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Hollway, W. (2008) 'Doing intellectual disagreement differently?' commentary on Frosh, S. and Baraitser, L. 'Psychoanalysis and Psychosocial Studies'. *Psychoanalysis, culture and Society* 13(4) special issue on British Psycho(-)social Studies. pp385-96.

In writing this comment on Stephen Frosh and Lisa Baraitser's article on psychoanalysis and psychosocial studies I shall take a different path from usual. I shall use it, and how I experience my position within it, to try to reflect on how to conduct intellectual disagreements with colleagues (who in this case are friends too) in a way informed by psychosocial principles.

I hesitated a long while before taking this as my main theme. Would it seem irrelevant to all those readers who don't know the protagonists or the history of our debates and collaborations within British psychosocial studies? Would it involve talking personally where it is inappropriate to do so? Wouldn't I rather use my word allocation in a refutation of the 'charges'? I decided at one point to replace my reply with an example of how I am working at the moment; my most recent psychosocial case study illustrating aspects of the identity transition involved in becoming a mother for the first time. But I decided that that could appear somewhere later, perhaps in the pages of this journal. I kept coming back to the idea that we – psychosocial academics – champion reflexivity but do not reflect publicly on how intellectual disagreements are conducted and that this would be an opportunity to do so. Would not psychosocial principles help to disentangle those difficulties when critique is experienced personally? I hope that those whose first reaction to this theme is that these 'personal' matters should be kept away from public debate will stay with this reply and discover that it has relevance for how we academics disagree with each other over intellectual matters in public.

But why, you might be asking, is Wendy Hollway taking upon herself these criticisms of a Kleinian approach to psychosocial studies? I will not repeat the examples that Tony Jefferson has neatly summarised in his reply, but I find it wounding to be the chief target of accusations about the uncritical use of 'top-down, expert-knowledge epistemological strategies of psychoanalysis, with their apparent certainties about the true nature of human subjectivity, accompanied by an interpretive practice that seems always to know best, or at least to know subjects better than they know themselves' (p2). As well as taking this personally, I take it on behalf of a wider body of emerging empirical psychosocial analysis that I appear to be representing. Yet there are some grains of truth in this caricature that I want to associate myself with; positions that are not deserving of the wholesale dismissal which, despite the claim of 'sympathetic', is conveyed in the ungenerous tone of this critique.

I am one of those who believe that psychoanalysis is key to a psychosocial approach because I have found no other body of theory that illuminates experience, action and subjectivity as it does to enrich otherwise reductively social accounts. To conflate this with a proselytising vision (p3) is one example of the aggressive tone of this article that spoils its interesting themes. Other people can draw on my central use psychoanalysis as they see

fit: there is evidence that it has been productive both theoretically and methodologically. Tony Jefferson and I (2000) have argued that subjects (including researchers) do not necessarily know themselves (as Frosh and Baraitser refer to in the above quotation). It would be impossible to take psychoanalysis (of any persuasion) seriously and not conclude that this can be so. A central strand of my (and our) work has been to follow this through into an epistemology and methodology for empirical research. This point is not separate, as implied (p2), from an interest in how psychoanalysis can provide an account of internality and agency (Hollway and Jefferson 2006).

If Frosh and Baraitser were just names of people that I had never met, I might feel uninhibited to hate them for it and attack them back, but Stephen and I have been collaborating on this intellectual and professional terrain for thirty years, with shared as well as different interests and approaches (the former invisible in this article). He has supported my work (with publishers and funders for example) on several occasions. Lisa is a more recent acquaintance but I was delighted to read her PhD work on the experience of mothering that led into subsequent publications and the forthcoming book, which I reviewed for the publisher. It was refreshingly different from my approach, which meant that I could get something new from it and was pleased to be able to cite it. So I continue to like them and anticipate positive collaborations with them in future. Their paper does not feel like a representative expression of their relation to the psychosocial nor to my work. I think that both of them would be shocked to think that their paper was experienced by me as an attack and this points to a set of academic conventions that normalise certain structures and tones of disagreement that I want to examine. It is not intellectual disagreement itself that should be challenged.

It is not easy to separate out emotional responses from reasoned disagreement. On first reading this article, I was upset, angry and defensive. I experienced the piece as targeting my work and had to check this initial experience carefully over some considerable time: it was not clearly stated and I wondered if I was being paranoid. Being a convinced user of Bion's ideas, I did two things: I lived with these feelings for a while, believing that it is better (yes, normativity here) to try to process my experience of the article than evacuating the bad feelings, for example by writing a hasty attacking reply, which would amplify the debate's paranoid-schizoid tendencies and tend to have effects on a larger entity, the psychosocial studies network so recently established in the UK. Time helps, but so do other people. For Bion, thinking well is an intersubjective achievement and helps especially when one feels under attack (Hinshelwood 1992 p). I am a member of a well-established psychosocial group in the Faculty of Social Sciences at the Open University and this group kindly agreed to an extra meeting, where we had a helpful, containing and creative discussion.

A few colleagues, who were not able to attend, emailed me their impressions:

'I read this paper and it made me feel anxious. I hoped I was never on the end of a review by either of the authors. I also had a strong feeling

that there was an opportunistic aspect to the paper that wasn't honestly addressed in the review of your work ... other works are ignored that Stephen Frosh is well aware of. ... I thought they made some good points about transference and countertransference as they are both difficult to work through. I realised that although I use my emotional responses to help me understand my subject as well as my identifications I am always worried about theorising them as transferences. ... For me psycho-social interpretations are useful at helping us to understand experiences. They illustrate the limitations of a single approach such as discursive or social construction etc, they don't fix and they add depth and that's not the same as claiming priority for the psychoanalytic approach. It is not an either/or debate.'

'The whole notion of "the psychosocial" is insufficiently specified ... It does seem to be a paper that is primarily concerned with critiquing Hollway and Jefferson's approach, rather than with engaging with the broader terrain of the psychosocial. I can think of many people whose work I would include within this remit who are not even mentioned here – Valerie Walkerdine, Helen Lucey, Ann Phoenix, Peter Redman, Paul Stenner, Gail Lewis, Erica Burman – to name but a few. ... I am slightly disturbed by the way that the paper relies on the unacknowledged 'authority' of psychoanalysis. I think much of the paper contributes to a view of psychoanalysis as being fundamentally about clinical practice, with the suggestions that psychoanalysis is best understood as something you can 'be'. ... Later in the paper they make a distinction between the therapeutic and analytic elements of psychoanalysis which ... gives space to work and think psychoanalytically without 'being' a psychoanalyst. (Having said that I do share some of their concerns about what happens when we use psychoanalytic technologies outside all the carefully founded and very specific confines of the analyst-analysand relationship – particularly the ethical dimensions of that and the difficulty of challenging yours and Tony's interpretative authority, partly because it's based in a discourse of affective responses).'

'I wondered about their characterisation of psychosocial research in the UK – the focus on your work and the position they afford you is flattering but rather lonely! The psychosocial field is broader and more thoughtful than their characterisation implies (Sasha Roseneil's case study of Angel springs to mind immediately as a recent worked through empirical psychosocial example¹). I was not aware of the stand-off between Lacanian and Kleinian thinkers ... but if it is there, the focus on one practitioner fuels it. I really enjoyed the section on Lacanian psychoanalysis and the psychosocial ... it struck me that a similarly thorough and thoughtful reading of Kleinian thought was needed. ... I thought the section on reflexivity was the main site where discussion

¹ Roseneil, S. (2006) 'The ambivalence of Angel's "arrangement": a psychosocial lens on the contemporary condition of personal life'. *The Sociological Review*, 54, 846-868.

would be useful. F and B mention intersubjectivity at the start of the paper but then revert to reflexivity. This seems to allow a rather shallow sketch of fieldwork dynamics and does not do justice to how you catch them and how you use them. ... This is not about using one's own feelings as a way of 'reading' another person in the total way implied but as a way of getting glimpses and insights into the other and importantly working out who the research subject is for the researcher. Helen Lucey has a nice worked out example in the methodology chapter of 'Growing up Girl'². ... They equate qualitative research with interviewing ... Although there are excellent examples of research dynamics in interview text ... the Becoming a mother research has used recorded interview, observation and fieldnote data. This produces data on women's lives (their homes, babies and other family members etc) which go beyond texts.'

I cite these at length here, because, as well as making some valuable substantive arguments, together they illustrate the value of the group in going beyond the strengths and weaknesses that are often held – and defended – in individual positions. In doing so, these comments suggest productive ways forward that are not mired in critique and counter-critique – in negativity. Among other things, my colleagues helped me to hold on to what was valuable in Frosh and Baraitser's article when I was wont to see only the features that I disliked and wanted to criticise. In Kleinian terms, the group helped me to move beyond turning the article into a wholly bad object, entailing reciprocally locating all the 'good' in my own position and thus reproducing an unhelpful dichotomy between a 'Lacanian' and a 'Kleinian' position in psychosocial studies. Without exception, the responses from my colleagues (illustrated in those I cite here) were of the kind that succeeded in holding good and bad in the same object. They helped me to move towards an ambivalent position instead: in Kleinian terms, a depressive position. If this is pious, I own up to piety.

I concluded that my paranoid feelings that I was being singled out for attack were not devoid of real substance; that I did come across as the main target. True that the article chooses to restrict its arguments to the use of psychoanalysis³ but even if the focus was strictly limited to those using Kleinian, post-Kleinian and object relations perspectives in empirical psychosocial research, many were omitted. Why is this? Interpreting it symptomatically, I wondered if my work wasn't covering for others. On the surface, the treatment was polite and the claims were of even-handedness. Yet neither of these impressions lasted as the thrust of the article became narrower, to produce what I regard as a caricature of the work that I, among others, represent. In the process, like was not compared with like (Lacanian psychoanalysis is rarely if ever used in empirical social science) and the

² Walkerdine, V, H.Lucey and J.Melody (

³ I noted the disagreement about whether psychoanalysis should have a role in psychosocial studies in my report of the founding conference of the UK psychosocial studies network in December 2007 (PCS xx).

ostensible aims of the article appeared to be hijacked as it became an unbalanced celebration of negative critique and attack on the positive development and use of psychoanalytic concepts outside their clinical frame.

There is a role for rigorous critique obviously: that is the good bit in their paper that I am trying to retain. It casts its light (not for the first time) on some weak spots that should be exposed to debate. Examples are the danger of wild analysis, the need to be sensitive to power relations, the unsophisticated use of psychoanalytic concepts such as transference, the need for reflexivity, the continuing challenge of how to talk about inner and outer without falling into a static dualism. However, readers who do not know the field could be forgiven for thinking that the tradition that is being criticised here is ignorant of these challenges, does not address them. This is far from the case.

Their one-sided critique is illustrative of a bigger problem: so many oversimplifications are wrapped up and packaged in these two ostensibly opposing traditions that the effect is polarising and the process resembles splitting: Lacanian/good – Kleinian/bad/equivalent to psychoanalytic certainty. Examples are taken that are easy to shoot down because they are not contextualised or historicised. The use of the concept of countertransference by Tony Jefferson and me was of this kind. We were involved in methodological innovations and ten years ago were writing a ‘how to’ book that would be accessible to students (in which it has proved successful). Frosh and Baraitser’s commentary recognise the value of these innovations (although I could feel the ‘but’ coming on, as it did: ‘However, it must be said’!).

Following this and other critique (Stopford 2004, Hoggett 200?), I hesitate about using the concept of countertransference in a data analytic context. There is a big debate here about power relations and the specificity of the clinical psychoanalytic context that has still some way to go. It is worth pointing out that fifty years ago Paula Heimann (195x) brought the idea of countertransference out of the consulting room and into everyday world of unconscious dynamics among people. In practice, relationships with interviewees as they develop are much more complex than Frosh and Baraitser’s argument allows (p28) and transference and countertransference dynamics are involved. I think it is probably more useful to characterise them as the mutual, infinitely recurring workings of unconscious intersubjective dynamics in research, as in other, encounters. How these are then used to get a sense of the participants in a piece of empirical research is then a question for a research ethics that must take account of unconscious intersubjectivity and not be based on assumptions of unitary rational self-transparent subjects. I do not feel guilty of mining psychoanalysis as a technology (p29) because the innovations that I have made in research practice based on psychoanalysis have been carefully inserted into a psychoanalytic ontology and epistemology. Lacking training as a psychoanalyst there are bound to be limitations to my understanding, but since I am not working clinically, the ‘dangers’ (which I think are often overstated) need to be worked out on the terrain of a research ethics and in

the context of questions about the validity of research interpretation. This is the work that I continue doing, as Stephen well knows.

The thing that I love about psychoanalysis, the thing that keeps me motivated as a researcher, is that it is so deep and complex that I keep on learning from it and keep on changing my approach. I think that good psychosocial practice in academic critique would be not to caricature opposing positions (or at least to explain why such examples were being chosen from a range). Good practice would also involve acknowledging the trajectory of ideas and what they represented at the time. I want Stephen and Lisa to do my work justice, or more broadly the genre of work that they are subjecting – partially – to critique, by recognising what it achieves as well as what it fails to achieve. Especially I want them to acknowledge the fact that it constantly reinvents itself, tries out new methods, new concepts, pursue new epistemological directions. This is risky. Riskier, I feel, than standing on the sidelines espousing the ‘pure’ negative critique.

I was surprised at the claim that the Kleinians had ‘won’ (not least because the terminology connotes a battle). But supposing this disparate set of positions (it is not a singular position) has recently achieved some recognition in British sociology and social policy, perhaps it is because it has something to say about contemporary social issues (for example, Froggett, Hoggett, Rustin, Ryan⁴). In contrast the wider debate around postmodernism – including in the Humanities – suggests that theoretical critique as a single strategy for knowledge production has exhausted its potential. Do Lacanians really think that one can live in the perfectly Saussurean moment, even if one dissociates the rest of life from intellectual work? The authors say that the critical always takes the form of negation, which is, I suppose, logically correct. But it needs to be allied with the positive, if it is not to implode in infinite regress. For example, if you are rigorous about the principle of suspicion, must you not be suspicious of suspicion itself and apply this suspicion, say, to the suspicion of normativity or humanism (labels that are routinely used as unthinking put-downs and signifiers of a more sophisticated, critical position).

Frosh and Baraitser here advance a wholly deconstructive, negative and critical agenda for psychosocial studies (p8). My goal, by contrast, would be to hold the negative and positive moments of psychoanalysis in tension when applying it to psychosocial research. This means both knowing and not knowing, interpretation and deconstruction, confidence about an emerging data analytic picture and recognition of its provisionality. Peter Redman, in our Open University group discussion, helped me by making this point in a clarifying way. He drew on Laplanche’s notion of unbinding and binding to give this insight more depth. Unbinding is essential to the psychoanalytic process and to creativity but if that is all there is, that way lies madness. At the

⁴ Froggett, L. (2002) Love, hate and welfare: Psychosocial approaches to policy and practice.’ Bristol: Policy Press; Hoggett, P., Beedell, P., Jimenez, L., Mayo, M. and Miller, C. (2006) ‘Identity, life history and commitment to welfare’ *Journal of Social Policy* 35, 4; Rustin, M.J. (1991) *The Good Society and the Inner World*. London: Verso; Ryan, J. () ‘Class is in you’

end of a psychoanalytic treatment, rebinding is necessary, but if that was all there was, we are stuck. These concepts can be applied to moment-by-moment change as well. Post-Kleinians, starting with Winnicott, have moved Klein's more fixed notion of paranoid-schizoid and depressive positions precisely to capture this with an emphasis on constant oscillation between the two. There need not be these false dichotomies drawn up; let us think more dialectically.

The way we produce knowledge – like the way that Stephen and Lisa have produced this paper – is constrained by many material and structural factors, so that it is never as comprehensive as we would like it to be. I was reflexively monitoring my own response as I reread part of one colleague's email that said 'I'm slightly irritated that their discussion of reflexivity does not engage with all the rich work from feminists and post-colonial scholars who have compellingly 'unpacked' the kinds of reflexivity that became a ritualistic, well-rehearsed list of social locations'. I noticed feeling twinges of discomfort that I took to be guilt for just the same omission when on occasion I am seeking to make a point about how a psychoanalytic understanding can complement socially reductive accounts. We end up using shorthand, we overgeneralise, we are pressed for space and time, we cut and paste arguments from other writings. All this means that we are in danger of caricaturing. Perhaps it is along these lines that I can defend Frosh and Baraitser's use of our deployment of the concept of countertransference in the context of the dynamics of an interview and research interpretation (p27): that they were using it to illustrate a general claim and it was not intended as a criticism of individuals' work. It is often hard to know how to evidence a claim without alighting on some example, belonging to some writer. Moreover, it is part of academic convention to proceed like that. I agree. I have done it myself. But I am trying to reform my own practice.

In my view, it is part of a phallogocentric tradition that is not only aggressive and potentially damaging of collegial relations, but it is damaging to the process of knowledge production over time (part of power-knowledge-practice relations, in Foucault's terms). Caricaturing is part of splitting positions and pitting them against each other; it exemplifies a paranoid-schizoid mode of organising experience. This does not reduce the phenomenon to psychology, rather it contributes to a psycho-social explanation of a phenomenon that is institutionalised and would only remain so if it were reproduced through the practices of (in this case) academic writers. In the case of Stephen Frosh and Lisa Baraitser's paper, their splitting of Kleinian and Lacanian uses of psychoanalysis in British psychosocial studies contributes to a wider social division rather than modifying it. In this sense their claim to be examining the debates is disingenuous. They are making a powerful intervention (while assuming a position that power lies with others), one that affects the discourses and practices available for other academics to identify with and reproduce. In this sense it 'hails' the paranoid-schizoid rather than the depressive position in readers and makes it harder to find and pursue common interests.

My interpretation⁵ of splitting in Frosh and Baraitser's article does not preclude – indeed it affords – an exploration of the institutional (social or societal) conditions that increasingly compromise adequate reflection and research, for example, the intensification of academic production that creates a serious lack of time and makes it impossible to read, remember and cite all the things that could be relevant and fair. But institutional constraints afford opportunities for desires and anxieties to leave their traces. In the absence of sufficient time to consider the whole relevant field, what and whose work become salient? If my work comes to mind, which bits pop up as potential examples and in the service of what position – liked or disliked? What purposes, perhaps not fully conscious, do they serve? For example, at the inaugural conference of the UK psychosocial network in December 2007⁶, I shared a platform with Stephen Frosh (Lisa Baraitser was in the audience) and reflexivity was the main theme of what I said. I have been developing this as a way of analysing the researcher's implication in knowledge production for years. Yet reflexivity is represented in the current article as belonging to a Lacanian critique and sadly missing from Kleinian traditions⁷. A writer's affects leave their traces on the content and structure of an argument. Logic is never free of desire. The wider social constraints neither exclude these dynamics nor dictate their content. This is what a psychosocial analysis points to.

I remember being taught as an undergraduate psychologist that academic writing should be completely separated from the writer (the principle is encapsulated in the rule that we should write avoiding 'I'). After thirty years of feminism and other approaches that bring reflexivity and intersubjectivity to the fore of understanding knowledge production, we have dismantled this principle but have not explored the consequences for writing and intellectual disagreement. This is just a beginning.

⁵ I think that this is more of a hermeneutic interpretation – a standard move in argumentation – rather than a pseudo-clinical one. Frosh and Emerson (..) have made such distinctions about the nature of interpretation.

⁶

⁷ See for example Paul Hoggett 'combining dialogical and psychosocial methods of research' ????. While he does not use the concept of reflexivity, he shows how he is being reflexive in empirical research practice. Like me, he might not welcome being bundled into a camp labelled 'Kleinian' although, like me, this is a part of the tradition on which he draws.