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Good is not good enough

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TILBURG UNIVERSITY

Understanding Society

TilburgPapers in CultureStudies

Paper 65

Good is not good enough:

Combining surveys and ethnographies

in the study of rapid social change

by

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Combining surveys and ethnographies in the study of rapid social change

Jan Blommaert & Fons van de Vijver

Introduction

This short research note is intended to stimulate debates across disciplinary and methodological-traditional boundaries. Such debates, we believe, are necessary for several reasons, of which two can be highlighted: One, exploring compatibilities and synergies across and beyond the layers of disciplinary structure that determine academic work is at the heart of scientific practice: it is the search for innovation and optimization of research tools and insights. Two, such exercises are always, perpetually, needed because our objects of research refuse to sit still. As scholars of humans in society and culture, our research instruments demand perpetual reality checking, because humans in society and culture are unpleasant enough to change perpetually, and methods for understanding social and cultural processes that were adequate yesterday are not guaranteed to be adequate tomorrow.

Certainly in an age of globalization, superdiversity, and complex online-offline dynamics in social and cultural life, several major adjustments are required to a scholarly apparatus rather more at ease with clear-cut categories, linear processes and transparent propositional meanings. This contemporary world is characterized by very rapid social change, some of it superficial but other aspects of it fundamental. Such changes occur across the total specter of human activity, collective as well as individual: they involve intense demographic changes; changes in the economic structures of society; in social structures of community, belonging, power and entitlement; in political structures responding to these other changes; to cognitive, psychological and identity changes; to linguistic and semiotic changes; to changes in the relationships between urban and rural areas, regions, nation-states and larger geopolitical units in the world. Such changes were anticipated almost two decades ago by e.g. Castells (1996) and Appadurai (1996). But it is now that the full scope, scale and weight of these changes become apparent and inescapable

These rapid processes of change defy the synchronic, sedentary, linear and static bias of sciences based on structuralist assumptions about the social and cultural world; they also challenge the structuralist assumption that generalizations need to be context-free abstractions; and they question the most fundamental presuppositions deployed in disciplines about how the world is and how humans fit into that. *Complexity*, *mobility* and *dynamics* are key defining features of the present world; they need to be converted into useful research instruments, and this job is far from being done at present.

In what follows we will attempt to sketch a broad methodological platform on which two very different methodological frameworks will be joined: ethnography and survey research. It is part of the tradition we mentioned just now that many would see these two approaches as the extreme poles of a continuum of scientific methodology. The former one would be the archetypal 'qualitative' kind of research, focused on intersubjective small-scale interpretive work; the latter would be the archetypal

'quantitative' approach based on an analytical distancing of researcher and object, standardized procedures and statistical factual outcomes. The non-compatibility of both is often, and widely, taken for granted; and practitioners of both (this does not exclude the present authors) would often be seen as hardnosed methodological fundamentalists with severely limited patience for the arguments of the other side. While both of us are profoundly committed practitioners of our respective disciplines, we believe that this old ritual of mock hostility prevents certain interesting and creative things from happening on both sides; and we believe that exploring differences and similarities between these frameworks can be highly productive in light of the challenges described earlier: we share an object, and this object is changing fast; all of us, consequently, permanently risk getting out of touch with that object.

The notion that we share an object is fundamental. It is the shared nature of the object that should make us realize that the views of that object from within our disciplines are necessarily partial. Different views represent different sides and features of the object, *but it is the same object*. And an object as complex as humans in society and culture surely tolerates multiple and very different views which, together, might perhaps bring us closer to a comprehensive picture.[1] Interestingly, this exercise has precedents, and a lot of what we will say contains echoes of work done decades ago but largely neglected nowadays.

The issue: what do we know?

If social and cultural environments are marked by complexity, mobility and dynamics, it means that very little can be presupposed with respect to the features of such environments. We can believe that a 'neighborhood' hosts a sedentary population – the neighbors – only to discover that an important part of the population there is actually nomadic, using the neighborhood as a transit zone to other destinations (e.g. Blommaert 2013). We can believe that two refugees from Iraq share a significant amount of features, enough to treat them similarly, only to discover that one is a highly educated person who prepared his escape from Iraq in some detail and can rely on a support network in the diaspora, while the other person is the victim of brutal and exploitative human trafficking leaving her with no support whatsoever, and with a very different set of challenges from the first one. We can believe that a pupil is learning disabled because he cannot memorize the inflections of the French verb *avoir*, only to discover that that same pupil knows all the players of FC Barcelona by heart, as well as huge amounts of relevant data about them. We can believe that a 'friend' on Facebook is a 14-year old girl, only to find out that this friend is a 28-year old man.

Such problems of presuppositions not holding true confront a broad variety of actors, from immigration officers to schoolteachers, urban planners, medical personnel,

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[1] Other sciences appear to have captured this. Establishing a diagnosis in a cardiology unit, for instance, involves extraordinarily divergent methodologies, including ('ethnographic') clinical inspection of outward features of the patient, anamnesis (i.e. talk), blood pressure measuring, blood testing, electro-cardiography, magnetoscopy and scanning, physical stamina testing, and so forth. The object – the patient's heart – holds it all together.

policy makers and – note – researchers. Superdiverse environments are best seen as complex systems in which several different forces generate crisscrossing, overlapping and conflicting processes, in which the statistical average or numerical majority is not the engine behind power and control, in which small 'deviations' or 'exceptions' – the 'error margin' of large-scale research – often represent the onset of massive future change, and in which well known activities and characteristics may result in nonlinear, unexpected effects. Complex systems have stochastic characteristics: they move in directions of which the general vector can sometimes be identified, but of which the precise outcomes cannot be predicted (cf Prigogine & Stengers 1984; see Blommaert 2013 for a discussion).

This generates problems. If a researcher cannot rely on accurate and robust baseline knowledge of the social and cultural environment s/he investigates, epistemological and methodological problems quickly occur at all levels. Designing a reliable population sample for survey research, for instance, is hard when the baseline knowledge of the population is questionable; research on social media usage in China, where due to strict internet policing very many users hide behind aliases and phony profiles, is a case in point (Wang et al, ftc.). If we don't know how many 'real' people are there, whether these people are male or female, young or old, highly educated or not, and so on – how then can we construct a reliable and representative sample? We can always make a sample, surely, but what is that sample actually representative of?

Informed readers have detected by now that the foregoing point leads us to a well-known issue in social-scientific and humanities research: the problem of ecological validity in survey work. The problem was powerfully sketched and stated half a century ago by Aaron Cicourel (1964); a more recent and clear formulation by Cicourel is the following:

> "The ecological validity problem can be stated as follows: To what extent is the content of questions asked commensurate with the socially distributed knowledge possessed by the respondents? Do the questions asked address topics, beliefs, attitudes and opinions the respondents routinely discuss in everyday life during social interaction with others? Further, to what extent can we assume that given the absence of ethnographic information about different communities, we can ignore the extent to which the wording and content of the questions are comprehended similarly by the entire sample? Are the questions, therefore, different from or are they in correspondence or congruent with observing the way respondents express themselves in their daily life encounters with others?" (Cicourel et al 2004: 8)

What Cicourel flags as a major issue here is that, when we abstain from involving ourselves in ethnographic research that establishes the specific universes of meaning and interpretation that individual people use in handling survey questions, we can only compensate that absence of ethnographically gathered knowledge by using a very large, and *valid*, range of presuppositions about the sharedness of such universes among our respondents. This can, perhaps, be reasonably feasible when we investigate groups of people of whom we know they share a lot of social and cultural material, and use this sharedness as our focus of investigation (think of small religious communities or soccer teams); it is however extremely hard to sustain when we engage in research involving very different groups of people. The more diverse the

populations we investigate, the lower our certainty about the characteristics of these populations. Simple-sounding terms such as "labor", or "satisfied", used in identical questions to a very diverse sample of respondents, may consequently be understood in ways so deeply different that it renders the outcomes effectively incommensurable. Pierre Bourdieu – a close friend and associate of Cicourel – made a similar point when he observed that opinion research presupposes the fact that everyone *has* an opinion on the topic of research, whereas for many respondents the confrontation with a survey question is actually the first time ever they are invited to think about a particular topic (Bourdieu 1972 [1994]). And both were undoubtedly inspired by Herbert Blumer's remarks, made in 1947, that "current public opinion polling has not succeeded in isolating public opinion as a generic object of study" (Blumer 1947 [2000]: 148). In other words: the 'public' qualification of opinions is not a priori an ecologically valid statement, for we do not know in advance whether the opinions we intend to describe are effectively present, shared, and generalizable as 'public' prior to our inquiry. *We cannot presuppose this*. What we can do, however, is to – first – establish ethnographically whether or not our object displays an adequate degree of social and cultural fit in the communities we shall investigate, and how this object is locally treated, thought about, discussed and/or acted upon by our respondents.

We will shortly move to consider attempts at doing precisely this. At present, it suffices to observe that the rapid change we currently witness in society and culture raises the issue of ecological validity from the dead and brings it back with a vengeance. The complexity now characterizing social systems precludes quick assumptions about semantic and praxeological sharedness and stability among nearly any population we intend to investigate. So what do we know? Not much in advance.

Ordered indexicalities

Cicourel's critique of ecological validity quickly zoomed in on the methodology of interviewing. Let us recall what he had to say on this issue:

> "The serious problem associated with self-contained interviews and surveys without ethnography is that we sample bodies but not their everyday behavioral environments. We lack systematic observation of respondents' daily life activities and the condition under which attitudes, opinions, beliefs, and folk knowledge emerge and are displayed. (…)
>
> Conducting interviews with closed- or fixed-choice questions or sending respondents questionnaires to fill out fixed-choice survey questions has often been an end in and of itself. There has been little or no interest in conducting systematic observation of the ecological settings in which respondents lived or played or worked, including their discourse practices. It is customary to interview a small sample of respondents before constructing a sample survey. The general idea was to explore questions in some detail before settling on a particular set of them for the larger survey. The small subset of a sample (the "pretest") was intended to satisfy validity issues, but respondents, even if interviewed at home, often answered questions with guarded enthusiasm. The quality of surveys, therefore, could be partially improved by using tape recorders. Random recordings would provide a sense of how the language used in the pretest oriented respondents to the study's goals and the extent to

which they resulted in modifications of the final questions employed. Tape recording a random subset of the final questionnaire sample could enable the research analyst to compare the way interviewers and respondents carried out the task." (Cicourel et al 2004: 8-9)

The problem, in its simplest form, is this: how can we be sure that the same question (containing the same lexicogrammatical and semantico-pragmatic pattern) has the same meaning for all the respondents we ask the question to? Cicourel himself, referring to the 'paraphrase problem', puts it as follows: "Is the format and content of a question commensurate with the way the information is organized in memory that would enable the respondent to answer?" (id.: 10). Cicourel's answer is negative, unless we ethnographically investigate this question; and for that to happen, we need to pay specific attention to the interactional structure of the research interview. The seemingly routinized and uniform question-answer sequences performed by survey researchers with their respondents hide a potentially debilitating interpretational diversity – this point has been made above.

But why? Cicourel formulated his concerns in the 1960s, more or less simultaneously to the birth and growth of sociolinguistics and linguistic anthropology, to which he himself substantially contributed. And from within sociolinguistics and linguistic anthropology, the details of the problem of the research interview gradually became clear: human interaction turned out to be governed by what is now called 'indexicality': delicate connections between talk and sociocultural context that are highly meaningful. The kind of meanings generated by indexicality broadly contain what is in more common parlance known as 'connotation': implicit but crucial structures of inference that are able to create entirely different meaning effects for the same lexicogrammatical and semantico-pragmatic patterns. Put simply: the same sentence can have an entirely different meaning uptake, depending on who, when, where and how it was formulated. Context is not just an influence and not just a dominant factor in the creation of meaning, it is a *determining* factor. The lexicogrammatical and semantico-pragmatic patterns in communication are directed by a *metapragmatic* pattern, which has its feet firmly in the sociocultural conditions – macro as well as micro and nano – under which such patterns are produced (see Hymes 1972; Gumperz 1982; Silverstein 1992, 2006; Rampton 1995; Blommaert 2005a, 2006; Agha 2007; Rampton & Blommaert 2011). In Cicourel's own formulation:

> "Indexical expressions imply either ambiguity or unstated elements of meaning. Their local comprehension, therefore, involves the interaction of prior or present compression of information. For example, phonology, syntax, semantics, pragmatics, prosody, mundane knowledge, and the perception of the social setting within which such expressions occur can all influence their meaning. Interview and survey questions invariably involve indexical expressions. The paraphrase problem, therefore, could include aspects of an expression's indexical properties. The notion of indexical expressions as used by ethnomethodologists assumes explicit reference to the respondent's local understanding of an expression, but to my knowledge does not refer explicitly to the organization of their memory of past experiences that might be relevant for understanding how a respondent might formulate or choose a fixed choice or open-ended answer to a question." (ibid)

Cicourel's student Charles Briggs synthesized and systematized the various issues raised by indexicality in research interviews in his classic *Learning How to Ask* (1986); still an indispensible book for anyone using interviews in research. Briggs' caution, strongly emphasized also by Cicourel, was that attempts to bring context 'under control' by, for instance, using white room environments for respondents, do not remove the problem because a white room is a context as much as any other context (quite an unnatural context as well), and various respondents can respond in very different ways to that context. Variability is *not a function of the context itself*, it is a function of *how individuals interact with contexts*. Using an identical context, thus, can lead to important insights in how individuals absorb and act upon such contexts; it does not however create a context in which meanings are no longer subject to variable indexical inferences.[2]

Several decades of systematic work on indexicality have led to an awareness that communication – *any* communication – contains a level of interpretational uncertainty because the indexical orientations displayed in communication are, in principle, not a priori detectable. Suggestions as to interpretational stability derived from the observation that identical utterances produce identical propositional meanings are dismissed because utterances contain vastly more than just propositional meanings; and if these nonpropositional, indexical meaning effects are not taken into account, any suggestion about interpretational stability is an expression of the researchers' wishful thinking but methodologically entirely unsustainable. Little firm belief can be given to research findings not based on this critical insight.

This insight naturally has the potential of removing any possibility of investigating large collectives of respondents. Indeed, in sociolinguistics and related branches of science, it gave rise to a rich tradition of micro-analysis of uniquely situated utterances or stretches thereof; much of contemporary discourse analysis instantiates this approach (see Blommaert 2005a for a discussion). If meaning is uncertain even at the level of the individual utterance, how can it be certain at the level of groups?

The answer to this, is *indexical order* (Silverstein 2003; Blommaert 2005a; Agha 2007). Indexicals appear not to be randomly generated – indexicality, thus, is not the cause of chaos in meaning. They occur in forms of order: dynamic and highly changeable forms of order, yes, but always order. The reason for this is relatively simple: we can only produce meanings, 'make sense' of what people including ourselves say, when there is a degree of *recognizability* in the signs we use, and this recognizability rests on conventionalization and ordering of indexicals into relatively perduring fields and patterns we call 'code', 'register', 'genre', 'intertextuality', 'entextualization' and so forth (see Agha 2007 for a survey).[3] Such forms of indexical order explain the fact that widely divergent groups of speakers – for instance, a highly

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[2] We can add a second warning here, formulated by Hymes (1966). Hymes points out that people very often are able to explicitly reproduce only specific, and severely limited, amounts of cultural material. Consequently, *asking* is not the best way to find out what there is to be found out: observation is indispensible.

[3] The 'ethnomethods' of Garfinkelian and Goffmannian ethnomethodology, to which Cicourel importantly contributed, also looked for locally ordered forms of making sense in everyday behavior. Cicourel later distanced himself from ethnomethodology and conversation analysis on grounds that it neglected the cognitive and psychological aspects of everyday practices.

international group of academics at a conference, using wildly different varieties of English – can still understand each other quite well. While every instance of communication is entirely unique, it still requires a level of recognizability, and hence collective sharedness, to become real communication. Recent work has shown that even in highly unstable and rapidly changing sociolinguistic environments, ad-hoc norms – agreements on the recognizability of communicative signs – emerge continuously and operate effectively in what, at first sight, may appear as a chaotic language context. This process of rapid norm development is called 'enregisterment': people continuously create communicative orders, communicative norms, even in entirely unstable environments (e.g. Silverstein 2003; Agha 2007; Moller et al 2011; Creese & Blackledge 2010; Rampton 2006; Blommaert 2012).

This means that there *is* a level of collectiveness to indexicals, that the 'paraphrase problem' can be brought, at least to some extent, under control, and that some degree of ecological validity may be achieved. It also means, however, that this can only happen if adequate investigation of such forms of order precedes the use of order as an assumption in research. Indexical order, in sum, cannot be taken as a given; one needs to go out and find out before it can be thus established. Ethnographic exploration of orders of indexicality are indispensible in any form of research that uses human interaction, *in any form*, in its research methods.

Combining ethnography and statistics: Bourdieu

Let us keep this in mind now, and turn to attempts to combine ethnography and survey work in coherent research designs. Let us also recall the fundamentally different orientation between both methodological traditions: Ethnography is concerned with validity and does not attempt to generalize over populations – it is not concerned with representativeness, in other words; statistical survey work is aimed exactly at representativeness and less at validity. When it comes to generalization, ethnography provides *qualitative* generalizations, i.e. *theoretical hypotheses* based on the systemic aspects of uniquely situated cases. Statistical approaches provide *quantitative* generalizations, i.e. calculated calibrations that speak to the entire population, *respective to a theory*. Both are valid forms of generalization – it is important to underscore this in a scientific universe in which increasingly only the second type of generalization looks respectable. A theoretical generalization is different in type from a qualitative generalization; the former is, as we know, dominated by the criteria of validity; the latter by criteria of representativeness. Misunderstandings over the notion of 'theory' tend to cause the confusion about generalizations: representativeness does not in itself constitute theory, it constitutes *proof* of theory. The line of production, so to speak, of theory itself is of a different order. Theory demands validity; its proof demands representativeness.

Pierre Bourdieu surely had a very sharp understanding of this. A point rarely picked up by his critics and followers, but perennially underscored in his own work, is the *ethnographic epistemology* that underpinned his work (Reay 2004; Wacquant 2004; Blommaert 2005b). Crucial theoretical concepts from his work – think of Habitus, Logic of Practice, and Field – are ethnographically grounded. That is, they are heuristic (and thus generalizable) notions derived from intense intersubjective engagement with respondents in real social environments (what Bourdieu always termed as the meeting of different Habituses).

Yet, Bourdieu used extensive surveys as the backbone of some of his most impressive work. *Distinction*, for instance, presents its readers with the results of a large-scale series of survey studies in which aspects of 'subjective' experiences of class structure were investigated. This survey-driven study is announced by Bourdieu as "a sort of ethnography of France" (1986: xi). Such statements in Bourdieu's work are not frivolous; they are fundamental methodological statements, and in order to understand this we need to listen to what he had to say about his general research approach. As said, he invariably started from ethnographic engagement in the field, where the confrontation of two socially grounded forms of embodied subjectivity (Habituses) provided the hypotheses to be statistically tested:

> "It was no doubt because I found myself in a situation where I could directly observe the disarray or the distress of economic agents devoid of the dispositions tacitly demanded by an economic order that for us is entirely familiar – in which, being an embodied and therefore naturalized social structure, they appear as self-evident, necessary and universal – that I was able to conceive of the idea of statistically analyzing the conditions of possibility of these historically constituted dispositions". (Bourdieu 2000: 18)

Slightly further, he again specifies the connection between ethnography and statistical analysis:

> "nothing had prepared me to understand the economy, especially my own, as a *system of embodied beliefs*, I had to learn, step by step, through ethnographic observation later corroborated by statistical analysis, the practical logic of the precapitalist economy, at the same time as I was trying as best as I could to figure out its grammar" (Id: 24)

It is important at this point to understand that Bourdieu started his academic life as an anthropologist – a science then dominated in France by Claude Levi-Strauss' version of structuralism. This structuralism, with its clear and abstract categorization patterns and lines of abstract generalization necessarily severing connections with experienced and empirical reality, clashed with Bourdieu's ethnographic experiences, in which he found confusion, contradiction and tension rather than the harmony of Levi-Straussian 'ethnology'. Generalization (the 'grammar' mentioned in the quote above), for him, should be *empirical*, not abstract, and reflecting the on-the-ground realities detected in ethnography. And such generalizations would be built by statistical work entirely grounded in ethnographic observation – the questions would be ethnography-based – and framed in an ethnographic epistemology, that is: an awareness that outcomes of statistical generalization needed to be fed back to the empirical on-the-ground realities from which they emerged.

This created a loop: ethnography-statistics-ethnography-statistics and so forth. And this loop explains the second major feature of Bourdieu's approach: he would return throughout his career to the same field sites for ethnographic follow-up work. This move *historicized* his work: the loop in which ethnographic material was tested statistically and then brought into a new ethnographic round of inquiry removed the synchronic bias of Levi-Straussian structuralism and made Bourdieu's object *dynamic*. His methodology, consequently, was one that addressed *change* rather than stasis. The acute historical awareness in Bourdieu's work is the second point, along

with his ethnographic epistemology, that shines through in almost every major theoretical statement made by him. Consider his definition of Habitus (Bourdieu 1990: 54):

> "the structures characterizing a determinate class of conditions of existence produce the structures of the habitus, which in their turn are the basis of the perception and appreciation of all subsequent experiences. The habitus, product of history, produces individual and collective practices – more history – in accordance with the schemes generated by history."

The ethnographic grounding of Bourdieu's approach removed the 'snapshot' (i.e. synchronic) quality from survey methodology and replaced it with a dynamic and change-oriented one.[4] Note that the dynamic theory, lodged in his central theoretical concepts, is generated by ethnography. Bourdieu moves from ethnographic generalization – his theory – to statistical generalization; the latter he qualifies as 'corroboration': statistical analysis enables him to grant his theory not just validity but also representativeness. His level of generalization is no longer, like in Levi-Strauss' 'ethnology', grounded in universalist abstractions; it is an empirical ('ethnographic') generalization, and this enables him to call *Distinction* and its many statistical data an "ethnography of France".

Towards an applicable instrument

Recall that Cicourel always insisted that he had nothing against statistics, and even found it a very important analytical tool, provided the statistics was applied to a field of which the validity had been tested. This validity resides in the commensurability of what is tested in surveys with what in local real conditions is experienced and articulated, how locally the world is constructed, so to speak. The solution offered by Bourdieu – a methodological loop in which ethnography and statistical survey work are continuously sequentially used to confirm or amend the findings – provides validity and enables representativeness. We thus get a two-pronged reality check, in which valid findings can be generalized towards representativeness, and in which this representativeness is in turn checked as to its validity in the field. The strengths and weaknesses of both approaches have now been blended into one approach, methodologically diverse but epistemologically unified, and tied together by a single object of which the various dimensions are recognized.[5]

Using this model as a template for further development and using the benefit of a vastly more refined understanding of ordered indexicals enabling new and more reliable forms of interviewing and questioning, we can start thinking of a concrete research instrument targeted at investigations of processes of rapid social and cultural change. The uncertainty and complexity characterizing such processes demands an approach which

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[4] The 'schemes' mentioned by Bourdieu fit into the category of 'genre', 'register' etc. mentioned earlier. In fact, it is on the basis of such notions, all referring to the partly systemic (structured) nature of human conduct, that ethnographic generalizations are made.

[5] The preciseness of this formulation is essential and deviates from many descriptions of what is commonly called 'mixed methods'. Methods are, in effect, not mixed but sequentially ordered; the epistemological direction is important, as well as the description of the object.

(a) invites and privileges longitudinal, or at least repeated inquiry;
(b) necessitates a continuous reality check of any insight and finding
(c) applied both to validity and representativeness, given the intensely dynamic nature of the object;
(d) uses ethnographic findings as (realistically) ecologically situated and (hypothetically) systemically meaningful features of a social system
(e) and statistical insights as pertaining to the scope and level of saturation of such features, thus credentialing the systemic nature of the findings.

Such an approach could be useful to determine the *vector of change* in complex and dynamic social systems. To be more precise: the dialectic between locally grounded findings, their scope and level of saturation might begin to tell us something about the way in which social and cultural change starts, develops and culminates over time within a given population. We know that in stochastic systems, change can begin in what is usually seen as 'exceptions' and 'deviations'. Such features are statistically insignificant; yet they can grow into the major transforming force in a system. Repeated statistical analysis can establish such patterns of developing change, elucidating its directions and scope, while repeated ethnographic inspection can establish the specific ways in which transformations in scope and level of saturation trigger qualitative changes in the feature.

To give a trivial example: changes in popular-cultural, say musical, trends start outside or in the margins of the music industry. Few people will know the music and the artists, and those who know it might see it as a revolutionary innovation in the field of music. When this 'new' music becomes popular, however, the revolutionary and innovative character of it will rub off, as artists and their music become mainstream. The growth in (statistically verifiable) scope here goes hand in hand with an (ethnographically verifiable) change in the place of this music in social and cultural contexts, a change in what it means in real life. The totality of this phenomenon cannot be comprehended by using just one angle – we need both.

This trivial example instantiates an elementary kind of social and cultural process, which is however occurring with astonishing speed and scope in contemporary societies; think of the explosive growth of online social media and gaming communities, the viral nature of seemingly innocuous memes on the internet, or the rapid demographic transitions in superdiverse neighborhoods.

We are well underway to such an approach. In the last five years we have built up much experience in analyzing fairly unstructured interviews so as to discover the discourse (language registers) individuals from various ethnic groups in South Africa use to describe themselves and others. These studies have yielded significant insights by combining a high ecological validity with statistical procedures in the analysis. We are currently using diary methods, again in South Africa, to get better insight with whom actors in a diverse environment interact on a daily basis and what are the topics of their interaction with members of the various groups (e.g. Cheung et al 2011; Valchev et al. 2013). The analyses of these data will allow us to develop "identity landscapes", which refer to the multiple identities people and highly diverse environments have and link these identities to their context of occurrence. Such studies can be easily extended to other environments, and combined with 'deep' ethnographies aimed at, for instance, linguistic landscape description. By repeating

such open-and questions across time periods, we will be able to unobtrusively gauge the shifting social context of immigrants and mainstream members. We will better understand whether they are differences in the groups with whom each interact. Understanding the open-ended answers of panel members requires much ethnographic knowledge of the respective groups. This knowledge is used in a close analysis of the responses, ethnographically described and analyzed using quantitative procedures (usually after tabulation and the use of statistical procedures for contingency tables). This combination of approaches has run smoothly in the teams we deployed until now. We can develop it further.

One final remark is in order. The analytic instrument we have in mind obviously involves teamwork, in which teams combining different methodological angles are formed around specific unified research objects. While it would be good if such teams were composed of people equally skilled in all the techniques deployed in the team, academic realists such as the present authors realize that this is not possible. It may not even be necessary to train people fully in both approaches, for what is needed is a *sensitivity* for different approaches and how they arrive at insights. We do not per se need ethnographers who are entirely versed in advanced statistical analysis; we need (to paraphrase Hymes) ethnographers who can *read* and *understand* statistical analysis; vice versa with people trained in survey research. [6]

As mentioned earlier: the crucial operational point is to establish an object of research that can be recognized as relevant to both. It is the presence of a shared object that establishes the 'indexical order', so to speak, in such heterogeneous teams. While this is not difficult work, it is often left aside, and with disastrous effects on communicability of findings and analytical punch. Intrinsically shared objects are abundant out there. What is needed is to get them in, discuss the different aspects that make up the complex nature of the objects, and draw an approach aimed at disentangling that complexity. Rapid social and cultural change is perhaps the most crucial feature of the social environments in which all of us dwell, and the most urgent thing to decode, understand and get used to. Analytically, it creates challenges; but the challenge is not just a euphemism for 'problem': it is also an invitation to think outside the box.

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[6] This also counts for *publishing*. A frequently expressed anxiety is that work within 'mixed methods' creates problems in publishing; given the tremendous pressure to publish, and publish in monodisciplinary key journals, this anxiety is understandable. There is, however, no compelling reason why *some* of the outcomes of joint work could not be routed towards monodisciplinary outlets while others are targeted at different audiences. In addition, innovative work done in-between disciplines invariably has innovative effects on the core disciplines, which is why cross-over work often acquires theoretical significance and relevance in specific disciplines.

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