

Back to the rough ground: From predictability to radical uncertainty

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In this article I will begin by thinking about how schools of psychotherapy come to be created, and argue that this is a political process rather than a 'scientific', technical or rational one. I describe ways in which practice can at times be unhelpfully rigidified into techniques which are used to build borders between schools. I follow this by a description of how Eliasian gossip is used to reinforce methodological silos.

Finally I consider group analysis' relationship to psychoanalysis, science and the medical model in order to try to shed light on the question 'one group analysis or many?'

Key words: politics of psychotherapy, science, psychoanalysis, group Analysis, research, technique

Of course therapists may be stimulated by [the innovators of schools] . . . but these should provoke him to thought, not to being a follower who identifies with their theories. (Heaton, 2013: 11)

Introduction

This paper was a key note at the 17th GASi Symposium in Berlin, 'Crossing Borders'. The theme of the day was 'One Group Analysis or Many?'

On my first humanistic psychotherapy training there was indecision as to whether it was better to be eclectic or integrative. Eclectic meant the ad hoc, cut-and-paste use of bits and pieces from a range of theories. To be 'integrative' was to have integrated these elements into a theoretical whole. In this realm border crossings between schools were the norm.

In contrast other trainings teach a single methodology. These tend to be more purist and take the crossing of borders into other methodologies to be a bad thing. Although, as Roth and Fonagy say, 'In everyday clinical practice, there is much that is "borrowed" from different orientations by all practitioners (Roth and Fonagy, 2005: 15)'. The idea of 'borrowing' implies that someone already has ownership over that which I borrow. Whether one is eclectic, integrative or purist, all take the pre-existence of the differing psychotherapy schools as unproblematic givens.

But where did they spring from? How did the borders and boundaries differentiating the schools come to be established in the first place? The story of the Neem tree is instructive.

The Neem tree

For over 2000 years components of the Neem tree have been used by farmers in India as pesticide. In 1992 an American agricultural company 'Grace' patented a version of the pesticide, and then claimed legal ownership of all such uses of the Neem tree. US patent law would only allow challenges to the legitimacy of the patent if it could be shown that prior knowledge had appeared *in a printed publication* before the patent application. This being folk knowledge, there was nothing in formal print. On this basis, the courts ruled that there was no scientific evidence and dismissed the actual evidence as anecdotal and non-scientific. After decades of legal battle, the patent was revoked.

There are two points I want to draw out from this. First, notice how a particularly narrow *definition* of evidence is being used to delegitimize and discount an obvious reality that is there for all to see, as though it did not exist. Patenting is not just a claim to knowledge; it is also a claim to be the author of, and *the* authority on that knowledge. Second, notice how the patenting process can be a way of colonizing something that had previously belonged to everyone. Versions of this sort of colonizing process have been going on since time immemorial.

Terra nullius

European adventurers stumbling across the 'new' worlds managed the extraordinary feat of perceiving lands full of people as empty, as 'No man's land', as *terra nullius*. Seeing it as empty, they claimed it

for themselves. The colonizer saw no contradiction in being able to appropriate indigenous knowledge from lands that were apparently empty of indigenous people. In this activity they conceived of themselves engaged in the activity of science; as bringing order to disorder. History itself was thought to begin with the colonizer stepping onto the so-called virgin land.

Psychologica nullius

Something of this dynamic has also been playing out in the field of psychology. Schools often claim to be the first to discover this or that, and being the first they take ownership of it.

Cognitive Behavioural Therapy (CBT) texts convey a picture in which before the advent of CBT the psychological arena was almost empty of all knowledge, and what there was, was unproven superstition. Anything not tested *according to specific decreed empirical protocols, simply did not exist*. The field was *psychologica nullius*.

The most effective and simplest way of doing away with prior history is by making no mention of it. CBT did allow for the prior existence of behaviourism, but little else. In this way CBT creates an 'Absolute Beginning', before which there was nothing.

For example according to the CBT pushers, Layard and Clark, it was only in 1978 that a British psychologist from the CBT camp,

George Brown . . . [who first] discovered that the family environment in which a person lives has a big influence on whether they relapse in the face of stress. (Layard and Clark 2013: 169)

It would seem that prior to 1978 no one had recognized the relevance of the family environment. But it is not just CBT that 'discovers' in this way. Phyllis Grosskurth, the biographer of Melanie Klein tells us that

[whenever] . . . a new idea occurred to [Klein] . . . she would immediately term [it] a 'discovery'. Winnicott called her a 'Eureka shrieker'. (Grosskurth, 1985: 121)

Another way of eradicating history is by giving concepts new names. Group analytic allusions to the matrix and social unconscious overwrite notions of discourse and ideology. They are spoken of as though they began with Foulkes or Fromm, and not with Marx, Althusser and Hegel. Politics is tamed and renamed as dynamics, after which it is incorporated into group analytic discourse.

Technical terms like ‘attachment’ or ‘splitting’ function as code words that serve as boundary markers and differentiators. Freudian texts primarily cite other Freudians and Rogerian texts mainly cite other Rogerians. In these ways we build and reinforce methodological silos, which we defend against all comers.

Creating schools of therapy: from method to methodology

All schools of therapy first started as an exploratory practice, which were latterly theorized. However, the theorizations had the unintended effect of closing things down in the following way.

For example, through practice, Carl Rogers thought certain core conditions to be beneficial to the therapeutic process. Meanwhile Freud thought that ‘the transference’ and its interpretation were central. Two things happened next. First, the forms of practice that had been found to be helpful became rigidified into rules and instructions called techniques. Second, when this knowledge is disseminated to others through psychotherapy trainings, a reversal takes place. Trainees are taught the theory first, and then a form of practice that fits with that theory. Not only is theory driven practice a kind of conformism, something else more troubling also takes place.

Each training insists that their trainees should interact in these ways, *and only these ways*. To deviate is to do wrong. In effect, trainings and institutes create faiths with disciples. The classical psychoanalyst must *only* interpret, the person centred counsellor must *never* interpret. In effect, what has happened is that a method has been generalized into a hegemonic methodology. The idea of analysis seems to belong to psychoanalysis. *Only* psychoanalysts analyse, and psychoanalysts *only* analyse. And if a Rogerian were to dare to analyse, they would be viewed as borrowing a psychoanalytic technique. In these ways techniques and values become colonized and take on the appearance of having always belonged to particular schools.

This is where the supervision process can come to serve a policing function, particularly during the training itself, when trainees are at their most vulnerable in relation to the power of the training authorities. Recently a CBT trainee was chastised by his CBT supervisor for being ‘too psychoanalytic’. Meanwhile I know of one or two trainees at the London IGA who were persecuted by their respective supervisors for not being psychoanalytic enough.

In my early days I myself reproved an IGA trainee for being too facilitative and not sufficiently analytic. At that time, by analytic I

meant detached and interpretative. In doing this I was reinforcing and legitimating a particular understanding of the analytic, and at the same time augmenting my own analytic credentials and authority.

I now find myself in a predicament. I discover that many of my ways of working apparently belong to other schools. For example, when questioning the rational basis of someone's paranoid beliefs, I could be said to be utilizing a CBT technique. At times, I am even able to be compassionate despite not having trained in compassion focussed therapy. Most recently, I discover that I have somehow been using some of the techniques of mentalization as well as those of mindfulness for some decades before they were invented. How is this possible?

Identity formation

Identity is process. And the naming or claiming of an identity is a form of process reduction (as Elias termed it). Identity is, amongst other things, constraint and closure. Identities utilize differences to generate differentiations. However similarities are also always present. I am different to you by virtue of a number of attributes and *at the same time* I am similar to you by virtue of a number of other attributes. Similarity is not the opposite of difference. Identities are constructed by emphasizing a particular difference and amplifying a specific similarity.

For these reasons there is the ever present danger of one identity morphing into another. This is where the border police come in. We delegate the official and formal tasks of policing the boundaries of our professional territory to our committees and supervisors. But there is also an informal policing work taking place through everyday interactions; it is we, it is you and I, that unbeknownst to ourselves, do the informal policing work through gossip. Norbert Elias (1976: xxv) describes the presence of gossip mills that work in subtle ways to keep groupings differentiated by continually demarcating the line between the 'us' and the 'them'. We all do it. Much is being tacitly said when over a cup of coffee, one colleague says of another with a certain look: 'he is very Kleinian'.

The gossip process is one of shame and humiliation and is integral to the processes of inclusion and exclusion. Foulkes has rightly said that belonging is essential for the possibility of psychological health. Shame is the motivator that encourages us to remain within the approved version of the 'us'. But of course there are disputes about which version should be the approved version.

For example, recently, a group analyst said in a group: ‘This has left me feeling worried. Am I the only one who feels like this?’ Later, her co-conductor was critical of her, saying that in revealing something about herself, she had ‘broken the analytic frame’. In speaking in this way the co-conductor determined what was allowable into the ‘analytic frame’. Similarly, some people said of my first book, ‘it is not very psychoanalytic’. Both statements are assertions about what is and is not within the approved version of the analytic, with the added implication that if it is not of ‘the approved kind’ then there is something wrong with it.

This is how various kinds of ‘us’ and ‘them’ groupings are generated within psychotherapy. In some instances, the borders between the ‘us’ and ‘them’ are so sharp that they cut anyone trying to cross them. In other instances the borders are more porous and forgiving.

Persons and things

Up to now I have been speaking about the politics of the divisions in our profession. I turn next to the substance of the divisions, and more directly to the theme of the day ‘one group analysis or many?’ It seems to me that there are three basic distinctions that group analysts are preoccupied by.

1. What is the relationship of group analysis to psychoanalysis?
2. Is the practice of group analysis a scientific activity?
3. What is the relationship of group analysis to the medical model?

Kant and Wittgenstein

I will build my argument on the distinction that was initiated by Kant and taken up by Wittgenstein (1953), this being the distinction between cause and reason. Kant said that the inanimate material world works as though it were a deterministic mechanism; to understand it one looks for causes. The relationships between various causes and their effects, once gathered and systematized, become the basis of empirical science. Scientific knowledge has predictive power.

But to understand humans, Kant said we look for reasons, not deterministic causes. We do not trouble ourselves to ask of a stone: ‘why did you fall?’ because the stone *is not complicit in its fate*. It is because humans are complicit in their fate that it feels so perfectly to natural to ask them: ‘Why? Why did you do this?’. We want to understand the *reasons* behind the actions. This is why, in Kant’s view,

humans cannot be understood by the methods of deterministic empirical science. Further, Kant asserted that any attempt to reduce human activity into the language of causes would be unethical, because it would reduce human beings into mechanisms, into things. In sum:

The inanimate reacts, whilst humans respond.

Freud disagrees with Kant saying that what looked to Kant like freely made reasoned conscious choices, were in fact driven by unconscious processes, which were themselves triggered by the chemical and instinctual substrate of the body. Freud saw himself as a scientist, and had ambitions for psychoanalysis to be a deterministic science. In one place Freud (1912) goes so far as to say: 'Anatomy is destiny'. In this sense (but not in other senses) Freud puts humans back onto the side of things. Sulloway wrote

Freud's entire life's work in science was characterized by an abiding faith in the notion that all vital phenomena, including psychical ones, are rigidly and lawfully determined by the law of cause and effect. (Sulloway, 1992: 94)

Imagining themselves to be akin to natural scientists, classical psychoanalysts take up the detached neutral stance of the natural scientist in relation to the patient. We know this as 'analytic reserve'. To get involved in conversation is taboo because it muddies the transference, it is a form of gratification, and so on. All this falls under the rule of abstinence.

As a medical doctor, Freud was immersed in the medical model. This is why we speak so readily of what we do as *treatment*. The prestige and prevalence of the medical model has meant that even today, in most countries on mainland Europe, the professions of psychoanalysis and group analysis are made up almost entirely of psychiatrists and psychologists. Oddly, this has taken place despite Freud (1926) urging the State and the profession to allow non-medical persons to train and practice as psychoanalysts¹. When all is said and done, the medical model places humans on the side of things. Whilst this makes a lot of sense when it comes to organic medicine, it makes little sense in relation to human suffering.

Group analysis: a peculiar science

If psychoanalysis and group analysis are sciences, then they are peculiar kinds of science, because unlike the natural sciences, they have little or no predictive power. Psychotherapy constructs its causal chains *retrospectively*. When working with the abuser John, we might

make sense of things by looking back and constructing propositions such as John became an abuser because he was abused by his father when he was a child. These sorts of construals are often convincing and meaningful; *their explanatory power is integral to the practice of psychotherapy*. However, these construals have no predictive power. We cannot predict on the basis of innumerable Johns that if Jack is abused, *he will* necessarily become an abuser. The point bears repeating.

Psychoanalysis and group analysis have retrospective explanatory power but no predictive power.

In this regard both Marx and Freud were wrong: History is not destiny; nor is anatomy.

Evidence-based therapies; evidence-based group analysis

This is where the evidence based therapies come in. They say that their therapeutic results are predictable and they have the evidence to prove it. This of course has made them very appealing to commissioners and regulators who favour them over the other therapies. It would appear that the writing is on the wall. You have to present your therapy in the language of positivist empirical science to be acceptable to the powers that be. It seems that you can't beat them; so you may as well join them. Given the prestige that positivist research enjoys, it is worth taking a closer look at what mainstream psychological research looks like today.

Back to the border

Borders, boundaries are a means of differentiation. If on one side of the border is acceptable empirical science, then everything on the other side is unacceptable. However, the unacceptable can be made acceptable by the simple device of moving the border.

As Ben Goldacre (2012) and others have shown, this is exactly what has happened in psychological and pharmacological research. Over the last few decades the borders have been consistently moved in directions that have lowered standards and favoured certain kinds of evidence over others; that which was previously problematic is now acceptable.

Today, psychology departments (along with the rest of the university) are factories that are obliged to churn out prodigious amounts research articles in order to put their departments and universities in good standing with the regulatory authorities and funders. To this end psychological research has been industrialized and proceeds along this sort of formulaic route.

You have to start with a specific discreet illness that you are going to treat. To this end you are obliged to choose one of the so-called mental disorders found in the DSM; otherwise you will not receive the funding, and your research would not be recognized by the regulatory bodies like NICE.

There are two points. First, in taking this step, you have immediately signed up to the medical model. In doing so, not only have *you accepted the medicalization of human suffering, you have pathologized the sufferer*. Second, despite the rhetoric claiming otherwise, the reliability of the diagnostic categories within the DSM are very shaky. Extensive studies have shown the regularity with which different clinicians diagnose the same patient as having very different psychiatric conditions (Moncrieff, 2011).

Next, you collect say 100 people that you have diagnosed as having this mental illness. You then discard most of them as being too complicated. You divide the 20 that are left into two groups of 10. One group is given treatment, and the other gets either nothing or a placebo. Some while later you measure the level of suffering to see how the treatment group has fared in relation to control group. What measurement actually consists of is the patient answering questions about how depressed they are by circling a number between one and five. From not depressed to very very very depressed. But the answer 'five' is no less subjective than the answer very very very depressed. It is by this kind of subterfuge that the subjective is made to appear objective.

Results of this kind are put through arithmetic manipulations to see whether the outcomes are 'statistically significant', $p < .05$ – (p less than .05). If they are, then you repeat the whole process. If the second result is also statistically significant, then you have reached the holy grail of empirical science; your results have been replicated. Your claims are now deemed to be objective scientific facts.

As soon as you have achieved this, you have the possibility of applying to get your treatment licensed by the health authorities. However, there is no limit to the number of studies you are allowed to do in your quest to find two results that are statistically significant.

And nor are you required to declare the studies in which the results were not statistically significant. (Kirsch, 2011)

Most importantly, although the results only apply to the specific characteristics of the 20 who were used in the study, the findings are dishonestly over-generalized, extended, and sold as though the treatment had been shown to be of benefit to the entire 100.

The treatment is now manualized because the protocols of positivist science require consistency and reliability. The expectation is that those following the manual will produce similar results. But manualization also functions as a patenting process. It is a way of colonizing psychological territory. It is for this reason that I find that some of my ways of working are suddenly no longer mine, because they have been written into someone else's manual.

There is one more point to be highlighted. In two studies spaced over almost 30 years, a researcher Sterling (1959, 1995) noticed that almost every so called scientific psychological study ever published miraculously claimed that their results were statistically significant. This, as Sterling says, is somewhat suspicious. People are increasingly starting to name this suspicious activity as *p*-hacking. It would seem that it is always possible to find a way of doing calculations behind the scenes to regularly arrive at this suspiciously significant result.

I personally am not against scientific research. I am not even against positivist research. All inquiry is research. But I am against the corrupt narrow industrialized research culture that prevails in mainstream psychology and psychiatry. A culture that discounts and dismisses other kinds of real evidence in an echo of the way that the patent laws were used to dismiss evidence of use of the Neem tree.

Old wine in *my* bottle

Having positioned group analysis in relation to psychoanalysis, science, and the medical model to some degree, what I want to do next is to say something about my form of practice. I will not be presenting my way of working as a new way of working, as that would be to hallucinate yet another *psychologica nullius*. I will not be trying to sell you new wine in new bottles. Rather, I will be describing the mix of old wines that happen to have collected in my bottle.

To begin with I want to say something about the terms patient and client. Broadly, humanistic therapists prefer to use the term 'client', because they say they are engaged in an activity freely chosen between

equals. Not only does this edit out the power-differentials in the consulting room, the term 'client' does not do justice to the work of depth, conflict and intimacy that therapeutic relationships entail.

Analysts meanwhile tend to use the term 'patient'. One meaning of 'patient', from its Latin roots, is that of suffering—the one who suffers. In this sense I am drawn to use this term. However, in psychoanalytic discourse generally, the use of patient is a way of creating a pathological Other. There is the one who suffers, and the one who is non-suffering or post-suffering. But I, the therapist, also suffer and continue to suffer. In this sense despite my different role, I remain a fellow patient. I hope to make clear shortly why this last statement and what follows from it is critical to the therapeutic process and not just sanctimonious sentimentality.

For these sorts of reasons I am drawn to use the much more unwieldy terms, the-ones-who-come-for-help and the one-who-is-paid-to-help (but-who-continues-to-need-help).

I will begin by telling you something about my form of practice. It is only then that I will theorize the practice. I want to proceed in this way in order to make transparent how my theorization is in part, a rationalization and post-hoc justification for what I do. I want to make this transparent, because I think this is how our profession actually accrues its knowledge. However, the danger in me setting up this kind of linear phenomenological narrative, of practice followed by a theorization of that practice, is to suggest that I think that the practice itself is theory-free. As I hope it will shortly become evident to you, I do not think this to be the case. All observation, all experience is necessarily theory laden.

Describing the indescribable in describable ways

The first thing I want to do is to remind us that all descriptions of the psyche are attempts at describing the indescribable in describable ways. There is no actual ego, nor projection. The concepts are metaphoric descriptions. Our actual language should be, 'it is "*as if*" such and such were happening'. We tend to forget this and think that such and such '*is*' happening.

This then is my attempt to describe a particular indescribable in describable ways. A lot of what I do as therapist is familiar and uncontroversial. What I mainly do is sense-making, which, amongst other things, I do by making links between past and present, between here and there. I take this activity to be one of meaning making rather

than truth finding. I do this by attending closely to what is going between me and the ones-who-come-for-help. Although this is not my preferred language, all these forms of interaction will be familiar to you as working with the transference, the activity of interpretation, and so on.

One reason that this language does not sit easily with me is because my form of interaction tends to be involved and engaged rather than detached and removed. I tend to be present in what I am saying; I tend to speak in the first person. I am inclined to inhabit my statements, thus emphasizing their partiality and their subjectivity. I am inclined to be transparent, in that I will often express what *I am* feeling and thinking in relation to what is arising in the consulting room. At times I have given voice to my opinion about things, and at other times I have made suggestions. I am a contributor and not just a listener and observer.

To the classically informed mind set, it will look like I have become arrogant, sloppy, lazy and dangerously self-indulgent. I will be construed as having violated a number of critical boundaries. So let me say a little more; *and what I will say constitutes the post hoc theorization and post hoc rationalization of my practice.*

Even though my way of interacting looks more like ordinary conversation, I am circumspect about what I reveal—making continual micro decisions regarding my responses, sometimes consciously, but most times subliminally. My stance might be described as ‘measured transparency’. I discover that the ways of working that I have drifted into have affinities with the schools of intersubjectivity and relational psychoanalysis. They both take the lived relationship in the consulting room to be central.

Classical psychoanalysts are likely to take umbrage at this construal, saying, but this is exactly what we do. We attend closely to the dynamics of relationship, the projections, transferences and so forth in order to help the patient face reality. This is true.

The difference between the two is this: the classical psychoanalyst attends to the relationship from outside it. They keep themselves removed and detached in the kinds of ways that I have previously described. This kind of analyst does not participate in a relationship; and even more: they view the patient’s attempts at getting the analyst to participate in a relationship as a regressive pathological manoeuvre that is to be resisted and interpreted.

Relationship in the classical psychoanalytic scenario is not only a one way street, it is a dead end. In contrast, in the relational paradigm

the one-who-is-paid-to-help is an involved participant who, with the others in the room, co-creates what takes place. Foulkes was well aware of this:

It is important for the therapist to admit that his personal influence is inevitably strong in spite of all his precautions to minimize this . . . [therefore he] should use it consciously rather than haphazardly or unconsciously. (Foulkes, 1986: 129)

To these ways of thinking, intersubjectivity is prior to subjectivity and the relationship is prior to that which relates. To my mind these propositions have echoes with the paradoxical ideas of 'Radical Foulkes', in which the social is prior to the individual, the very individual that constitutes the social.

The stance of the classical analyst towards the patient is that of an I-It relationship, in which the analyst teases out the unconscious causal determinants of conscious life. Meanwhile the stance of the relational therapist tends towards an I-Thou relationship, in the service of something I will call 'meeting'. (Buber, 1958)

In using these sorts of terms, I-Thou, meeting and so on, I might be thought sanctimonious and preaching from on high. My intention is the opposite. It is to get back to the ground. It is a wish to get away from hallucinated certainties and (in Wittgenstein's phrase) to '*get back to the rough ground*'².

Kant's injunction against the I-It has two components, a moral and a logical. The moral part says that to treat humans as mechanisms is to demean their humanity. The logical goes like this: The very fact that humans have the capacity to choose to some degree how to respond, their capacity for free-ish will, means that their responses will be unpredictable. For this reason alone they cannot be treated as predictable mechanisms, as 'Its'. In other words it is their very unpredictability that makes humans human.

The key terms here are those of *co-creation*, *emergence* and *unpredictability*. What we are faced with here is a kind of radical uncertainty, a profound uncertainty that will not be tamed by rationality or any other means. If all this be true, then when it comes to psychotherapy, the treatment paradigm is quite simply the wrong paradigm. Treatments require us to know where we are going, or at the very least know where we want to get to, before we even begin. But if I do not know where I am going, if I *cannot* know where I am going, then whatever it is I am doing as a psychotherapist cannot be construed of as a treatment. Not only am I unclear as to the solution, I mostly

remain unclear as to the nature of the problem. Having no clear start or end points, what should I do, how do I decide what to do?

Stumbling in the dark

Confronted as I am by radical uncertainty, I mostly stumble in the dark. All I can think to do is to try to find and meet with others. I am not sure as to what I mean by 'meeting', and so I am not sure how to convey it to you. What I can tell you is that sometimes an experience of meeting and being met arises between us. This momentary experience feels deeply meaningful, although I would be hard put to tell you what that meaning was.

But this sort of moment is a rare occurrence, because all parties, including me, are caught up in a whole host of defences, denials, fears and obfuscations. All of which gets in the way of meeting. This is why I have said 'trying to meet', because the 'trying' is in itself the therapy. This trying consists of working through the dross so that we might meet, which we can only do as vulnerable suffering beings.

Having no manual, having no bearings, I trust to my intuitions. This is what we all mostly do and is not particularly mysterious. My intuitions are the amalgam of my sense of things in the moment, which are formed by my history and includes my thoughts, beliefs, feelings, principles, values and experiences. There is no objectivity here.

Even Freud, who despite his scientific aspirations had the humility to say:

If everything depends on my interpretation who can guarantee that I interpret right? So after all, everything is left to my caprice. (Freud, 1926: 219)

Mired in subjectivity and faced with radical uncertainty, my intuition moves me to respond this way rather than that. Despite radical uncertainty, I nevertheless commit myself to it. In this darkness the feeling of sincerity is my guide as to what to do or not do, to say or not say. But sincerity is no guarantor of anything; it is full of risk. Misunderstandings, hurts and conflicts are part and parcel of what takes place in the dark. In these moments, caught up in difficulty and doubt, I look to faith to sustain me. However, many times it turns out that this faith was misplaced. Faith, intuition and sincerity are fallible and unreliable compasses, however, they are all I have and so I have to make do with them. It is for this reason that I think that the work of

psychotherapy is an ethical work, a work of trying-to-be-with rather than of doing-to, a work of healing rather than of curing.

The activity of psychotherapy might well be unpredictable, but as we know, the ones who come for help are often stuck in fixed repetitive patterns, reproducing the same dynamics over and over again from situation to situation. In Freud's pithy phrase, they are endlessly repeating without remembering. This then gives us another way of conceptualizing the psychotherapy process, which is, to help the one's-who-come-for-help transform from *predicable reactive beings into unpredictable responsive beings*.

So I too have a goal; I too have ends in mind—the transformation of reactive beings into responsive beings. So how is my way of thinking different from a treatment paradigm?

My not very satisfactory way of squaring this circle is as follows. First, although I have notional ends in mind, I have no idea what they are or how to get there or to even recognize them as a destination if we were ever to get there. These ends have something to do with feeling more fulfilled, of having more autonomy, of being less fearful, of life being more meaningful. But these sorts of ends are not present in my mind when I am immersed in the micro moment by moment interactions that constitute that journey. If I had these ends in mind, then my actions would no longer be sincere, because they would have become instrumentalized. I would be doing something in the service of achieving a particular end.

In her response to Richard Billow's article (2017), Miriam Berger (2017) uses the term 'experience *near*' to capture the attitude of the relational therapist. I go further, towards 'experience *in*', with all the attendant difficulties that it will necessarily give rise to.

Whilst this is an experience of immersion, it is not an experience of merging. There remains an 'I' that is 'in' the experience, that 'has' the experience. This 'I' however, is not encapsulated and impregnable; I am moved by and through the experience. Transformed by the experience itself, I become something other than what I was a moment before. As I have discussed previously (Dalal, 2014) this speaks to the tension encapsulated by Foulkes' two terms—group analyst and group conductor, and to the tension between involvement and detachment as spoken of by Norbert Elias (1956). This then serves as a corrective and reminder that there are degrees of involvement and detachment; they are not opposites. The same is true of the dichotomy I-Thou and I-It. In a sense all these terms are analytic abstractions that are instances of process reduction. Psychoanalytic positivists however

imagine that it is possible to firmly locate themselves on the I-It pole which grants them objective clarity, a clarity that is to be protected from being contaminated by experiences of I-Thou.

Theories, and even reflection itself, are ways of trying to describe the indescribable in describable ways. We cannot do otherwise because language is fundamental to thought itself. As necessary as this is, I think that what we forget is this: however captivating our descriptions, that which we are trying describe *remains* indescribable; something always escapes the description. However grand our theories, I think that the reasons our work works, when it works, remains a deep mystery.

Conclusion

Depending on the taxonomy one uses, there will be many different answers to the question: 'one group analysis or many?'. By using the distinction between cause and reason, and focussing on the stance of the practitioner, I have come up with the answer: two kinds of group analysis.

The first is the I-It kind which is grounded in the idea of treatment, which requires detached and interpretative; the second kind is the I-Thou kind grounded in the idea of meeting, which requires the therapist being involved and responsive.

I have construed of the work of psychotherapy as the transformation of reactive beings into responsive beings. I think the responsivity of the therapist, the-one-who-tries-to-help, is necessary and critical to this task as it is the means by which the responsivity of the ones-who-come-for-help is brought alive.

I have suggested that the essence of our work is mystery, a mystery that does not collapse into mysticism; a mystery that does not pander to mystification. I think that there is an anomaly at the heart of the group analytic profession which is this: even though most colleagues are likely to think of suffering as meaningful, and the activity of psychotherapy as meaning making, their form of practice continues to replicate the distanced stance of scientist delivering treatment. When I say this sort of thing, I come across two sorts of reactions. The first is bemused agreement. 'What's the big deal? Of course we are detached; the analyst needs to be detached to do the work of analysis'. For them the detached stance is both unproblematic and technically appropriate.

The second reaction is one whereby I am told that this is a straw man; that psychoanalysis and group analysis have moved on since

those early days. But my continuing experiences at conferences, supervisions and in conversations, is one in which many large and small group conductors keep themselves above and outside the group, delivering occasional commentary on the group process. Several participants on recent (2016/2017) introductory courses to group analysis have described their group conductors as little different from the way a classical psychoanalyst would interact. Their group conductors were primarily silent, inscrutable and detached.

The way questions from group members are dealt with by conductors tends to be very revealing. In the main they are not dealt with at all; questions are habitually met either with silence or with an interpretation. In which case the response is non-dialogical.

I do not claim this to be universally true. I know of many colleagues who are not like this, but I come across the detached and authoritarian stance sufficiently often to claim this to be a prevailing norm—certainly in the UK, and I have come to understand from colleagues, also elsewhere.

Of course all these bits of evidence I put before you, can be dismissed as anecdotal and not real, ‘scientific’ evidence at all; recall the Neem tree. The main point I want to make is this: Group analysts that practice in this way, sit comfortably within the territory called psychoanalysis, if psychoanalysis is taken to mean the classical stance that I have just described.

In ending my article with a description of my kind of therapy, there is the danger that I might be heard as saying that there are two kinds of therapy: my kind, and the wrong kind. I do not think this, even though I have strong disagreements with some of the other ‘kinds’. I actually believe that the real answer to the question ‘one group analysis or many?’ is that there are as many different kinds of group analyses as there are group analysts.

But this unruly conflictual diversity, is anathema to the ‘managerialist’ bureaucratic mind-set that is increasingly coming to prevail in our institutes and trainings. Bureaucrats like order; they like standards and standardization, and so they think uniformity and conformity to be a good thing. Their defensive risk-averse mind set fits well with that of the puritans and fundamentalists within our profession who would seek to impose their authoritarian certainties upon us.


This toxic mix of fear mongering and moral superiority can paralyse us into compliance. Let us then hold fast against the positivists, the bureaucrats and the fundamentalists, who, intoxicated by their certainties and sense of moral virtue, would suffocate all innovation

and creativity under the banners of purity and predictability. They would insist that there is only ‘one group analysis’, *their* group analysis.

Notes

1. Freud was prompted to write ‘The question of lay analysis’ in response to the legal proceedings that were instigated in 1926 against a prominent non-medical member of the Vienna Psychoanalytic Society, Theodor Reik, for breaching the Austrian law against ‘quackery’. According to this law only those with medical degrees were allowed to treat patients. Some version of this situation continues even today in Austria, Germany, and no doubt also elsewhere.
2. ‘We want to walk; so we need *friction*. Back to the rough ground!’ Wittgenstein para 107, *Philosophical Investigations* 1953.

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References

- Berger M (2017) A Response to Richard Billow’s ‘Relational Group Psychotherapy: An Overview. *Group Analysis* 50(1): 23–9.
- Billow RM (2017) Relational Group Psychotherapy: An Overview. *Group Analysis* 50(1): 6–22.
- Buber M (1958) *I and Thou*. London: Continuum International Publishing.
- Dalal F (2014) A Ruminant on Intimacy and its Defences in the Consulting Room. *Group Analysis* 47(4): 392–407.
- Elias N (1956) Problems of Involvement and Detachment. *British Journal of Sociology* 7(3): 226–252.
- Elias N (1976) Introduction. In: Elias N and Scotson J *The Established and the Outsiders* (1994) London: Sage.
- Foulkes SH (1986) *Group Analytic Psychotherapy, Methods and Principles*. London: Karnac.
- Freud S (1912) On the Universal Tendency to Debasement in the Sphere of Love. In: *Standard Edition* vol XI. London: Hogarth.
- Freud S (1926) The Question of Lay Analysis. In: *Standard Edition* vol XX. London: Hogarth.
- Goldacre B (2012) *Bad Pharma: How Drug Companies Mislead Doctors and Harm Patients*. London: Fourth Estate.
- Grosskurth P (1985), *Melanie Klein: Her World and her Work*. London: Hodder and Stoughton.
- Heaton J (2013) *The Talking Cure: Wittgenstein on Language as Bewitchment and Clarity*. Hampshire: Palgrave Macmillan.
- Kirsch I (2011) Antidepressants and the Placebo Response. In: Rapley M, Moncrieff J and Dillon J *De-Medicalizing Misery*. London: Palgrave Macmillan, pp. 189–96.

- Layard R and Clark DM (2013) *Thrive: The Power of Evidence-Based Psychotherapies*. London: Penguin.
- Moncrieff J (2011) *Demedicalizing Misery*. London: Palgrave Macmillan.
- Roth A and Fonagy P (2005) *What Works For Whom? A Critical Review of Psychotherapy Research*. 2nd Edition. New York: The Guilford Press.
- Sterling T (1959) Publication decisions and their possible effects on inferences drawn from tests of significance—or vice versa. *American Statistical Association Journal* 54: 30–34.
- Sterling T, Rosenbaum W and Weinkam J (1995) Publication decisions revisited—the effect of statistical tests on the decision to publish and vice versa. *American Statistician* 49: 108–112.
- Sulloway FJ (1992) *Freud Biologist of the Mind: Beyond the Psychoanalytic Legend*. Cambridge, Mass: Harvard University Press.
- Wittgenstein L (1953) *Philosophical Investigations* trans G.E.M. Anscombe London: Macmillan.

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