

EENY MEENY MINEY MO¹ – CHOICES AND CONSEQUENCES FOR GRADUATE STUDY IN EDUCATIONAL LINGUISTICS

Rinelle Evans
University of Pretoria

Novice researchers face interrelated and complex choices when embarking on an inquiry. The consequences or limitations of each decision may be far-reaching. This article is an auto-reflection on the intricate process of conceptualising and operationalising a thesis in which practical and contingent factors appear to have had a stronger influence in decision-making than philosophical frameworks or supervisory guidance. It aims to underscore the lengthy and often confusing route that graduates follow in pursuit of a higher degree. The target readership is students, emerging scholars, supervisors, and reflective practitioners in the educational linguistics research arena. The actual study pertained to a community project offering televised academic support to Grade 12 learners and sought to establish why the rate of oral interaction between presenter and learners was unexpectedly poor during instructional broadcasts. Key findings suggested that the rate of viewer participation during telelessons was not directly influenced by their limited English proficiency as initially anticipated, but by a combination of variables related to technical limitations, presenter nescience, and inappropriate methodological design.

Keywords

research design choices, consequences of research choices, postgraduates, research dilemmas, instructional television

INTRODUCTION

Unlike using the children's rhyme to arbitrarily eliminate someone from whatever game is being played, or to assign them an unenviable task, choosing a research design is far more complex. Yet, like the history of this particular children's rhyme, selecting the epistemological and methodological framework within which to pursue doctoral studies is fraught with contention and debate. It remains a common premise that good research is solidly grounded in the existent literature and that the lacunae which are identified often help to determine the selection of an overarching research design and methodology. Yet this is not an obvious choice, nor can it be taken for granted that a particular investigation merits a specific design. Where a research study has been commissioned or linked to a monetary grant, the design is often dictated by the funding organisation (Roberts, 2004). Doctoral students too, are at times at the mercy of a supervisor's preference and may have little choice in their research question or the protocols designed for data collection. There are numerous decisions to be taken at various points in the inquiry and making an injudicious choice may have ethical implications, contaminate the data, prolong the inquiry or, at worst, invalidate the findings (Anderson & Arsenault, 1998; Bogdan & Biklen, 2007; Leedy & Ormrod, 2010, McMillan & Schumacher, 2001).

Prior to engaging in doctoral studies, I had naively assumed that the only choices to be made were whether to enrol for a programme and, then, what to investigate. I knew from others that this apparently arduous task also entailed much complaining and frustration, yet few spoke of the lengthy and often confusing process of conceptualising an inquiry. Despite the numerous texts offering guidance on research methodology, I came to agree with Huberman and Miles (2002) that deciding on a research design is generally over-simplified and also not well documented. I reflect in this article on the subjective experience of engaging in doctoral studies and make explicit the choices I made while completing the inquiry. I start by explaining what prompted me to enrol for a PhD and why I selected instructional television with particular reference to poor viewer participation as a focus of inquiry. Next, I detail how I deliberated on a research design and justify the methods used for data collection and analysis. I also explain the unsystematic process of executing an inquiry within a broad paradigm that culminated in my findings and the formulation of an instructional dissonance theory as reported in my thesis. I conclude with a reflective discussion of the consequences and limitations of my deliberate choices.

CHOOSING TO ENROL

Prior to joining academia, I had been a teacher of English by profession. Primarily for promotional purposes, but also in the spirit of life-long learning, I had enrolled for five different career-specific programmes post-baccalaureas and had relished the academic challenge each offered. The hallowed halls of academia had never appealed directly to me although I had envisaged being a teacher educator as the pinnacle of my career. The choice to enrol after my appointment as a lecturer was thus driven by a combination of variables; chiefly overt pressure exerted by institutional requirements and peer competitiveness. Career advisors had also suggested that the letters PhD on a business card would create better consultancy options as one would be 'more marketable', endorsing the perception that a doctorate is a 'status-bringing "object" to be achieved' (Batchelor & Di Napoli, 2006:18). Others suggested 'finishing the academic race.' Once more, I picked up the gauntlet.

CHOOSING A FOCUS OF INQUIRY

I spent close on a year actively discussing several research topics with scores of senior academics. A Dean finally asked a definitive question which prompted me to choose televised instruction as a focus of inquiry. The actual study pertained to a community project offering televised academic support to Grade 12 learners and sought to establish why the rate of oral interaction between presenter and learners was unexpectedly poor during instructional broadcasts. The initial proposition that learners' poor English proficiency accounted for the low reciprocity was, however, refuted. This topic was a logical choice at the time as it was closely related to my working world. However, had I known then what I know now about formal research and the labyrinth leading to the professoriate, I would probably have chosen differently in order to expedite the route. Selecting a topic that either had more direct bearing on my Master's dissertation or had focused on a specific feature of English language teaching, would perhaps have positioned me professionally more directly within my expertise and experience. Nevertheless, as is often the case, choosing my topic was spurred by close personal involvement in a particular project where the phenomenon of low reciprocity had piqued my interest. Choosing differently may have caused me to fall into the trap of academic

expedience, as Reeves (2000:3) cautions could happen by ‘conducting whatever studies yield the most publications’.

CHOOSING AN EPISTEMOLOGICAL FRAMEWORK

Although I had above-average experience of studying and performing fundamental research tasks, I had obtained these formal qualifications prior to the mid-1990s; I had only been exposed to the basic quantitative methods of inquiry as were common at the time. I thus engaged in doctoral studies with the view to a scholarly career sans being grounded in ‘an extended disciplinary training in a particular network of research theory’ (Yates, 2004:72). This may not necessarily have facilitated my choice as, although I was seemingly less marked by prior moulding, I spent months wandering in a fog of ignorance sensing my way around, back-tracking, changing course and stumbling amidst the myriad nuances of basic research jargon and designs. Intuitively I knew that I would not fit into a neat, unadulterated paradigm. Useable but insufficient quantitative data already existed, collected as part of my responsibilities as project manager of the initiative I finally researched, yet, by virtue of my language and literature teaching background, I was more comfortable analysing and interpreting texts than working with stanines, means and correlations. I was also naturally inclined to a descriptive and narrative line of reporting and was thus grateful for the shift towards qualitative research that had taken place in the education domain since I had last engaged in studies. I do, however, admit that, swayed by the earlier hegemony of statistical methods and uninitiated into the qualitative paradigm at the time, I had several misgivings about such apparently subjective and non-quantifiable methods being sufficiently scientific.

Merriam (1998) refers to the researcher’s philosophical orientation as that fundamental belief about the nature of reality and production of knowledge. I immersed myself in an extensive reading programme, endeavouring to understand the research jargon of epistemology and apply it to my own philosophical stance. Not only was it necessary to clarify my personal thinking on these matters, but I was also obliged to select nomenclatures for my approaches, methods, and tools from the confusing set of labels used by various authors. To illustrate: Miller and Brewer (2003:238) acknowledge that ‘qualitative research goes under several sobriquets such as humanistic model of social research, unobtrusive methods and ethnographic approaches’, while Huberman and Miles (2002) state that the interconnected activities which define the qualitative research process ‘go by a variety of different labels’ Merriam (1998) in turn, refers to several terms used interchangeably. This particular choice took a long time as I kept cross-checking that the concept I had just discovered was indeed but another label for a more conventional one.

As my exploration progressed, I also discovered that an intense struggle between the two prevailing paradigms had taken place: both claimed superiority based either on the value of rich descriptive data or hard, generalisable data. Proponents of each claimed mutual exclusivity and this ‘binary divide’ (Miller & Brewer, 2003:192) was evident in the choice of words used to describe this period in research history: divide, schism, ardent dispute, contests, controversy, purists, versus, opposition, and even as serious as at war. The quantitative versus qualitative debate appears to have been so divisive that

... some graduate students who graduate from educational institutions with an aspiration to gain employment in the world of academia or research are left with the

impression that they have to pledge allegiance to one research school of thought or the other' (Johnson & Onwuegbuzie, 2004:14).

This slave adherence to a particular paradigm suggested not only a strategy to secure tenure, but also that strong emotional tension was commonplace among academics who were antagonistic towards and demeaning of others' beliefs. To me it also implied a lack of choice and limited vision; blindness to discovery as well as the possibility that data could be forced to echo the paradigm.

Although qualitative research was at one time considered inferior and controversial, particularly by natural scientists, the qualitative-quantitative debate no longer commands centre stage. The 'demise of a controversy' (Murray, 2003:6) has been the result of quantitative and qualitative proponents accepting that each approach is able to supplement the other, thus serving a complementary rather than dominant role. Qualitative research is now well established as a rigorous discipline and is applied by academics of note in the social and educational sciences, e.g. Creswell (1998), Denzin and Lincoln (1994), Dey (1993), Guba and Lincoln (1989), Krathwohl (1993), LeCompte, Millroy and Preissle (1992), Merriman (1998), Miles and Huberman (1994) and others. The substantial bank of methodological writings published by such authors have added 'significantly to the existing edifice of thought on qualitative methods and the world view behind them' (Tesch, 1990:16) also justifies my point. This approach to knowledge production is now fully fledged and so widely acknowledged in the broader research domain that one need not indulge in an extended defence of its merits. However, debates related to combining quantitative (QN) and qualitative methods (QL) in one research design by varying the weighting of QN/QL data, style of analyses and documentation persist. Johnson and Onwuegbuzie (2004) argue for a trilogy of research paradigms (QN, QL and mixed methods) or 'methodological pluralism' (Miller & Brewer, 2003), which describes the current practice of many scholars. They advocate that a mixed methods approach does not replace either of the traditional paradigms, but draws on the strengths of each and minimises limitations, offering 'a logical and practical application' (Johnson & Onwuegbuzie, 2004:17). Creswell (2009) purports that the application of mixed methods research has come of age, while Miller and Brewer (2003:239) promote the view that a researcher's choice of method (QN or QL) depends on whether 'technical expediency is a higher priority than methodological purity'. Even older theorists like Comaroff (1982) assert that there are no 'best' methodologies and that none exists outside any theoretical considerations. I was persuaded, too, by claims such as that the best answer (to a research question) frequently results from using a combination of qualitative and quantitative methods (Murray, 2003). The mixed-methods approach thus seemed an expansive and creative form of research that would not restrict or constrain; an eclectic means of thinking about and conducting research.

CHOOSING A RESEARCH DESIGN

Johnson and Onwuegbuzie (2004:24) describe a research paradigm as

a set of beliefs, values, and assumptions that a community of researchers have in common regarding the nature and conduct of research. The beliefs include but are not limited to ontological beliefs, epistemological beliefs, axiological beliefs, aesthetic beliefs, and methodological beliefs. In short, a research paradigm refers to a research culture.

It is these beliefs upheld by a research community, which commonly direct and help sharpen the focus of a study. I wrestled with the question: Does one have an inherent paradigm, adopted over years of scholarly exploration, or does one consciously choose one?

Hughes (1990) argues that research practice derives from methodological preferences, while Crotty (1998) suggests that the decisions pertaining to designing a research framework are interrelated and range from discerning the broad assumptions a researcher brings to the project to those practicalities of collecting and analysing the data. Creswell (2009) outlines matching the problem with the research approach, taking the researcher's personal experience and training into account and finally mixing the target readership into the equation. I spent many months making sense of these varying and, in some cases, conflicting views. At a research support session in the initial stages of my PhD studies, I was introduced to the four sociological paradigms (Radical Humanist, Radical Structuralist, Interpretivist, Functionalist) used for the analysis of social theories as propounded by Burrell and Morgan (1979). Their matrix – although not a research paradigm per se – categorises mutually exclusive social worldviews, and attempts to define fundamentally different perspectives for the analysis of social phenomena. This means that any system, or entity, may be viewed from whichever paradigm in an attempt to understand social action. However, the choice of paradigm implies that the particular phenomenon being studied will be scrutinised and textured differently if the same phenomenon were to have been viewed through one of the other paradigmatic lenses. It is against this background that I address the possible ways in which my study could have materialised and motivate why I chose the interpretative paradigm.

My study sought to answer why Grade 12 learners refrained from asking questions during instructional broadcasts, even though bi-directional technology allowed for synchronous, oral presenter-viewer interaction. Viewer participation in an instructional context is often associated with a political or ideological positioning and I could have chosen to locate my study within a discourse of power, autonomy, or emancipation. My supervisor – a proponent of Radical Humanism and an acclaimed critical thinker and leader in education – could have encouraged me in this direction, yet I did not feel pressure to bow to this framework of thought which seeks to liberate the individual from social constraints and to change the limitations of existing social arrangements. This positioning within the Radical Humanist framework would have implied a study about learners changing or recreating their world by being empowered. It would have implied regarding the telepresenters as agents of power or even oppressors, but I chose not to foreground power discourses, critical consciousness, or political emancipation. Although the unit of analysis lent itself to being considered a social issue, i.e. a community project established by a powerful colonial learning institute attempting to improve exit examination pass rates in disadvantaged communities, my inquiry was not set against the backdrop of critical theory or postmodernism. I do not claim political innocence, but I took no overt stance in identifying any underlying hegemony. Why not? No doubt, I chose to position my inquiry differently because I lack fluency in the discourse required to describe my discomfort with being part of an unjust system, which spawned the very disadvantage and inequity that the community project was attempting to redress. Perhaps I sensed patronage and, already engulfed by my own location within a set of power and pedagogical relations, recoiled from a possible barefaced address thereof. This avoidance strategy would undoubtedly have been a political reason for choosing to work within a less intimidating research paradigm.

The Functionalist paradigm is firmly rooted in the sociology of regulation and adheres to an objectivist point of view (Burrell & Morgan, 1979). In the guise of action research, this approach would have suited my political sensibilities and pragmatic inclination better. This quadrant mirrors the Pasteur quadrant of Stokes (1997), in that fundamental knowledge is sought within the context of solving practical problems. I had fleetingly deliberated whether I should approach the study as an action research project. Sufficient collaboration between participants and researcher existed and a change of practice leading to improved interaction may have been advocated, but the technological evolution of video streaming was advancing too rapidly to allow for such a project to evidence change. I would at best have managed to describe a single research cycle in my thesis without leeway to implement any further intervention. Advancing an agenda for transformation was thus not feasible either. Furthermore the participants were not a constant variable as learners annually passed through the education system while the same telepresenters were not necessarily appointed again. The uncertainties relating to the pending termination of the project also militated against pursuing this Functionalist approach. Potentially the emergent findings would not impact on the process sufficiently due to time and technological constraints and thus the interventions would not have been fully realised. Action research is generally very context-specific and my findings may not have been applicable to any other contexts. I would also have needed a more solid grounding in development and action research. The decision to discard this paradigm was purely practical.

I had the least predilection for the Radical Structuralist (positivist) paradigm. Not having a sound grasp of structural relationships, nor of the contradiction and modes of domination that exist within an 'objective' social world, this view was in conflict with my acceptance of a phenomenological reality. The multiple research sites would have offered rich possibilities for accurate sampling, as well as identifying correlation and perhaps even deviation trends. Yet, based simply on my perceived inability and lack of experience in working with sophisticated statistical methods, this choice seemed daunting. Although reliable on-campus support existed for processing and analysing quantitative data, the logistics of gathering sufficient and accurate statistical data within the required time frame was not feasible. In most cases, sites were geographically remote while lack of over-night accommodation and personal safety aspects complicated field visits. Logistically, it would have been virtually impossible to create the controlled conditions which experimental methods demand in a developing country context. Erratic communication networks and regional language barriers would have further complicated data gathering. In addition, it did not seem expedient to be generating numbers with the view to generalising and applying them to similar projects when only one other local instructional television project existed. Projects in industrialised countries had come full cycle and were moving into the on-line environment while those in developing countries had dissimilar contexts and thus the data may have had limited application. Although a questionnaire survey accounted for the largest portion of viewer-related data, I did not use statistical methods, as I did not attempt to measure a particular phenomenon. Responses were converted to percentages and merely served as tentative gauges of interaction and were helpful in guiding my questioning during the semi-structured interviews.

Working within a positivist view would have established more accurately exactly how poor and widespread the viewer participation was. I could, for example, also have correlated a variable such as the mother tongue, age, or gender of participants with their lack of interaction. I could have chosen to compare the interaction patterns of various sites or those of first (L1) and second language (L2) speakers viewing the same lessons and thus have confirmed or refuted my initial hypothesis that insufficient English proficiency was the prime

reason for poor interaction. Furthermore, this paradigm did not favour the research question, which had been formulated to suggest an understanding rather than a calculation of the data. My 'Why' rather than a 'How' research question allowed for both an investigation of real-life events and a holistic description of the participants' experience of interactive televised instruction in their particular context. It also seemed an ill-match of numerate data while seeking to measure a social phenomenon that was not necessarily 'an external, knowable entity' (Miller & Brewer, 2003:123).

Perhaps the prime reason for working within a less objective, standardised framework was that, although the data generated during the pilot study were modest in scope, their quantitative nature did not suggest a key explanatory variable. Tentative findings helped identify and shape early propositions, but I realised that I needed a multiplicity of data sources and collection methods in order to more accurately establish factors that may have inhibited oral interaction during a televised lesson. Several subsidiary uncertainties remained regarding the low levels of reciprocity and thus the paucity of answers generated by quantitative data was a deciding factor in not using a Radical Structuralist approach.

Since aspects of an oral communication cycle and, by implication, human interaction and social relationships were under scrutiny, rich, descriptive data was best solicited using qualitative methods. In fact, the tinder for my inquiry was a quest for a deeper understanding rather than a clinical reportage of findings and I thus framed the research within the interpretive tradition. A central tenet of this paradigm is to refute the existence of an objective reality by framing the social world as a process by which individuals created shared meaning for themselves. As Merriam (1998:202) states: '... reality is holistic, multidimensional and ever-changing; it is not a single, fixed objective phenomenon waiting to be discovered, observed, and measured as in quantitative research.' The assumption underlying qualitative research, that reality can never fully be declared or understood, implies that knowledge creation (research) can only discover an approximation of the truth or provide perspective rather than truth (Patton, 1990).

It was these multiple perspectives of realities and consultation or, better still, collaboration with all role players in a particular social setting that allowed for a richer understanding of a reality. Further justification of my choice lay therein that the study primarily was set in a particular context that relied heavily on the personal insight of the researcher as co-creator of meaning. Secondly, my description of reality was systematic but not confined to a rigid, prescriptive framework. Appropriate to this framework was the hybrid methodology I chose above Guba's (1990:81) belief that 'accommodation between paradigms is impossible', ignoring, too, the incompatibility thesis (Howe, 1988) which posits that QL and QN should not be mixed. In contrast, Johnson & Onwuegbuzie (2004:24) use the label 'integrative research' as it is more inclusive and paradigmatic, and recommend applying the contingency theory that accepts that all three paradigms are superior under different circumstances, but places the responsibility for making the appropriate choice of approach squarely on the researcher.

For this study, I used a mixed-method approach to data collection for both descriptive and explanatory purposes. I used quantitative instruments (questionnaires and presenter logs) in conjunction with qualitative methods and strategies (viewer group interviews, presenter interviews, site facilitator telephonic interviews, video analyses, transmission observation, researcher field notes). By utilising a small-scale quantitative approach, I endeavoured to establish how prevalent poor participation was, while rich experiential data, e.g. interviews,

identified why viewers refrained from interacting during televised instruction. Multiple data sources and collection methods enabled me to gather substantial situational information in a natural setting. The tools for data collection were designed so that the research question could be answered in the most appropriate ways. My personal inclination to reflective thinking also enabled me to gain an understanding of the behaviour of the participants and render their voices as clearly as print permits, in order to enhance an understanding of their ITV experiences. Furthermore, I was able to easily adapt the traditional formal style of academic writing to a more appropriately intimate one as I rendered the direct experience and perceptions of the participants in this study within their unique context, using rich, empathic description. I thus argued for the positioning of my inquiry within the interpretivist framework, the prime purpose of which is describing and interpreting phenomena (Reeves, 2000:7). I sought, by means of 'a creative enterprise that depends on insight and imagination of researcher' (Miller & Brewer, 2003:64) to attach meaning to the data. This process was not mechanical but required 'skill, imagination, and creativity' (ibid) yet sufficient rigour, as the interpretation of the data needed to be credible enough to stand public scrutiny. Although common denominators exist in the diverse views of reality, the Interpretive paradigm differs from the Radical Humanist in that it seeks to describe and explain rather than transform reality. It is from this theoretical position that I turn to an explanation of the methodological process.

CHOOSING THE RESEARCH METHODOLOGY

Since the community project, as the unit of analysis, not only involved various participants, but also had a definite context set within specific parameters, it could rightfully be considered what Smith, in Merriam (1998:27), calls a 'bounded system' and classed within the research genre as a case study. Huberman and Miles (2002) define a case study as a research strategy which focuses on understanding the dynamics present within single settings. Yin (1994), in turn, suggests that a case study is the preferred strategy when the researcher has little control over actual behavioural events or when the focus of the study is on contemporary phenomena. The notion that the researcher is separate from the subject of study is not compatible with interpretive philosophy. I, too, could not separate myself from what was being studied, all the more for having worked so closely with the project over several years in a variety of roles. Furthermore, the formulation of my research question also favoured a case study method focussing on participants' experience of interactive television in a developing country context. The primary informants were the nine subject experts who presented telelessons, close on 300 viewers and five site educators. I myself was a participant observer in this inquiry. I undertook fieldwork at several sites; chiefly those related to the participants' immediate domain, i.e. viewers were interviewed at school and presenters at the studio facilities.

A case study typically permits the combination of data collection methods and varied data sources provide both quantitative and qualitative evidence. The unique strength of a case study lies in the richness of evidence it generates. It is the rigorous and fair presentation of such empirical data that makes the case study a challenge, even more so since, as a research endeavour, it has been strongly criticised for bias. Case studies, although currently not in vogue and considered an easy way out by some, do permit accomplishing various aims (Huberman & Miles, 2002). Mine provided an explanation and generated a theory.

Henning, Van Rensburg, and Smit (2004:11), purport that the purpose of a particular inquiry primarily determines the method of data collection and analysis. Although I am in agreement

with their notion that the inquiring and dialogical mind carefully considers various options and needs the liberty to change tack as circumstances demand, the sobriquet bricoleur that they use to describe the qualitative researcher suggests cobbling bits together as best one can rather than becoming a master at the craft. I am also in contention with Roberts (2004:110), who admits that the choice of a research methodology is not simple, yet simplistically proceeds to state that the methodology selection rests primarily on the problem being investigated, the purpose of the study, the theory base, researching skills and the nature of the data. I found this latter principle confusing as it inverted the logic of systematic planning prior to data collection. Her assertion that students should not try to make their study fit a predetermined methodology but that ‘...learning by doing’ [was] the name of the game’ contradicted the more conservative views which suggested a definitive proposal prior to embarking on a full-scale enquiry. A final incongruity is her reminder that selection of a research design may depend on the research committee members which, in fact, implies lack of choice on the part of the postgraduate student. I aligned the sequencing of my research phases with the advice offered by Reeves (2000:8) that ‘research methods should not be selected until a researcher is clear about research goals as well as the nature of the research question to be addressed within a particular study’. My choice of methodology was thus not only moulded by the task in hand but also by personal preference and the availability of collegial support, with special reference to the use of Atlas.ti™ – a software programme used for qualitative data analysis.

Another choice made related to ensuring trustworthiness or validity in my inquiry. Again multiple perspectives on what this concept constitutes abound, and the array of terms used by the theorists is just as confusing: authenticity, goodness, adequacy, and credibility. Creswell and Miller (2000) suggest that the choice of validity procedures is governed by how researchers choose to validate their studies as well as their own philosophical assumptions. I chose to triangulate certain data; use member checking; researcher reflexivity; create a systematic audit trail; and peer debriefing for various procedures or phases of the study. I reported using thick, rich description, while candidly disclosing my role and assumptions as researcher, as well as possible biases. I made many other choices, e.g. determining research site criteria, which were no less significant or easy, but they have not even been alluded to due to length limitation. I now review broadly the outcome of the various decisions I made.

CONSEQUENCES OF MY CHOICES

Although I had a fair sense of direction by the time I defended my proposal, my ‘complete strategy of attack on the central research problem’ (Leedy & Ormrod, 2010:46) was not all cut and dried. Since my inquiry had not been commissioned or bound by the constraints of a funding agency, I was not only at liberty to choose my supervisor but also had free rein with regard to formulating the research question. I experienced no pressure to work within a particular paradigm since no institutional perspective was entrenched. The degree of freedom I enjoyed in making the various choices would possibly be envied by those more restricted. Yet without definite parameters, my unbounded independence resulted in much insecurity – a classic trait of doctoral work (Creswell, 2009; Batchelor & Di Napoli, 2006; Graves & Varma, 1998). My uncertainties may possibly have prolonged the research process, yet my initial ‘struggle-and-search’ has undoubtedly deepened my conceptual understanding of how knowledge is created. The process of research design and positioning had not been linear either, but iterative and unpredictable. By this I mean that, prior to my initiation into multiple methodological paradigms and intellectual frameworks, I had been unfamiliar with the

profusion of labels used to describe various methods and protocols and therefore some decisions, which had been made intuitively, only gained labels later.

An analysis of my reflections on the multiple decisions made during the research experience suggests that, apart from one's perceptions of reality, several other factors, some of which are external to the cognitive and affective domain of the scholar, influence the choice of a design. As a visual summary, I consider the sway of the following three broad categories outlined in Table 1.

Table 1: Factors influencing choices in an inquiry: types and degree

Categories	Types	Degree of influence		
		High	Med	Low
Innate factors: Researcher	Position as researcher * <i>political (ideological) orientation</i> * <i>pedagogical view</i> * <i>personal preference</i> * <i>professional training and ability</i>	X X X X		
Innate factors: Inquiry	Posing of the research question(s) Parameters of the inquiry Practical considerations Paucity of data		X X	
External factors: Researcher and inquiry	Pressure of an institutional philosophy and tradition Persuasiveness of a supervisor Peer pressure to enrol			X X X

It would thus appear from the categories suggested above that factors inherent to the researcher primarily influence the various choices made in the research process, while factors pertaining to the inquiry, e.g. how the context and constraints of the inquiry dictate the circumstances in which research could progress, also have a significant impact. On the other hand, the external factors lying on the low extreme of the continuum are context-bound and had no apparent influence on my choices. This runs contrary to conventional opinion that claims pressure from an institutional philosophy or the persuasive powers of a supervisor severely restrict the options that a doctoral candidate may execute.

The various choices I made throughout the research process culminated in two unexpected outcomes: the invalidation of my initial proposition and the discovery of several mismatches. In the first place, interaction during telelessons appeared to be constrained by factors linked to presenter-as-initiator-of-communication, rather than viewer inhibitions or linguistic shortcomings as initially thought. Had I chosen to work in another paradigm other than interpretive, I would not have discovered that the rate of viewer participation during interactive TV lessons was influenced by a combination of variables rather than by an isolated factor of limited language proficiency, as originally conceived. Although these variables influenced certain broadcasts singly, the telepresenter was, in fact, the silencer and played the compounding role in stifling interaction. Furthermore, I would not have discovered the many mismatches evident in policies and practices.

As I decoded the data at a complex level, the key findings revealed another dimension. I had clustered the various empirical findings from three categories that encompassed the viewer, presenter, and context into the following themes: *Paradoxical perceptions*, *Presenter nescience*, and *Problematic practicalities and partnerships*. A fine-combing of these themes gave rise to a sense of mismatch or noise. In several instances, the presenters' message was distorted in such a way, by several factors, that its delivery or decoding was not aligned to expectation. I not only pinpointed an unpredicted disparity between my initial propositions about why viewers were not participating and the eventual findings, but the data also indicated several facets that ought to have dovetailed neatly but which were at variance with each other. These discrepancies resulted in mismatches of expectations, needs, and application and gave rise to my formulation of an instructional dissonance theory. By this I mean the ignorance or denial of hindrances, barriers, and distortions that permeate and negatively affect interpersonal communication between the instructor and learner. Instructional communication is successful but not meaningful, i.e. despite a carefully encoded message, which is sent untrammelled and also decoded (acknowledged) successfully, a mismatch of meaning (sense, utility) occurs. In order to start creating instructional equilibrium again, I underscore the importance of a meticulously encoded instructional message coupled with effective lesson design and apposite presenter behaviour during content delivery. In order to restore full balance, dynamics relating to the viewers and technology also need consideration (Evans, 2005:168). As Wisker *et al.* (2003:92) acknowledge, the use of positivistic quantitative methods 'fail to capture attitudes, feelings and developments'. In statistical terms, dissonance was an outlier sans numerical significance and not identified until a qualitative methodology was used. It was also the combination of methodologies that steered my deeper understanding of poor reciprocity in this study.

CONCLUSION

As a result of the particular choices I had made during the inquiry, my initial proposition was negated and the theory relating to instructional dissonance emerged. I hesitate to judge my choices as right or wrong, but concede that choosing differently would have had different consequences. I still have unanswered questions; new ones have surfaced from the process. For instance, I did not discover the full extent of the viewers' inadequate English proficiency, nor the degree to which cultural reticence, in fact, may have been an inhibiting factor in more traditional classroom settings. Furthermore, I would like to have known why the presenter training was apparently not effective and whether radical operational and even technical interventions would have improved presenter-viewer interaction. These considerations may highlight shortcomings in my inquiry, but had I made other choices, the process of inquiry may have produced findings which would have differed vastly from the thesis eventually awarded the philosophiae doctor.

Should a child be familiar with the system of algorithms, it would be easy to anticipate which player will be chosen or discarded by merely starting the counting rhyme referred to in the title of this article, at the appropriate point. This account of how I deliberated on the process of selecting or eliminating research paradigms and their related methodologies, indicates that choosing one is far more complex than picking a playground team mate. The choices made during my advanced studies were not as random as the title implies and thus did not yield predictable consequences. Although several considerations impacted on my choices, my biases arose primarily from a particular preference for a theory of knowledge and ultimately I engaged in the social research practices associated with my philosophical frameworks.

Researchers are thus seemingly able to perform best when their work is grounded in an epistemological position with which they are comfortable (Henning *et al.*, 2004:1). Yates (2004:65), on the other hand, claims that ‘Good research is research that demonstrates an original voice and an original perspective, that is in some way creative.’ The decisive consequence of my choice to embark on the process of completing this higher degree has been a change in and renewal of my learning experience allowing for “the formation of a new sense of identity as somebody who can consciously make sense of the world ...in richer and more complex ways” (Batchelor & Di Napoli, 2006:17). Ultimately, in order for it to be thus, one ought to operate, unshackled by external pressures, within a familiar framework which permits the creation of a research design that dovetails with the particular context, as well as one’s personal philosophy, preferences and proficiencies.

END NOTES

1 Etymological references are not in agreement. Some suggest that this counting-out rhyme used in children’s games was originally a type of exorcism used by medieval druids; others claim it to be a Scottish rhyme become garbled by juvenile tongues. Some sources suggest that the racial term prevalent in certain variants pre-dates slavery and thus actually refers to the Devil. No racial slur is intended.

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BIOGRAPHICAL NOTE

Rinelle Evans holds a doctorate in curriculum and instructional design with special reference to instructional communication via television technology. She currently is a senior lecturer involved with teacher education at the University of Pretoria (South Africa). Her academic interests relate to English language teaching, instructional communication and dissonance, multiliteracies and bilingualism. Email: revans@postino.up.ac.za