovations"*); the second, again at Ischia, in 2005 (8-10 aprile) entitled: "*Formazione e ricerca in psicologia clinica*"(*"Training and research in clinical psychology")*.

its interpersonal, social and psychosomatic ramifications. Clinical psychology also aims to promote conditions of socio-psycho-biological well-being and related behaviours, through interventions that may be preventative, in the various clinical and environmental situations. *In its different strategies and methods psychotherapy constitutes the applicative domain that most characterizes clinical psychology, as the point of greatest convergence between demand, available psychological knowledge, phenomena studied and usable methods.*[6]

The last part of the definition therefore explicitly shows the weight and importance that psychotherapy as a praxis is believed to have for clinical psychology, characterizing its identity as a disciplinary and scientific domain[7]. However, talking about psychotherapy generically does not have much sense either from the historical point of view (Reisman, 1991; Freedheim, 1992; Tallis, 1998) or from that of concrete clinical operativity (Wachtel & Messer, 1997; Cionini, 1998; Codispoti & Clementel, 1999): it is obviously more correct to talk about models of psychotherapy, the existing models and those that are constantly being identified, or that in the future will be proposed.

Here then we see that this discourse seems to tie in closely with what was presented before. In other words, it is claimed: clinical psychology is substantially psychotherapy and, given that as far as psychotherapy is concerned, from the theoretical and the applicative point of view, the legitimacy of its theoretical models and different operative approaches must be recognised, then it is absolutely necessary to underline its parallel right to existence, stating that, as we have seen, the diversity of the epistemological and theoretical-methodological premises benefit the progress of clinical psychology as a discipline. It should however be remembered and stressed that the differences we are talking about are defined *inalienable.* Inalienable for whom? The answer, not too malicious, that springs to my lips is: clearly for the psychotherapy models themselves, for the respective training schools, for the respective domains of intervention, for the respective areas of cultural diffusion. Evidently every psychotherapy model has, or believes it has, too much to lose in pursuing whatever goal of integration regarding the foundations of a clinical psychology theory of technique: as we said, then, each one with *his* hammer, driving in *his* nails. With the blessing of all, and of each one: with the guarantee for all and for each one, of his own "hunting" ground, his own spaces of recognition, and his own *audience.*

However, it may not all be quite such "plain sailing". When in 1997, I quoted Maslow, the comment I made seemed and still seems appropriate: it is certainly a paradoxical and ironic coincidence, but the original English *nails* which we correctly translate *chiodi*, also means *unghie (finger-nails).* And perhaps, by dint of driving in one's nails with one's hammer, one runs the risk of hitting a few (finger) nails. Examples? I will give a few, which seem significant, without claiming to be exhaustive.

I am given the opportunity to provide a first example by a brief foray into and subsequent comment on the issue of evaluation which, as you will remember, I listed as point *3)* of the critical areas to be dealt with.

Elsewhere I have recently explored (Grasso & Stampa, 2006a) the issue of models of psychological normality or of mental health that are required to serve as the background to a research project on the effectiveness of psychotherapies[8]. I will take up several of the points here[9].

Clinical research designed to measure the efficacy of psychotherapies involves recording and measuring the patient's "progress". What models of "psychological normality" or "mental health" are

[6] My italics

[7] Not far from this view is the ministerial declaration defining the clinical psychology scientific-disciplinary sector. In fact it says:
"The sector comprises competences related to methods of study and intervention techniques which, *in the different operative models (individual, relational, family and group)*, characterize the clinical applications of psychology in different domains (persons, groups, systems) for the solution of their problems. In the fields of health, of psychological distress, of psychological aspects of psychopathologies (including psychosomatic, sexological, drug addiction), these competences, extended to psychophysiology and clinical neuropsychology, are aimed at the analysis and solution of problems through interventions of evaluation, prevention, psychological rehabilitation and psychotherapy". [My italics]

[8] Cfr. also Grasso, 2002, 2005.

[9] A longer version of this paper can be found in this issue of *Rivista di Psicologia Clinica on line* (Grasso & Stampa, 2006b).

such models oriented towards? What models of "psychological normality" or "mental health" is this "progress" oriented towards? If we examine some of the contributions which deal with the question (Vaillant, 2003; Wakefield, 2004) we can obtain paradoxical results, simply by following the extreme consequences of an intrinsically ideological vision of the problem, not recognised by the authors. This ideological vision could be expressed as an equation which sees conformity equal to normality equal to health: the feeling of subjective well-being, psychological and/or physical, corresponding to upholding current values and lifestyles, or to the recognition of one's membership of a sector of society, is assumed in an a-critical, linear way, to be the basic parameter of clinical judgement.
In this sense, at least from my point of view, one cannot help having difficulty sharing the perspective that identifies the *Mercury programme* selection process for the first seven American astronauts in the '60s, as the prototype for research aimed at a scientific definition of mental health.
On this, the already cited Vaillant (2003) states:

> This study underlined both the importance of mental health and its links with common sense. The seven astronauts did not just have exemplary CVs, but they were also capable of loving. They all came from *happy united* families[10] in small towns. At around thirty, they had all married and had children. Although they were adventurous test pilots, they had had few accidents in their experience as pilots and also in the previous period. They could bear both close interdependent collaboration and extreme isolation. They trusted others and they did not complain in situations of discomfort. They showed high tolerance for positive and for negative emotions. As they were not particularly introspective, they rarely showed their inner emotions, but were able to describe them if asked. They were aware of the feelings of others and avoided interpersonal difficulties.

If such a description can be considered a valid example of what is scientifically meant by mental health, where is the positive change or the innovation compared to older models that conceived mental health as a utopian ideal or saw it as strictly linked to morality, models which the article cited and common sense tell us to reject? Can it be that the aims of such statements are not as "purely" scientific as they would have us believe? It would seem so: these assumptions, seemingly so elementary, in the definition of highly problematic concepts like that of mental health and illness, avoiding the slightest confrontation with any paradigm of complexity, perhaps have as their main reference point the aim of social control. But that is another story, which I have dealt with more analytically in the works mentioned above, and which anyone interested should consult.
It is obviously not a matter of countering an ideological vision based on the exaltation of conformity with an alternative vision based on other values. It is a matter of deciding whether these models are already paradoxical in themselves, purely on the level of method. Allow me here to skip some steps in my reasoning: the basic root of the problem probably lies in the use in psychology of the scientific paradigms on which medicine relies. This use is as common as it is incorrect.
How did the "medical model" get into clinical psychology and psychotherapy? As Nesse and Williams (1994) write:

> Powerful forces have pushed psychiatry to adopt this "medical model" for psychic disorders. The change began in the Fifties and Sixties, with the discovery of pharmacological treatments against depression, anxiety and symptoms of schizophrenia. These discoveries pushed the government [of the United States] and the drug companies to allocate funds to research into genetic and physiological correlations to psychic disorders. To define the latter and to be able to compare data from different studies, a new method of psychiatric diagnosis was created, drawing a clear line around different sets of symptoms, instead of seeing them as unbroken gradations of emotions caused by psychological factors, past events, and particular experiences in life. University psychiatrists became more and more interested in the neurophysical causes of mental disorders. Their hypotheses were conveyed to Internal medicine specialists in training seminars for general practitioners. Lastly, with the spread of health insurance policies and the availability of federal funding for medical assistance, psychiatry associations began to lobby for the disorders they treat to be classified as organic illnesses like all others, and consequently eligible for the same insurance cover.

[10] My italics

An American “story”, one will say. And then, if certain substances change the mood or the behaviour in the expected way and in the direction desired by the patient, if they encourage funding for the research centres, if they are compatible with the control criteria of the insurance companies, what’s the harm?
The fact is, the “medical model” is imperialistic, as American culture often is in other sectors of community life. From “practical” psychiatry with biological underpinnings, the “medical model” (and insurance model) filters into psychotherapy: on this, we might recall the psychotherapist/patient agreement illustrated in the voluminous, authoritative handbook of Davidson e Neale (1986). Stampa (1990) sums it up thus:

> The therapist undertakes to “try to help the client” to achieve “goals of modifying his behaviour” which are carefully itemized. This is followed by a precise description of the techniques to be used, and the sources giving the clinical indications for those techniques in relation to the client’s disorder; the predicted dates by which the improvements expected by the client will progressively manifest themselves; lastly, an assessment of the probability of “undesirable side effects”, which are also described and for which the therapist indicates the methods of monitoring. In his part of the document, the client formally acknowledges these declarations, and in turn declares that he will provide the therapist with all the information needed to enable his/her professional intervention, as described above; he undertakes to pay the agreed fee, which is specified precisely. The agreement is also countersigned by a witness.

It is impossible to understand this approach unless it is in a context in which the private insurance companies determine the market, laying down certain strict conditions:

> (a) the diagnosis must be made according to standardized forms; (b) the treatment must be *diagnosis related*, exactly as in medicine and surgery (*n* days for viral hepatitis; *n* days for a fractured ulna: how many sessions for a phobia?); (c) at the end of each session patient and therapist agree on a account of the session, with an evaluation of the step reached.
> In other words, it is a series of requests made-to-measure for a strictly medical and behaviourist approach.

Except that psychotherapy, as a psychological intervention within a relationship, cannot adopt the same forms used in a pharmacological treatment. Unless psychotherapy is seen as a purely anti-symptomatic intervention…And here there is a set of very different objections.
But how is this discourse connected to the hammers, nails, and (finger) nails above? Simply by taking into account that reducing clinical psychology to psychotherapy involves, on the one hand, justifying the equivalence of the various psychotherapy models from a theoretical and methodological viewpoint. This leads, on the other hand, to the hegemony of some models and their related evaluation methods, namely, those that are more compatible with the naively (or non-naively?) dichotomous approaches mentioned above, increasing the temptation for the other models – if one wants to publish in high impact international journals, and therefore have research funding, participate in conferences, get promotions etc. – to avoid confrontation and give up the connection with one’s fundamental hypotheses for the sake of adhering, often a-critically, to simplifications of the kind cited earlier.
I think this is what is happening consistently in our country to therapeutic models with a psychodynamic or, to say it more clearly[11], psychoanalytical orientation.

11 Also in this case, the ministerial declaration on the “dynamic” scientific-disciplinary sector makes a clear reference to psychotherapy:
“It includes scientific-disciplinary competences that consider from a psychodynamic and psychogenetic point of view, the representations of the self, intrapsychic processes and interpersonal relations (of family and group) as well as the competences related to the applications of this knowledge to the analysis and *treatment of psychic distress and of psychopathologies*. It also includes scientific disciplinary competences related to methods and techniques characterizing the studies in this disciplinary domain”. [My italics]

Reducing complex concepts in order to *operationalize* them is a current issue. Let us take, as an example, one of the certainly most difficult and controversial aspects of the therapeutic relationship in a psychoanalytical frame: countertransference.

How should we explain this, if not through the arguments advanced earlier, for instance the creation of the *Countertransference Index* (CI) (Hayes, Ricker & Ingram, 1997), a tool made up of a single item, which thanks to a *Likert*-type scale, indicates how far the behaviour of the therapist has been influenced by countertransference, in the session just completed[12]?

Or, again on the same issue, how should we view the process that led to the construction and use of the *Inventory of Countertransference Behavior* (ICB) (Gelso & Friedman, 2000), where the initial 32 *items* describing countertransference behaviour were evaluated by 11 psychologists judged to be experts on the question of countertransference (who by? And above all, based on what?), who, using the familiar *Likert* scale, where 1 indicated that the *item* did not express any type of countertransference behaviour and 5 indicated that the item was an excellent indicator, chose the 21 that were later used?

In these cases are we not faced with a massive process of simplification? And what can be said of the measurements that are obtained with these tools? Judging by the uses that are made of them, which can be found in the literature, there are two types of results.

On the one hand, there is the *concretization* of the concept studied: what is obtained on countertransference, in other words, is not seen as - at the best - a probabilistic, if not casual estimate of the behaviours linked to countertransference dynamics, but it *is* countertransference. This later goes so far as to indicate for instance that countertransference itself, the theoretical outlines and delineations of which, after countless research projects, have been progressively lost, is defined and constituted by eight dimensions, which are perhaps just the result of a simple factor analysis[13]. Apart from the fact that, concerning this last example connected to the *ICB*, the training of the clinical operators involved in the study was psychodynamic, eclectic (?), cognitive-behavioural: but doesn't the concept of countertransference emerge and develop within the psychoanalytical tradition (Freud, 1937; Heimann, 1950; Racker, 1968 and, obviously, many others)? How must it have been (if it has been) conceptualized elsewhere? For instance, in the eclectic perspective, (depending naturally on what type of eclecticism we are talking about)? [14] I do not think this is what is meant by integration of perspectives, which is a long and complex process of cultural confrontation; this seems more like a *melting pot* where every distinctive characteristic is lost for the sake of a vague *empirics*.

On the other hand, there is a more cautious attitude which, aware of the critical nature of some of the assumptions, anyway claims the right/duty to propose evaluations and/or possibly rudimentary conceptions (because it is said, there are none better at present) to be progressively refined later: the problem is that, in view of what we have seen, these are often not inaccurate measurements that can be perfected later, but they are simply *non* measurements[15]. The not infrequent outcome is to leave

[12] And what happens to the psychotherapist in relation to a certain patient, *outside* the therapeutic setting?

[13] Specifically: *Overwhelmed/Disorganized*, *Helpless/Inadeguate*, *Positive*, *Special/Overinvolved*, *Sexualized*, *Disengaged*, *Parental/Protective*, *Mistreated/Criticized* (Cfr. Zittel, Westen, 2003).

[14] Can countertransference be, as is said today, *a pan-theoretical concept*? This little formula is used for instance in the therapeutic alliance (Horvath, 2005). It seems to me to be a shortcut to avoid having to use conceptual rather than empirical tools to account for and give meaning to what happens in the relational dynamics of psychotherapy.

[15] Some further examples of simplification and of reductionism?

A first example. The interesting book by De Coro and Andreassi (2004), which provides a broad panorama on empirical research in psychotherapy, quotes, about efficacy studies on childhood psychotherapy, the research by Casey and Berman (1985), considered by experts of the sector a turning point in studies on the subject. Using the technique of *meta-analysis* the two researchers analysed 75 research works on childhood psychotherapy (patients from 3 to 15 years of age) carried out between 1952 and 1983, with the aim of providing an *empirical* answer to questions like, "Does psychotherapy work with children?", obtaining for the first time a more than affirmative answer with an *effect-size* of 0.71. One wonders, with children? From 3 to 15 years of age? One wonders if Casey and Berman have ever even *seen* a teenager of 15 and a child of 3, to be able to

point A and through a series of steps, to reach a point B that has nothing, or very little, in common with the aspect of departure: this is a risk that Horvath himself underlined at the recent conference of the *Society for Psychotherapy Research*[16].

The paradoxical outcomes do not only concern some of the contents of scientific research in our domain, but also the framework supporting international recognition of the validity of research. Let us look at the question of *impact factors*, mentioned above.

I do not want to take up the criticisms of *IF* expressed authoritatively and extensively by others[17], which I however substantially support; let me just recall the work of Vaillant (2003), referred to above.

Now, whoever makes justified criticisms of his statements (cfr. for example Wakefield, 2005, whose criticisms are also rather harsh concerning the theoretical-methodological consistency shown by Vaillant) obviously cannot help citing him. Let us suppose, for a hypothesis, that Vaillant's article raises not just a few isolated criticisms, but a chorus of criticisms: the consequences will be that, instead of being censured for publishing an article gaining so little support from the scientific community from the theoretical and methodological point of view, and instead of being shunned by other scholars, the journal that published the article (*IF* = 6.913) will instead see its *IF* shoot up along with its appeal, since, as we know the number of times a journal is cited compared to others, represents a growth index for the impact of that journal.

One is tempted to say that this is a little like what happens with the phenomenon of the ratings of the worst *trash TV*, in which the *approval rating* has long been replaced by the *viewers rating:* it does not matter whether people talk well or badly about a programme, what matters is that they talk, because this means it has been seen, regardless of its value which may be – and often is – execrable, and only in this way can it be *sold* and be appetising for rich *commercials*.

Another example of crushed nails? I would like to cite two phenomena that struck me particularly and seem to represent a demonstration of the erosion, from the left and the right, as one would once have said (the reader can decide where to place right and left, now that the identification is not longer so immediate) of spaces not only of intervention, but also of the cultural presence of clinical psychology.

The first phenomenon is in my view the increasing diffusion of *counselling* (Nelson-Jones, 1995), with the concomitant training courses, requests for professional recognition, etc. etc. All this has gone on in parallel with a progressive detachment from the psychological matrix and more specifically from the clinical psychology matrix of this practice. The process of such a purely psychological practice moving away from psychology reaches its acme with the return of *philosophical counselling*, whose birth-date scholars of the sector agree to set at about twenty-five years ago.

The discipline of philosophical consultation aims, as one can amply read on the web, to provide help and orientation to individuals who are not able to cope alone with the problems of daily life. The task of the *counselor* is to help people to reflect on their professional and affective choices, on relational and

---

see them as comparable. One also wonders whether the two researchers have ever read or heard for instance of the research of Estes (1938), student of Allport, that led to the hypothesis of a "*trained incapacity*" to evaluate people for those who had studied psychology in the 1930s.

A second example. Respecting privacy, I will tell what happened recently in a study meeting on research in psychotherapy. In illustrating some research work, the reference sample (?) was presented, of subjects between 15 and 65 years of age. One of the round table panel made an observation, aimed at pointing out the difficulty of such a group (not sample!) due to the age disparity of the members and the lack of precision on their socio-economic level. The disarming reply was, "You can't take everything into consideration. Clinical research is not an easy matter!"

Underlying all this is what I would call a sort of quantification *fetishism*. Statistical analysis is transformed in these cases from a tool into an actual research objective: one carries out research so as to quantify, regardless of what and how one is quantifying. And yet again, ignoring three complex systems one is dealing with: is there any need to recall the recent "disadventures" of the statistical analyses to describe voter behaviour in Italy, as earlier in the United States?

[16] Cfr. A.O. Horvath's contribution at the round table "*L'integrazione dei trattamenti e la ricerca*", at the National Conference of the Italian Section of the *Society for Psychotherapy Research*, San Benedetto del Tronto (AP), 16-18 September 2005.

[17] Cfr. for example Figà-Talamanca, 2000.

emotional dynamics and on the recognisable meaning of any states of anxiety or depression experienced.
Showing some enthusiasm, Galimberti in the daily paper "*la Repubblica*"[18] at the end of 2004, announced its spread to Italy in these words:

Those who ask for a philosophical consultation are not "sick", but just in pursuit of meaning. And where can a meaning be found, or rather the meaning which flows, underground and ignored, through our lives unbeknown to us, if not in the proposed meanings embodied in philosophy and its history? Thus it was, that in 1981 the German philosopher Gerd Achenbach opened the first studio of Philosophical Consultation in Germany, which was followed by the foundation of a Society for Philosophical practice, later becoming international with its spread to Holland, France, the United States and Israel. In 1999 the *Italian Association of Philosophic Counselling* was set up in Italy. Books translated include those by Pierre Hadot, *Spiritiual exercises and ancient philosophy* and by Lou Marinoff, *Plato is better than Prozac* and *Le pillole di Aristotele* which bring the general public nearer to the figure of the great philosopher who is interested in individuals without claiming to be a "therapist". In 2003 the Faculty of Education at the university of Catania organized the first conference on Studies on Philosophical practices, while the university of Venice introduced the first course as part of a broader project on philosophical practices, which also envisages a Masters on the subject. The Turinese hospital 'Le Molinette' has employed a philosophical consultant alongside a psychologist at the counselling centre for its employees, while Quartiere 4 of the Municipality of Florence has set up a public service of philosophical consultation in an extension of social services available to the citizenry.

And also:

And those who refuse to hand themselves over to the deadening, since they still have a certain self awareness, who do they turn to when they encounter not this or that pain, around which the psychotherapies buzz, but the essence of pain which is the impossibility of finding a meaning? Here the psychotherapies are no use because it is not "pathological", as they would have us believe, to ask oneself questions, evaluate one's ideas, examine one's vision of the world to see how narrow, how limited, how fossilized, how rigid, how forced, how unsuitable it is to cope with the changes in one's life and such rapid and unpredictable changes in the world. If not all pain is pathological, an answer to this type of suffering and distress, rather than by psychotherapy, can be obtained from philosophy...

It seems clear that psychotherapy, and along with it clinical psychology, are fully assimilated to the idea of "illness", of being cured of a "pathology" which must be grounded in a decidedly medical perspective, to the happiness of those clinical psychologists who heat up their watered-down identities in the comforting warmth of a white coat. The result however is a progressive erosion of the cultural and professional presence in domains of so-called "normality".
Emblematic, in this regard, is the title of one of the books quoted by Galimberti: *Plato is better than Prozac*. This ultimately seems to be a pattern that is neither excessively obscure nor excessively new: on the one hand the medical-psychiatric intervention on mental disorder, on the other the *philosophical* intervention on existential problems[19]. And psychology? Clinical psychology? Its specificity? Paradoxically missing. The stress on psychotherapy as the foundation of clinical psychology involves, I think, this type of drift.

[18] Cfr. U. Galimberti, *Se un filosofo ti prende in cura*, "la Repubblica", 15 December 2004
[19] Wandering round the *web* it is possible to make some strange and interesting discoveries. For example, you can find that there are at least two important associations connected to *philosophic counseling* (and I am certainly neglecting others, to whom I apologise): one, with some kind of psychological contamination, at least in its title, is AIP (*Associazione Italiana Psicofilosofi*), the other SICOF (*Società Italiana di Counseling Filosofico*), which has its own school, a journal, and even a professional roll (?) with a list of members. Gleaning information from the CVs that are offered by the most representative exponents, one can also read that some of them can boast a basic training that includes specializations in psychiatry, and also auxiliary professional competences linked to hypnosis, sexology, and even to electroconvulsive therapies, shown by teaching experience and publications: as the old adage warned, *primum vivere, deinde philosophari (First live, then philosophize)*

It is up to the psychiatrist, then, to deal with pathology, and to the philosphical *counselor* to help to understand the roots of dissatisfaction with daily life, affective relationships, work: in a word it is up to the philosophical *counselor* to act as the psychologist, the clinical psychologist.

The second phenomenon that I want to draw attention to concerns recent Italian ministerial decisions assigning post graduate University Specializations in Clinical Psychology to the medical teaching area and introducing them in the Faculties of Medicine and Surgery, with the corollary of a consequent change in title from schools of specialization in clinical psychology to schools of specialization in psychotherapy. The considerable protest caused by these events, consisting of legal recourse through the TAR, announcements by the regional and national order of psychologists, requests for the opinion of the National Council of Universities, shows, in my opinion, how rash it is for clinical psychologists to assimilate *tout court* clinical psychology to psychotherapy from the cultural and scientific point of view: this involves yet again the risk of losing our own specificity, with the consequent possibility of being swallowed up by powers that are culturally stronger.

It should not be forgotten that the Schools of Specialization in Clinical Psychology are structurally connected to the National Health Service. In view of what has been said above, this would seem to mean that the presence of clinical psychologists in the local health facilities is found necessary only as dispensers of psychotherapy: what about the work of consultation, analysis of contexts, organizational aspects, and training?

It is a little strange that these facts correlate so well with phenomena of the type mentioned earlier concerning *philosophical counseling*: they are both products of the simplification and the reductiveness that now govern so many aspects of our social, political and cultural lives and that are advancing at an unstoppable pace, promoted by the progressive crumbling of culture and by the specific process of impoverishment of the education system, caught between the lack of resources and planning and the scarce appeal amidst to the dominant social values.

And we have arrived at the thorny problem of education/training. As will be remembered, at the beginning of these notes, I indicated my scepticism about the demand for education (obviously in the field I am dealing with, i.e. clinical psychology) which does not seem to me to be “increasingly strong, diversified and informed” as it appeared only five or six years ago from my specific observation post of university education.

I do not believe I am alone in reporting a strong sensation of general decline in the educational domain: certainly, there are still motivated, hard-working, responsible students, and the prospects we can offer them have considerably diminished in recent years. But the majority of those in education, stunned like their teachers by the mindless roundabout of reforms and counter-reforms which see them not as privileged referents, but as sacrificial victims, bewildered by the empty rituals of credits and the resulting courses parcelled out in lots of 350 pages (not one more!) of preparation, confused by the redundancy and the contradictoriness of laws and organizational constraints, seem to be devoured by the demon of “go fast” rather than “go deep”, in sight of a finishing line which is often hazily defined both in professional and scientific terms[20]. Or it might be more correct to say they ‘seemed’. I think that in the last year or year and a half, we have been seeing a progressive abandonment of even this last sterile chimera: in other words, “go fast” now seems just as ineffective as “go deep”, which is too complicated and difficult. The only thing that remains is to “go” just for the sake of it, one would think.

There is then one further paradox concerning clinical psychology.

On the one hand, it consists of the fact that the previously recalled plurality of models underlying the discipline obviously also tends to produce a multitude of *inalienable* differences in the “clinical” training projects that have taken shape with varying degrees of success in different universities in our country, with the formula known as “3+2”.

[20] See the *flop* of section B of the Order of psychologists, devoted to *three-year short degree graduates*. The Order of psychologists of Lazio, the largest in Italy, compared to 11,000 registered members in Section A in 2004,of whom over 5,000 were psychotherapists, had only 14 in Section B.

On the other hand, the ministerial "simplification" (another one!) of disciplinary sectors means that in order to guarantee a basic skeleton for the "clinical course", more attention is paid to the outer form of the sum total of different elements than to the substance of contents that could be integrated. In other words, referring for example to two "clinical" courses set up at two different universities, any subject whatsoever, as long as its label corresponds to the sector (for clinical psychology the sector has the code M-PSI/08), is thought to be comparable (superimposable) to any other subject of the same sector[21]. It is as if two baskets of fruit were considered equivalent, regardless of the fact that in one there are mainly oranges and in the other mainly apples, not to mention the different types of oranges and apples. The result is that the presumed and vaunted possibility of moving from one basic course to another, from a basic course here to a specialistic course there, is in fact unfeasible or feasible only in theory: to go back to the fruit, the flavours remain different, one tends to drown out the other or be drowned out, and the fruit salad that sometimes emerges is often inedible or at least not easy to digest for a body that is already "exhausted". And the further paradox is that around all this there is a feverish activity of evaluation (evaluation nuclei, *ad hoc* commissions etc)[22]: evaluation that can be a mere procedure or an eminently cultural fact.

But if from a process of contextualization and integration of disciplinary contents, education/training is reduced, in the best cases, to a mere juxtaposition of perspectives, it actually gives up its role as the phase of pre-eminent importance in providing the basis for and in developing a culture of critical evaluation. This is not so much or not only at the level of the transmission of knowledge, so much as on the more substantial level of the experiential maturation of the players involved in the education process. This is like saying that it is impossible to think of educating for critical evaluation without necessarily going through evaluation in education.

In conclusion? I must admit that dealing with the issues I have commented on in these notes I cannot help feeling an unpleasant sensation of *déjà vécu*. It seems after thirty years working in the domain of clinical psychology, that some issues come up in cycles, that some conflicts seem to reproduce themselves, as if what has been said, done, written on them had not left the slightest trace, had not led to change in any aspect, had not left a single memory.

On the matter of the atomization of behavioural dimensions, at times indiscriminate, resulting from measurement procedures of which we have seen some examples above, is it really necessary to recall Rapaport? In the '50s, referring to a debate that was going on at the time, Rapaport, talking about evaluation and measurement, distinguished between "molar" and "molecular" behaviour and with reference to gestalt conceptions, maintained:

> units of behaviour are those units that are whole, in that they have a self-contained meaning, but they would lose it if they were further subdivided. Gestalt psychology calls all units molar units, in contrast with the molecular units that are obtained by making subdivisions that take no account of meaning (pag. 188).

And again, still in 1950, he warned against the danger of reification, consisting of giving conceptual constructs used to express testing data the value of relations *really* operating in the psychical organization of any individual.

---

[21] So it is also theoretically possible for courses that call themselves "clinical" not to contain any subject called "clinical Psychology" (no matter what the container holds, obviously!) but to offer other subjects coded M-PSI/08, entitled roughly "freely inspired by …".

[22] One may find for instance that our students' level of "satisfaction" is around 80%, plus or minus a few decimal points. So everyone seems happy: students, teachers, degree course, faculty, university. This ignores the fact however that the percentage is calculated on the attenders, i.e. on those who can be "tested". If, for instance, those who attend are 30% of the potential users (out of a hypothetical total of 250, say 75 students), is it sensible to be reassured by 80% of these "happy" students (60), or isn't it wiser to wonder about the over 75% in total (190 students) who, since they do not attend or are not satisfied, may not be exactly "happy"?

And so it is rather like digging a hole in the sand: it does not take much for the hole to cave in with all the sand that has been dug out and piled around the edge, and one is back to the start. As likely as not the digging starts again[23].
If so, then it means we are dealing with a problem that is first of all a cultural problem. Within our discipline on the one hand, and on the other amplified by the connotations being assumed by social contexts: i.e. by the superficial values pushed by advertising, by the stress on a culture of appearances and sensationalism fed by an invasive media so often devoid of all content apart from the amplification of surface appearances, by the already mentioned tendencies to simplification, generalization, reductionism and conformity. I will stop for I realise that the risk I run is of assuming the tone of a Savonarola and that is absolutely not my aim: I simply want to point out that, in my opinion, this is the context that we must acknowledge and which can be confronted, by those who are willing. Each of will decide if his hammer hits more nails or more (finger)nails.
The *adventure* triggered by this online journal seems to me capable of making a serious contribution to the non dogmatic or militant analysis and exploration of these problems. This is my hope and for my part I will work in this direction.
Although, if I must express my own feeling, I am not over confident.
I am however calm: because if my feeling of scepticism and lack of confidence should be transformed into a real existential crisis, I know I can turn to someone who will help me rediscover trust and meaning. Outside the field of psychology. Of clinical psychology in particular.
For example, I could contact the *Center Sophon* which deals with precisely these problems, and with excellent results it seems, although unfortunately its headquarters are in Jerusalem.
"*No problem*", anyway, as the trendiest exclamation of our time says: because the *Center Sophon* has for some time been offering the *Philoso-phone* service which intends, over the phone, to provide "*a first-aid hot-line for ethical dilemmas and existential problems*"[24].
On the subject of nails/(finger)nails and of circumstances that tend to compromise whatever situation, the English have a slightly macabre but effective saying, that someone or something can be *a nail in the coffin* for a particular situation: is that too pessimistic for clinical psychology and what surrounds it? Perhaps, and I hope I am proved wrong: but meanwhile, looking around me, it seems that so it is… if we like it. And also if we don't like it.

References

Casey, R. J., & Berman J. (1985). The Outcome of Psychotherapy with Children. *Psychological Bulletin*, *98*, 388-400.

Cionini, L. (1998). *Psicoterapie. Modelli a confronto* [Psychotherapies: comparing models] . Roma: Carocci.

Codispoti, O., & Clementel C. (1999). (Eds.). *Psicologia clinica: Modelli, metodi,trattamenti* [Clinical psychology: models, methods, cares]. Roma: Carocci.

Davidson, G.C., & Neale, J.M. (1986). *Abnormal Psychology. An Experimental Clinical Approach* (II ed.) New York: John Wiley & Sons (Trad. it. *Psicologia clinica,* Bologna, Zanichelli, 1994).

De Coro, A., & Andreassi, S. (2004). *La ricerca empirica in psicoterapia* [Empirical research in psychotherapy]. Roma: Carocci.

Standard 26/2/13 18:40
Formattato: Tipo di carattere:10 pt

[23] On the other hand this alternation of "tides" and the consequent feeling of stagnation is not just typical of clinical psychology: it is also found in the broader social field, in culture, in politics. Perhaps we should "have a change of air", emigrate, as some authoritatively maintain these days?
[24] Cfr. *www.geocities.com/centersophon/pc-sophon.html*

Figà-Talamanca, A. (2000, october). *L'Impact Factor nella valutazione della ricerca e nello sviluppo dell'editoria scientifica* [The impact Factor in the research evaluation and in the development of the scientific publishing]. Paper presented at IV Conference National Informative System for Maths SINM 2000: un modello di sistema informativo nazionale per le aree disciplinari [Informative system model for the disciplinary areas] , Lecce.

Freedheim, D. K. (1992). *History of Psychotherapy: A Century of Ch*ange Washington, DC: American Psychological Association (Trad. it. *Storia della psicoterapia: Un secolo di cambiamenti.* Roma: Edizioni Scientifiche Ma.Gi, 1998).

Freud, S. (1937). *Die endliche und unendlicheAnalyse,* Psyche: Zeitschrift für Psychoanalyse und ihre Anwendungen (Stuttgart), 44: 9 *(*trad. it. *Analisi terminabile e interminabile,* Torino, Boringhieri, 1979).

Galimberti, U. (2004, 15 december). Se un filosofo ti prende in cura [If a philosopher care for you]. *la Repubblica.*

Gelso, C. J., & Friedman S. M. (2000). The Development of the Inventory of Countertransference Behavior. *Journal of Clinical Psychology*, *56*, 1221-1235.

Grasso, M. (1997). A proposito della chiusura della Rivista *Psicologia Clinica*: qualche riflessione sul passato e sul futuro della Psicologia Clinica in Italia [About the closing of Rivista Psicologia Clinica: remarks on the pass and the future of Clinical Psychology in Italy]. *Psicologia Clinica*, *3*, 101-105.

Grasso, M. (2001). *Introduzione* [Introduction]. In M. Grasso (a cura di), *Modelli e contesti dell'intervento psicologico* [Models and contexts of psychological intervention] (pp. 13-22). Roma: Kappa.

Grasso, M. (2002). Alcune note sul rapporto tra etica e competenza in psicoterapia [Remark on the relationship between ethic and competence in psychotherapy]. *Psicologia clinica e Psicoterapia psicoanalitica*, *1*, 59-69.

Grasso, M. (2005, April). *Psicologia clinica come correzione di un deficit o come promozione di sviluppo negli insegnamenti delle Facoltà di Psicologia e di Medicina* [Clinical Psychology as deficit correction or development promotion for the teaching in Psychological and Medical Faculties] . Paper presented at conference Formation and Research in Clinical Psychology, Ischia.

Grasso, M., & Stampa, P. (in press). *Diagnosi psichiatrica e modelli di salute mentale: osservazioni su alcune criticità metodologiche per la ricerca in psicoterapia*.

Grasso, M., & Stampa, P. (2006 September). *Chi ha slegato Roger Rabbit? Diagnosi psichiatrica e modelli di salute mentale: osservazioni su alcune criticità metodologiche per la ricerca in psicoterapia* [Who *un-framed* Roger Rabbit? Psychiatric diagnosis and mental health models: observations on same methodological criticality for psychotherapy research]. Paper presented at the national conference SPR/Italia, Research in psychotherapy and quality care, San Benedetto del Tronto (AP).

Hayes, J. A., Ricker, J. R., & Ingram, K. M. (1997). Countertransference Behavior and Management in Brief Counseling: A Field Study. *Psychotherapy Research*, *7*, 145-153.

Heimann, P. (1950). On Counter-Transference. *International Journal of Psycho-Analysis*, *31*, 81-84.

Horvath, A. O. (2005). The Therapeutic Relationship: Research and Theory. *Psychotherapy Research*, *15*, 3-7.

Maslow, A. H. (1969). *The Psychology of Science: A Reconnaissance.* Chicago: Gateway/H.Regnery Co.

Nelson-Jones, R. (1995). *The Theory and Practice of Counselling.* London: Cassel.

Nesse, R.M., & Williams, G.C. (1994). *Why We Get Sick? The New Science of Darwinian Medicine.* New York: Times Books (trad. it. *Perché ci ammaliamo?* Torino, Einaudi, 1999).

Racker, H. (1968). *Transference and Countertransference*. London: The Hogarth Press (trad. it. *Studi sulla tecnica psicoanalitica.* Roma, Armando, 1970).

Rapaport, D. (1967). *The Collected Papers of David Rapaport* [Gill M.M.,Ed.]. New York: Basic Books, (trad. It. *Implicazioni teoretiche dei procedimenti psicodiagnostici.* In M. M. Gill (1977). (Ed.). *Il modello concettuale della psicoanalisi: Scritti 1942-1960* (pp.181-205) Milano, Feltrinelli).

Reisman, J. M. (1991). *A History of Clinical Psycholog.* New York: Hemisphere (trad. it. *Storia della psicologia clinica.* Milano, Cortina, 1999).

Stampa, P. (1990). "Malpractice": Una nota su alcune condizioni epistemologiche e conseguenze pratiche dell'errore in psicologia clinica ["Malpractice": notes on epistemological conditions and practical consequence of the error in clinical psychology]. In P. Colamonico, G. Montesarchio, & C. Saraceni, *Psicodiagnostica e psicoterapia: parliamo di errori* [Psycho diagnostic and psychotherapy: talking about errors] (147-159). Roma: SIRP.

Tallis, F. (1998). *Changing Minds. The History of Psychotherapy as an Answer to Human Suffering.* London: Cassel.

Vaillant, G.E. (2003). Mental Health. *American Journal of Psychiatry, 160*, 1272-1284.

Wacthel, P. L., & Messer, S. B. (1997). (Eds.). *Theories of Psychotherapy. Origins and Evolution*. Washington, DC: American Psychological Association.

Wakefield, J.C. (2004). Realtà e valori nel concetto di salute mentale: il disturbo come disfunzione dannosa [Reality and values in the mental health concept: disorder as unhealthy disfunction]. *Psicoterapia e Scienze Umane*, *4*, 439-464.

Wakefield, J.C. (2005). Il concetto di salute mentale: una critica a Vaillant [Mental health concept: criticizing Vaillant]. *Psicoterapia e Scienze Umane*, *1*, 91-96.

Zittel, C., & Westen, D. (2003). *The Countertransference Questionnaire.* Emory University. Unpublished manuscript.

Websites:

www.collegiopsiclinicauniv.it
www.geocities.com/centersophon/pc-sophon.html