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Symptom or Sinthome?

A critical review of *Burnout and intersubjectivity: A psychoanalytical study from a Lacanian perspective*

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Positivism casts a long and impregnable shadow over the way qualitative researchers have designed, conducted, and presented their studies (Altheide, 1987: 66-69; Altheide & Johnson, 1994: 485; Corban & Strauss, 1990; Denzin & Lincoln, 1994: 4-6), and the way in which this research has been evaluated by others (Ambert et al., 1995; Kiser, 1997: 151; Mays & Pope, 1995: 109; and Van den Hoonaard, 2001). For the sake of simplification, I will hereafter define qualitative researchers as those subjects who have employed a bricolage of research strategies (Denzin & Lincoln, 1994: 2) aimed toward an "ongoing critique of the politics and methods of positivism" (Denzin & Lincoln, 1994: 4). It thus becomes difficult to assess the integrity of the proclamation that qualitative methodology has

ruptured the fabric of traditional research methods, obtained its status as “a field of inquiry in its own right” (Denzin & Lincoln, 1994: 1), and signaled a “methodological revolution” (Denzin & Lincoln, 1994: ix). It would be more accurate to make the following two arguments: qualitative research has been the result of successive reformations of a single overarching paradigm,¹ and/or; today, methodological revolutions are paradoxical, they take place through selective conformity with the dominant paradigm rather than through waves of historical reformations.

David L. Altheide (1987) provided us with a nice example of how document analysis in qualitative research has been applied to combat the hegemony of positivistic methods. Ethnographic and Quantitative Content Analysis (ECA/QCA) were juxtaposed to demonstrate significant differences in process and to highlight the rupture ECA produced in the originating positivist framework. QCA came into existence through sympathies with the objective standpoint of social research, researchers typically produced designs that called for the examination of the frequency and variety of messages within textual documents and this task was often delegated to quickly trained researchers whom employed researcher-developed protocols (Altheide, 1987: 67-8). An overarching emphasis on the verification of hypothesized relationships motivated this type of research strategy – conventionally, the procedure was to move unidirectionally from the construction of categories to “sampling, data collection, data analysis and [finally] interpretation” (Altheide, 1987: 66-7). The role of the researcher was therefore restricted to four stages of the overall research process: the preliminary research design; theoretical and protocol development; data analysis, and; the final interpretation and discussion of the findings. By way of contrast, the presumption of ethnographic content analysis was that meaning occurred retroactively and that the researchers task was to move iteratively through all phases of the research process such that a strong convergence between the data and the conclusions become possible. The emphasis was on the discovery of themes, categories, and, finally, the core category rather than on the verification of a pre-established viewpoint. As Corbin & Strauss have put it: “In grounded theory, the analysis begins as soon as the first bit of data is collected. [...] This is why the research method is one of discovery and one which grounds a theory in reality (Corbin & Strauss, 1967: 6).

Grounded theory provides qualitative researchers with a paradoxical form of methodological emancipation: while the emphasis lies exclusively on the domain of what I will soon identify as the Lacanian *sinthome* (the iterative and retroactive process of social research) it does so according to the dogmas of grounded *theory*.² This theoretical dogma aligns itself quite nicely with Lacanian dogma: the relationship between the analyst and the analysand is typically negotiated, iteratively interpreted and retroactively inscribed with meaning (this is why it is often acknowledged that the symptom occurs logically in the future); finally, the Lacanian emphasis on *sinthome* strikes a chord with the attempt in grounded theory to identify with the “reality” or, if you like, contingency of meaning, found (with)in the empirical world. Moreover, the oft-repeated role of the Lacanian

psychoanalyst is to reject the position of the *sujet suppose savoir* (subject-supposed-to-know): in other words, the analyst, like the grounded theorist, must ward off *a priori* assumptions and find the symptom by first identifying with the *sinthome*.³ Grounded theory therefore signals a paradigm shift of sorts: on the one hand, grounded theorists can not overcome a discussion of the *theoretical* aspects of their methodology, they must therefore satisfy the dominant paradigm by offering it a preliminary empty theoretical gesture, as in logico-deductive theorizing. In this way, grounded theory is successful precisely because it negates its own status as a theoretical presupposition while simultaneously affirming it in the eyes of the dominant paradigm. It thus gives way to (the semblance of) freedom of interaction in process: “

[...] logico-deductive theory [as in positivist theorizing], [...] is merely thought up on the basis of a *priori* assumption and a touch of common sense, peppered with a few old theoretical speculations [...] The verifier may find that the speculative theory has nothing to do with his evidence, unless he *forces* a connection (emphasis added; Glaser & Strauss, 1967: 29).

It may be said that grounded theory *forces* the researcher, more than any other methodology (despite its stated intentions) to form a connection between data and theory. The promise of this approach is to be found in its ability to retroactively inscribe the empty theoretical gesture with symbolic content. Finally, we have the standard for a truly revolutionary paradigmatic shift in methodology: “we have taken the position that the adequacy of a theory for sociology today cannot be divorced from the process by which it is generated” (Glaser & Strauss, 1967: 5). It thus becomes possible to envision an intimate connection between the methodology of Lacanian psychoanalysts⁴ and qualitative research methodologies. This was the crucial but missing component in Vanheule et al.'s study *Burnout and intersubjectivity: A psychoanalytical study from a Lacanian perspective* (2003).

In a critical book on contemporary Lacanian theory, Slavoj Žižek described the necessary movement from conventional understandings of symptom to the more nuanced understanding of *sinthome*: “We can say that 'symptom' is Lacan's final answer to the eternal philosophical question 'Why is there something instead of nothing?' – this 'something' which 'is' instead of nothing is indeed the symptom” (1989: 77). The symptom comes about as the result of symbolic impositions onto the lack at the heart of social existence, it is the *something* which keeps subjects from confronting the horrors of the *nothing*. The conventional notion of symptom stops short of engaging with the real of existence, it does not identify with the philosophical assumptions guiding the research process.⁵ In sociological terms, *symptom* is the result of a fundamental refusal on the part of the subject to identify with the obdurate reality of social and symbolic interaction (Fine, 1999: 537). Instead, the notion imposes a foreclosure on the complexity of social existence by pinning it to an *a priori* commitment and then endlessly subjecting this complexity to the foreclosed symptom. Žižek proposed that this is a failure at the level of the subject's identification, and that it is the role of the psychoanalyst to free the subject from this way of identifying: “when we are confronted with the patient's symptoms, we must

first interpret them and penetrate through [that] which is blocking further movement of interpretation; *then* we must accomplish a crucial step of going [...] of experiencing how the [...] formation just [...] fills out a certain [...] lack [...] in the Other” (emphasis added; Žižek, 1989: 80). This latter step results in a perspectival shift from symptom to *sinthome* and *then* back to symptom again. Grounded theorists—unlike many qualitative researchers that mimic/recast positivist assumptions—have been among the first to achieve this perspectival shift in sociological research: they engage in the complexities of social reality and understand at some level that the symptom should be conceived as *sinthome*; that is, grounded theorists seek to identify the 'cultural consistency' of various social environments as a way to demonstrate how researchers and subjects impose meaning onto the world (Cf., McCracken, 1988: 18-19 & Žižek, 1989: 81). The appropriate response to Vanheule et al.'s study (2003) is to claim that the research design and overarching methodology hindered the ability of the researchers to provide readers with a more rigorous examination of burnout among the subject/sample population – instead, we are prematurely provided with the foreclosing symptom of burnout.

Burnout and Inter-subjectivity

With reference to the literature, Vanheule et al. (2003) concluded that there is a discernible connection between the subject's perceptions of their work relationships, pathological stress patterns, and burnout and that this connection merits further research. However, they discovered little in the way of qualitative research that “systematically examine[d] the connection between burnout, on the one hand, and typical patterns in the perception of interpersonal relations, on the other, in a methodological stringent way” (Vanheule et al., 2003: 322). The authors believed that a Lacanian perspective remedied this omission by providing rich theoretical descriptions of the inter-subjective dimension of social experience “since for Lacan the intersubjective relation [...] is central” (Vanheule et al., 2003: 322). Having established Lacanian theory as being up to the task of providing a unique perspective with regard to the subject matter, and qualitative research as the overarching methodological framework, the authors presented a tremendous breadth of insight into Lacan's theoretical enterprise.

First, Vanheule et al. (2003) introduced the reader to the philosophical underpinning of Lacan's notion of inter-subjectivity: the master/slave relationship. There are two things to consider here. First, the master/slave relationship produces two types of identifications (or “styles of interaction”) toward the other: imaginary and symbolic identification, and; second, the relationship toward the other is dependent upon three stages of development (primordial symbolic recognition; the imaginary struggle for power, and; symbolic redefinition) (Figure 1 in Vanheule et al., 2003: 323). According to Lacan, the process of identity construction proceeds on the basis of the mutual recognition of the others

condition, between master and slave; as Vanheule et al. put it: “[this] relation is [...] created when person x and person y implicitly *agree* to take up, respectively, the position of 'slave' and 'master'” (emphasis added; Vanheule et al., 2003: 323-3). It is therefore the symbolic recognition of the other as master which founds the subject's relationship to her or himself as slave, whereby the message 'you are my master' is transmitted from the subject to the other and then reflected back in the inverted form 'I am your slave' (Vanheule et al., 2003: 324). This permits the second stage of the development to occur whereby the master is regarded as an authority that deprives the slave of freedom and produces her or his discomfort (Vanheule et al., 2003: 324). The structural realities of subject/other and slave/master are transcribed into roles or performances and conflicting interests are pursued. A battle begins within the domain of the imaginary order: each subject has a preconceived image of the other and this impresses upon the reflections of her or his own identity (Vanheule et al., 2003: 324). Based on this understanding, the authors made a preliminary recommendation: to avoid burnout, one must “return to the basic relation and [...] develop a meta-perspective on the relation structure” (Vanheule et al., 2003: 327).

Sample, Measures, Analysis & Results

The authors assumed that the subject's discourse about the functionality of a given subject group in their place of employment was an accurate reflection of the relationship that the subject had to her or his symptom. The foundational question asked during the interpretation phase was: “Do the people identify themselves with the role outlined in the narrative scenario (an imaginary reaction) or do they try to establish their relation in their own way (a symbolic reaction)?” (Vanheule et al., 2003: 328).

Sample

The authors spent the majority of their sampling section discussing the preliminary quantitative screening process from which they drew their smaller “random sample”. A discussion of the relevance of such numbers as the percentage of female respondents, the average age of respondents, and length of time working in the sector was sorely missing from the essay (this information was relevant only to the quantitative sample). The sample relevant to the study at hand was selected from a larger group of 185 people who responded to a screening question that they would like to participate in an interview with researchers. Of this larger population, only 15 of the highest and 15 of the lowest respondents from the initial screening were selected to participate. There are a number of errors and omissions to be found in the sampling section of the article. While in the larger quantitative screening the population was drawn from a random sample⁶ this does not necessarily mean that the sampling

procedure used in the subsequent qualitative study was likewise based on this same sampling method (as the authors have claimed); instead, it seems that sampling at the qualitative level was performed according to what has commonly been called “purposive” or “theoretical” sampling. As Barbour put it,

With purposive sampling, researchers deliberately seek to include [...] deviant cases to illuminate, by juxtaposition, those processes and relations that routinely come into play, thereby enabling 'the exception to prove the rule'" (2001: 1116).

Vanheule et al., (2003) purposively selected 15 of the highest and 15 of the lowest scoring respondents from the original population, thereby allowing for the comparison of two extreme deviant populations (those with the highest burnout scores and those with the lowest). It was believed that the discourse about each subject group's relationship with others in the workplace would allow for the emergence of patterns of difference during the coding process. Had this study been focused more on the differences found in the described perceptions of burnout between women and men in the workplace, which many readers may have been expecting from the study, the analysis could have explored these dynamics (Ambert et al., 1995: 885). However, the reader is left with a strange lingering question as to why such information about the screening sample was included and why this information was thought to be of more relevance than the purposive sampling methods. Barbour contends that this approach to sampling does not use the qualitative data to full advantage, Vanheule et al., have not explored some of the differences between types of respondents during the analysis: what use is it to tell us about the differences in gender or age if this information is not again made relevant during analysis?

One can imagine a number of alternative ways to gather a sample of relevant data. A theoretical sample would have allowed for the emergence of theory grounded to the best extent possible in the data: one might have gathered subjects suffering with symptoms of burnout and compared their responses internally and externally against a group of subjects who have not experienced symptoms, without necessarily highlighting the quantitative sampling methods of previous studies. The researcher would therefore have the opportunity to shape themes effectively out of nothing, out of the complexity of the world according to the lives of subject groups, without presupposing the Lacanian theory of inter-subjectivity or the precise definition and concomitant symptoms of burnout: “

[Here] the analyst makes an initial selection of informants; collects, codes, and analyses the data; and produces a preliminary theoretical explanation before deciding which further data to collect and from whom. Once these data are analysed, refinements are made to the theory, which may in turn guide further sampling and data collection. The relation between sampling and explanation is iterative and theoretically led (Mays & Pope, 1995: 110).

Indeed, such an approach would have provided richer data, more appropriate theory, and improved rigour (as well as validity and reliability). Ambert et al. (1995) urged evaluators of qualitative research to ask the following question: “Was the sampling strategy clearly described and justified?”; indeed, we may conclude that Vanheule et al. have not provided readers the level of detail or justification that would be required for this study to be considered good qualitative research.

Measures

In their review of the literature, Vanheule et al. claimed that previous research into burnout was not 'methodologically stringent' (2003: 322); of course, the reader can only assume that Vanheule et al.'s study was an attempt to render this problem a thing of the past. However, one of the requirements of methodological stringency—what I will hereafter refer to as 'rigour'—must be to encourage qualitative researchers to provide a level of rich detail about the methods they used (Cf., Ambert, 1995: 884). Mays & Pope expanded upon this imperative, arguing that qualitative researchers need to create an account of method and data which can stand independently so that another trained researcher could analyse the same data in the same way and come to essentially the same conclusions (1995: 110). Unfortunately, the information presented does not measure up to the level of rigour that evaluators and readers have come to expect from good qualitative research.

The reader is told that the researchers used “semi-structured interviews” that focused on “the major difficulties that [subjects] experienced at work in their relations with clients, colleagues and executive staff, and how they dealt with such difficulties” (Vanheule et al., 2003: 329). The interview 'discussion' revolved around “two critical incidents” in the workplace as well as what satisfied them the most about the incident. We are told that “two trained interviewers conducted the interviews” but nowhere is the reader told if these were the same researchers that were involved in the publication of this article, if these were hired graduate or undergraduate students, the full extent of their training, the setting, whether or not they took field notes and, if they did, how these were used in the interpretation phase of the research (Cf., Kirk & Miller, 1986: 52-59), what protocols they were given to code the interview data, etc. This type of content analysis bears a striking resemblance to the more conventional design outlined by Altheide (1987: esp. 67-68). In the end, the reader is only told that the interviews lasted about 1.5 to 2 hours and were recorded on tape. The reader is not informed as to how the subjects were protected from harm during and after the research project.

Analysis

The reader is informed that interviews were typed out verbatim and processed according to the work of such 'grounded theorists' as Glaser & Strauss (1967) and Corbin & Strauss (1990). However, we are not provided with any indication of how this work influenced the analysis or results. Indeed, it is difficult to glean from the study that their work was anything but tacked on to provide for what Bryman & Burgess called 'an approving bumper sticker' (2001: 1116). At the very least, the researchers should have considered the inductive nature of the methodology outlined by both Glaser & Strauss (1967) and Corbin & Strauss (1990); the former two theorists described the process of "the discovery of theory from data [which is] systematically obtained and analyzed" (Glaser & Strauss, 1967: 1), and the latter described the process of "data collection and analysis [as] interrelated processes" (Corbin & Strauss, 1990: 6); and yet, the study at hand moves serially from theory to data collection and analysis and then to the verification or rejection of the original theory. Ambert et al. argued that good qualitative research "falls within the context of [the] discovery [of theory] rather than verification [...]" The primary commitment is to the empirical world" (1995: 880) and this serves as a useful a commentary, of sorts, in evaluating Vanheule et al.'s study (2003).

The interview data was coded and analyzed using the Atlastic computer program, but no detail was given as to what features of the program were employed and how the program assisted the efficiency of the overall project (Cf., Bringer et al., 2006). Computer assisted qualitative data analysis software (CAQDAS) provides the possibility of generating "excerpts from the program [that] can be included in written reports to demonstrate rigor and allow others to more accurately evaluate the research" (Bringer et al., 2006: 247) and yet no excerpts or indication of an audit trail were present in the final written report. One must presume that one of two situations occurred: (1) the authors presented the researchers with protocols developed on the basis of their theoretical vantage point and trained selected individuals to employ and seek out themes using the CAQDAS, ultimately seeking out data that verified the original hypothesis, or; (2) the authors hired individuals to ask lines of questions (and here no indication on the line of questioning is presented), record/transcribe the data, and present it back to the primary researchers for analysis. In any case, the codes were reviewed and all discrepancies were resolved, but the reader is not presented with any information as to how these discrepancies were resolved. Does this mean that contrary evidence was discarded or ignored?⁷ What were the discrepancies? Could these discrepancies have provided pathways into other categories? Perhaps the discrepancy was between two types of high burnout, which resulted in the three levels of identification outlined in the final write up. In the end, we are left to evaluate based only on our intuition. Above all else, there are two main issues here: the first has to do with the lack of detail (relating to issues of rigour and reliability), the second with the emphasis on the 'resolved' disagreements between coders (relating to issues of reliability and validity); Barbour argued that "the

degree of concordance between researchers is not really important; what is ultimately of value is the content of disagreements and the insights that discussion can provide for refining coding frames” (2001: 1116). Ultimately, this is a failure of reliability. However, there is also a double failure here at the level of validity. Mays & Pope argued that “the way the researcher examines 'negative' or 'deviant' cases—those in which the researcher's explanatory scheme appears weak or is contradicted by the evidence” is important for establishing validity and that “[t]he researcher should give a fair account of these occasions and try to explain why the data vary” (1995: 110).⁸ Another way of explaining this is that the rigour of the study is extremely poor, the reliability of the study is intuitively assumed to be weak, and the validity is presumed to be of extremely low grade.

An alternative source of validity—what Kirk & Miller (1986: 22-23) have called “theoretical validity”—comes from demonstrating that a correspondence exists between the theoretical explanations of the phenomena and the data from which the theory has been extracted; unfortunately, this type of coherence is difficult to achieve from the more positivistic methodologies, including qualitative methodologies that merely seek to verify existing theoretical perspectives. Ambert et al. have argued that this type of validity is, above all else, a goal of qualitative research, and that it is best achieved by refining the “process of theory emergence through a continual 'double fitting' where researchers generate conceptual images of their settings, and then shape and reshape them according to their ongoing observations” (1995: 881). Likewise, an alternative means of increasing reliability—what Kirk & Miller (1986: 42) have called “synchronic reliability”—may be established through comparisons of data which come from different sources: in this case, “reliability can be most useful to [...] researchers when it fails because a disconfirmation of synchronic reliability forces the [researcher] to imagine how multiple, but somehow different, qualitative measurements might simultaneously be true” (Kirk & Miller, 1986: 42). Naturally, grounded theory invites greater degrees of both theoretical validity and synchronic reliability by emphasizing the iterative process of theoretical explanation, data collection, data analysis/coding, and interpretation.

It is important to emphasize that the ultimate failure of this study was its lack of “thick description” (Ambert et al., 1995: 885) which is an indication of poor rigour. Rigour is of paramount importance for conclusively establishing degrees of validity and reliability (and, as we will see, generalizability), and could have been sufficiently incorporated into the final write up simply by extracting the data from the CAQDAS; the overall level of theory used in the final essay—which, many times, verged on redundancy—may have been reduced in order to include greater detail about methods. This would have also resolved some of the other issues presented here.⁹ Similarly, one would expect issues of validity, reliability, and rigour (but also generalizability and ethics) to be of equal importance in the clinical setting.

Results

Three types of relationships were distinguished on the basis of the analysis, each centering around a key 'catch phrase' (what we have previously defined as the symptom or core category): 1) "I refuse to accept that you are my master" (coded as "imaginary relationship"); 2) "My master's will is my law, but I get caught up in it" (coded as "imaginary relationship"), and; 3) "Departing from this structure, I am creative" (coded as: "symbolic relationship"). Each relationship was matched with the scoring group found in the preliminary quantitative screening: the first two constituted differing 'types' of the group with the highest burnout scores and the final relationship consisted of subjects with the lowest burnout scores.

The first relationship centered around an imaginary tension between the subject and the colleagues and executive staff. Subjects in this category were found to have passed responsibility for their dissatisfaction to somebody else in the workplace, they continually challenged this/these other person(s) as a result of the primary imaginary identification. The Lacanian insights about how this relates to the theory of intersubjectivity were outlined: "By identifying oneself as the other's opposite [...] people [...] feel [...] wronged, disappointment, envy, and aggression" (Vanheule et al., 2003: 330). A single quote from one of the respondents (Respondent 20) was used as an example of how an imaginary identification with an *other* can lead to conflict escalation ("one ends up in a vicious circle, convinced of being in the right, on which the battle is based") (Vanheule et al., 2003: 331); according to the theory, the respondent has not realized that the more he or she invests into defending the wrongfulness of the other the more she or he establishes for herself a symptom of burnout. Indeed, as Vanheule et al., have put it: "This reaction appears during the interview itself in the way the respondent goes on about the problem endlessly, without being able to create structure in the way it is talked about" (Vanheule et al., 2003: 331). The most typical battles that occurred among those in the highest type-1 burnout group were between colleagues and superiors and consisted of disagreements about who was "right" and "wrong" about particular critical situations (respondents 18, 19, 31, and 25 were cited, but not quoted, as examples) and battles among colleagues about who "deserves" positive evaluations from superiors (respondents 19, 32 were cited but not quoted).

Subjects in the high burnout score type-2 were found to be prone to self-sacrifice, typically effacing themselves for the sake of the other (Vanheule et al., 2003: 332). The relationship between subject and other centres on a longing to satisfy the perceived desires in the other. The slave identifies with the perceived desire of the master and embodies the position of the other's desire through the proper role of servant. Unfortunately, the work of the slave tends to become too much of a burden for the subject, resulting in burnout. Respondent 21 provided an excellent example: "I love my job. I do not want another job, but sometimes the pressure of work is too high [...] [I]f my colleagues or my superiors do not have enough time or are unable to do something alone and ask me to help, I think:

this is the interest of our work” (as quoted by Vanheule et al., 2003: 332). The tendency for subjects exhibiting this type of identification is to progressively eliminate interests that are contradictory to the perceived desire of the other, as a result subjects gradually restrict communication with others so as to prevent further role identification (Vanheule et al., 2003: 333).

Finally, subjects in the low burnout scoring group began from the determining symbolic structure, ultimately ignoring issues of power and the other's desire. These subjects deal with problems pragmatically and directly, they do not take direct responsibility for the conflicts between themselves and the other nor do they feel that the conflict embodies a deeper meaning about the relationship between their essential 'selves' and the essential 'other'. The authors put it nicely: “These people seem to do their work based on what they desire and on a wider view of life. Taking the situation of the conventions in the group into account, they try to establish the relations in their own way, and succeed in creating something else from a clear awareness regarding the place they have” (Vanheule et al., 2003: 334).

It may be argued that one of the greatest strengths of this study was in the area of generalization. Firestone described “analytic generalization” as the ability to take conclusions found in a specific study and use them to provide evidence that supports an overarching theory (1993: 17). Vanheule et al. (2003) verified the Lacanian theory of intersubjectivity (more particularly, the three levels of identification with the other) using the interview data and thereby conferred some degree of analytic generalization. However, there are two weaknesses present: first, one gets the sense that this type of extrapolation was not necessarily the intended outcome; instead, it is clear that the authors were interested in putting Lacanian theory to use by explaining burnout to those interested in studies of human relations in the workplace. Second, the researchers did not anticipate “plausible rival explanations” (Firestone, 1993: 19). Indeed, the researchers could have improved analytic generalization by seeking out rival explanations or negative evidence. In this regard, some of the following questions might have been posed: Are there any nonsubjective indicators of burnout that are worth examining in order to account for limitations to the highly subjectivist theory motivating the study? How did the “others” feel about the person experiencing burnout and how does their discourse relate to the discourse of the subject in question? Are there any factors contributing to the subject's symptom at the level of (super)structure within the workplace (i.e., role / occupation / duties / shifts)? Thus, while analytic generalization was an important outcome of the study, it must still be understood as very weak. Similarly, practicing (Lacanian) clinicians must be expected to generate valid and reliability conclusions such that they can improve their ability to extrapolate to theory and, indeed, advance contemporary Lacanian psychoanalysis in the broader community.

Conclusion

I contend that qualitative research may suffer from intellectual burnout as a result of a general desire on the part of individual researchers to reform, depart from, or accept without question, positivistic assumptions in one's own research methodology. Much of qualitative research appears to be in a perpetual stage of *becoming* with regards to its independence/divorce from traditional positivism; much of the research—including Vanheule et al.'s study on *Burnout and intersubjectivity* (2003)—has continued to move through successive reformations of the traditional positivistic world-view thereby reinforcing a position of inferiority by remaining at the level of the imaginary identification instead of reinscribing the symbolic relationship with active (rather than reactive) meaning. Lacan's methodology, as a rereading of grounded theory, promises a third path – one not followed by Vanheule et al.¹⁰: Lacanian qualitative researchers will have to closely identify with the *sinthome* of good qualitative research (grounded theory comes closest to this methodology). Moreover, this rereading of grounded theory will require researchers to produce more rigorous accounts of the research process than has been produced in Vanheule et al.'s study. Readers and evaluators must be able to assess—rather than assume that the researcher(s) are simply *sujet suppose savoir*—degrees of validity, reliability, and generalizability, as well as ethical guidelines.

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- 1 Corbin & Strauss have argued that qualitative research retains the “usual canons of ‘good [positivist] science’” but that it requires “redefinition in order to fit the realities of qualitative research” (1990: 4). Elsewhere they described this as a “modification” of traditional scientific evaluations (Corbin & Strauss, 1990: 4). However, they later argue that qualitative research has its “own standards—and canons and procedures for achieving them” (Corbin & Strauss, 1990: 4-5).
- 2 At least two theoretical assumptions influence the research design: 1) “[P]henomena are not conceived of as static but as continually changing in response to evolving conditions” (Corbin & Strauss, 1967: 5), and; 2) “Actors are seen as having, though not always utilizing, the means of controlling their destinies” (Corbin & Strauss, 1967: 5).
- 3 Slavoj Žižek, the foremost Lacanian theorist today, gave a great example of the logical inconsistency of establishing the symptom without first identifying with the *sinthome*: “In the TV-series Columbo, [...] Columbo first knows with a mysterious, but nonetheless absolutely infallible certainty, who did it, and then, on the basis of this inexplicable knowledge, proceeds to gather proofs” (Žižek, [2009]).
- 4 As Alain Badiou puts it: “Truth must be submitted to thought not as judgment or proposition but as a *process* in the real.[...] We continue with the process of a truth [...]” (2002). In terms of theory, I am prone to refer to this as the theory-process (like Badiou's 'truth-process'), where the theoretical emphasis is on the process, and the process is, itself, theoretically justified (i.e., we have both *theory*-process and *theory-process*).
- 5 And this commitment to philosophical reflection is a critical component of all (evaluations of) qualitative research. As Denzin & Lincoln put it: “All qualitative researchers are philosophers [and are] guided by highly abstract principles', [including] beliefs about ontology [...], epistemology [...], and methodology [...] These beliefs shape how the qualitative researcher sees the world and acts in it” (1994: 13).
- 6 And there is absolutely no problem with drawing from a larger random sample (Cf., Mays & Pope, 1995: 110).
- 7 I am convinced that this is what the researchers did: “With a view to comparison with Lacan's intersubjective theory, codes were grouped into co-ordinating codes and matrixes” (Vanheule et al., 2003: 329-330). Unfortunately, we are not informed as to how much of this data went to waste.
- 8 Furthermore, researchers should provide evidence that they *intentionally* sought out observations that might have contradicted or modified the analysis (Mays & Pope, 1995: 112).
- 9 Mays & Pope have also argued that proper evidence (fieldwork notes, interview transcripts, documentary analysis, etc) must be detailed in the final study for readers and/as evaluators of qualitative research to assess the overall study (1995: 112).
- 10 The data served to verify the *a priori* theoretical model of a single 'great man'; in the late 1960s, Glaser & Strauss challenged this method of research: “Part of the trend toward emphasizing verification was the assumption by many

sociologists that our 'great men' forefathers [...] had generated a sufficient number of outstanding theories on enough areas of social life to last for a long while" (1967: 10).