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# Studying “Going Concerns”: Everett C. Hughes On Method

by Rick Helmes-Hayes

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What principles shall guide us in the discovery of men’s secrets; what, in the telling of them?

[Hughes 1971 [1956], 431]

## Introduction

In North America, during the middle decades of the Twentieth century, the work of Everett Hughes (1897-1983) was central to a wide range of disciplinary specialities, including race and ethnic relations, work and occupations, and education. Beginning in the early 1970s, he became subject to considerable critical attention from US scholars eager to examine his legacy [Baker 1976; Becker *et al.* 1968; Burns 1980; Coser 1994; Daniels 1972; Faught 1980; Fielding 2005; Heath 1984; Holmstrom 1984; Reinhartz 1995; Riesman 1983; Riesman and Becker 1984; Simpson 1972; Strauss 1996; Weiss 1997]. In Europe, by contrast, Hughes had no such profile. Only after his death in 1983, in the context of a growing, if belated, interest in the general legacy of the Chicago School [see Rémy and Voyé 1974; Grafmeyer and Joseph [eds.] 1979], did French and, now, Italian scholars begin to pay appreciative attention to his work [Hannerz 1983; Peneff 1984; Winkin 1988; Coulon 1992; *Sociétés Contemporaines* 27 [juillet] 1997, entire issue; Wax 2000; Chapoulie 2001]. Much of this attention grew out of an interest in Hughes’s contribution to the development of interpretive sociology and fieldwork [Chapoulie 1987; Chapoulie 1996a; Chapoulie 1996b, 11].<sup>1</sup>

*I presented an early version of this paper at the 25th annual Qualitative Analysis Conference at the University of New Brunswick, Fredericton, NB in May 2008. It is based in part on Helmes-Hayes [1998].*

<sup>1</sup> Some scholars would argue that Herbert Blumer was and is the most important figure in the development of the fieldwork tradition at Chicago. Platt disagrees. She grants that Blumer was responsible for formulating symbolic interactionist theory but notes that his work was conceptual. He “published very little empirical work himself,” she said, “so did not provide exemplars .... [H]e did not provide what are conventionally regarded as methods and that it was not clear how to translate his

## Purpose

The purpose of this paper is to outline Hughes's "methodological orientation," including his conception of fieldwork [Platt 1997; see also Platt 1996; Platt 1998; Chapoulie 1987; see also Chapoulie 1996ab; Chapoulie 2001], bearing in mind the mutually constitutive relationship in his work between theory and method and his conception of research ethics.

## Preliminary Observations

Hughes regarded the fostering of fieldwork as among his primary responsibilities and crowning achievements and expressed the wish that his students and colleagues would build on this aspect of his legacy.<sup>2</sup> It is surprising, then, that he never wrote a systematic overview of issues of epistemology, ontology and their relations to theory and method or, even, a practical "how-to" field manual [Hughes 1971, vi]. This does not mean that he was methodologically naïve. Quite the opposite. He was a key combatant in the heated battles that took place in American sociology beginning in the 1940s regarding the respective merits of quantitative/statistical and qualitative/ fieldwork methodologies. Like others of the period who refused to cede the field to advocates of neo-positivism, he was heir to a rich legacy of sociological and anthropological fieldwork – British cultural anthropology, the British and American social survey tradition, etc – to which he referred for inspiration, benchmarks, and advice. For his part, Hughes was especially well versed in the fieldwork-based classics of anthropology and sociology [Hughes 1974; Weaver 2002; Chapoulie 1987, 266].<sup>3</sup> As well, he was familiar with Vivian Palmer's *Field Studies in Sociology*, published at Chicago in 1928 and was a major motive force behind *Cases in Fieldwork* [Hughes, Junker, Gold, and Kittel 1952], a sourcebook which grew in part out of a fieldwork course he had initiated at Chicago in the late 1930s [Hughes 1971 [1960], 497-498; Winkin 1988, 39 n4, 40]. A subsequent book, Buford Junker's *Fieldwork* [1960], in which Hughes was deeply involved [1971 [1960]], was a further development along this line. It reflected the fact that after 1945 American fieldworkers were becoming more rigorous about their approach to observation, classification, and analysis of

system into data" [1995: 92; see also Blumer interview by Wiley 1982; cited Platt 1995: 92; Chapoulie 1987: 262, 290 n. 34; Fine 1995: 7].

<sup>2</sup> Late in his career, reflecting on his legacy, Hughes claimed that the fieldwork course he initiated and taught at Chicago beginning in the late 1930s [Hughes 1971 [1960], 497-498] was "a great thing," a major accomplishment which, in his view, had a significant impact on American sociology [Hughes interview by R. Weiss, n.d. ca 1980-1981].

<sup>3</sup> About Hughes's knowledge of anthropology, see Hughes 1974 and Weaver 2002.

data [see Chapoulie 1987, 269-271, 274-279; Chapoulie 1996b, 15-16; Platt 1996, 30].<sup>4</sup> So while Hughes never wrote a methodological treatise, there is “methods talk” scattered throughout his work and a reader can systematize it for herself [Chapoulie 1996b, 20]. It is a challenge to do so, though, for Hughes had a broad, eclectic conception of fieldwork not limited to *in situ* observation. A further difficulty is that his methodological orientation can be understood only by appreciating its relationship to his theoretical frame of reference: interpretive institutional ecology.<sup>5</sup> I have described this frame of reference in detail elsewhere [Helses-Hayes 1998] and offer a brief recapitulation of it here as a final prolegomenon to a description of his methodological orientation.<sup>6</sup>

### Interpretive Institutional Ecology

“Interpretive institutional ecology” is a dualistic and multi-level approach which combines a microsociology rooted in a type of *interactionism* with a meso-/macro-sociology rooted in anthropological functionalism and *human ecology* [see 1936, 1939, 1946, 1957a, 1969]. The microsociological aspect draws on Simmel’s formalism, Weber’s work, and a set of non-Blumerian interactionist sensibilities which focuses on settings and situations. The “setting” for Hughes’s analysis was usually an “institution” [e.g., Hughes 1931; Becker *et al.* 1961; Becker, Geer and Hughes 1968], an instance of “formally established aspects of collective group behaviour” [Hughes 1957a, 227]. Indeed, he once defined sociology as “a science of institutions” [Hughes 1971 [1942], 15] and it has been remarked that his work set the institutionalist side of the agenda in the Second Chicago School [see Short 1971, xxvi; Simpson 1972; Faught 1980].

“Institutional settings” operated at three seamlessly connected levels of “social interaction”<sup>7</sup>: micro, meso, and macro. People engaged in social interaction – sometimes face-to-face, sometimes indirectly – in immediate institutional settings (a factory, a hospital) or within institutionalized social relationships (boss/worker, doctor/patient) as they constructed selves, defined situations, pursued careers, and struggled to construct and reconstruct institutions in an effort to make them responsive to their

<sup>4</sup> The relatively late appearance of such materials is discussed in Gobo 2005, 2-4; see also Winkin [1988, 40]; Fielding [2005, 3]; Platt [1996].

<sup>5</sup> Others who share my view that Hughes’s work contains a relatively comprehensive theoretical approach include Simpson [1972], Strauss [1996, 281-282] and Emerson [1997m 40, 47]. For the opposite view, see Chapoulie 1987 and Chapoulie 1996ab].

<sup>6</sup> For a summary of Hughes’s theoretical frame of reference, see Helmes-Hayes [2005].

<sup>7</sup> Hughes [1971 [1956b], 508] defined “social interaction” in very general terms; for him it was “the subject matter of sociology.”

often conflicting needs and desires. Chapoulie has correctly remarked on this count that in the course of carrying out such analyses Hughes coined several “abstract” and “general,” but not “global” theoretical concepts – restriction of production, career line, dirty work, etc. – useful at the micro- and mesosociological levels of analysis. But Hughes’s approach was broader than this because, in his view, the dynamics of micro- and meso-settings – including the unfolding “careers” of individuals or occupations – were often largely determined by forms of social interaction – macrosociological processes and institutional systems such as demographic changes and the division of labour – that also needed to be understood. Hughes attempted to describe and explain the workings of these processes and systems using what he referred to as an *ecological* framework [Simpson 1972, 548-554; Chinoy 1972, 561; Faught 1980, 75; Burns 1980; Strauss 1996, 272].<sup>8</sup>

This ecological framework had two purposes: 1) to describe and explain typical mesosociological social processes operational at the level of *single institutions* [e.g., Hughes 1931, 2, 6-8; Hughes 1939, 304-309; Hughes 1957a, 232-247; Hughes 1969, 147-153; see also Burns 1980, espec. 349-352]; and, less centrally, 2) to capture the dynamics of phenomena such as industrialization and colonialism in terms of the logic of *interinstitutional relations* at the level of the “social system” [Hughes 1933; Hughes 1971 [1935]; Hughes 1938ab; Hughes 1971 [1951a], 323], or what we now refer to as macrosociology [Hughes 1939, 289-295; Hughes 1957a, 248-255, 267-280 *passim*; Hughes 1958; Hughes 1969, 130-137; see Simpson 1972; Burns 1980; Chapoulie 1996b, 9, 21; Helmes-Hayes 2000].

At all three levels of analysis he portrayed social interaction in terms of institutions as “going concerns” (“a favourite phrase of his,” according to Strauss [1996, 272]) struggling to survive in an ecological setting. Indeed, the idea of the institution as a going concern (sometimes referred to as an “enterprise” [e.g. Hughes 1957a, 227]) was the conceptual vehicle which united the three levels of interpretive institutional ecology. He highlighted this fact in the preface to *The Sociological Eye*.

In any society there are certain mobilizations of people for expression or action. They are “going concerns” (...) If we are to study human society, we must attend to the going concerns which are subject to moral, social, and ecological contingencies. It is thus that institutions are discussed in these papers as enterprises [Hughes 1971,

<sup>8</sup> It is important to appreciate that Hughes understood his work as falling in the ecological tradition, as the following comment, written in 1977, reveals: “I think I can rightly claim to have trod the *ecological* path from my graduate school days until now” [ECH Papers, BC, Box 5, File: “Memorandum on Possible Lecture at the University of Tennessee, Knoxville”; 8 March 1977, p. 5; emphasis added].

viii; see also ECH Papers, UCHI, Box 77, folder 1; Hughes interview by R. Weiss, n.d. [1980-1].

The dualistic character of interpretive institutional ecology can be seen as well in Hughes's (admittedly infrequent) remarks regarding the metaphysics of theory and method. He was well aware of the fundamental ontological and epistemological differences between scientific and interpretive/ constructionist approaches [Hughes 1971 [1962a], 457] but rather than choosing between interpretivism and science, drew on both as need required and tried in so doing to combine them. His interactionist sensibilities sensitized him to the processual and constructed nature of social reality and led him to have a deep appreciation for the importance of language and meaning construction. Nonetheless, he rejected both the ontology and the epistemology of a strictly constructionist approach. He likewise rejected the related notion that sociology's main purpose was to ferret out the subjective meanings that individuals and communities attached to their gestures and actions and to describe how they came to construct and share such definitions [Daniels 1972, 402, 407]. In Hughes's view, there was an objective, if changing, social structure 'out there' and he was quite prepared to talk in Durkheimian terms about "social facts which gather themselves into wholes changing and moving according to rules of their own" [Hughes 1931, Preface].

Following in this scientific/ structuralist vein, he insisted that you could make objective truth claims despite the existence of multiple, competing definitions of the situation [see Riesman and Becker 1984, x]. However, while insisting that human behaviour was patterned and somewhat predictable, and that one could make objective truth claims, he rejected the scientific doctrine of determinism. In a faculty seminar in 1951, Hughes, along with some of his likeminded colleagues, argued that prediction was and would always be "a matter of probability – of approximation – rather than any kind of certainty" [EWB Papers, UCHI, Box 33, Folder 3, Document: "Minutes, Faculty Seminar, Department of Sociology, 8 November 1951, p. xx; see also Hughes 1971 [1959a], 453]. Allied with this view were two others. One was a rejection of the 'reflectionist' theory of truth. "Any model," he argued, "is inevitably an abstract and partial account of reality." The other was his acceptance of the tentative nature of truth claims. "We are not seeking absolute truth," he claimed. "We are engaged in an enterprise to understand the world of man and this is a changing and moving world" [ECH Papers, BC, Box 5, file: "Memorandum on Possible Lecture at the University of Tennessee, Knoxville"; 8 March 1977; see also Weiss 1997, 546].

So for Hughes sociology was neither a social science in search of nomothetic laws nor an exercise in ideographic description and interpretation. On the question

of the possibility of generating “universal propositions” as opposed to “historical knowledge,” he wrote, “most of social science will fall in between the two poles of universal generalization and historical investigation” [EWB Papers, UCHI, Box 33, Folder 3, Document: “Minutes, Faculty Seminar, Department of Sociology, November 8, 1951, p. 11; see also Hughes, 1971 [1952], 299-301]. And he held two further methodological views consistent with this sense of the discipline. First, sociology would remain more craft than science [see Chapoulie 1996b, 23]. Second, when making methodological (and theoretical) decisions, “demonstrated utility” rather than scientific or interpretive “theoretical/methodological purity” was his guide. “We’ll have to use whatever methods we have to [in order to ...] understand the ongoing process,” he said. “You have to have devices for finding out where the hell the action is (...) And this is going to lead you into all sorts of methods. But you’ve got to suit the methods to [the problem]” [Lofland 1980, 276-277]. There is a price to be paid for choosing utility over purity, of course, and it is possible to argue that Hughes is guilty of a series of sins related to ontological and epistemological inconsistency and incommensurability.<sup>9</sup> In that sense, his sociology is messier than sociological approaches that proceed on the basis of interpretive or scientific theoretical/methodological purity, but it was a price he willingly paid, for he had no faith that either pure science or pure interpretivism was up to the task of understanding the human condition.

## **The Craft of Sociology: Eleven Principles**

In the pages to follow, I describe Hughes’s methodological orientation in terms of eleven interrelated principles. These principles are deeply intertwined, making it impossible to discuss them one at a time. Thus, where necessary and feasible, I have grouped them.

### *On Theoretical- Methodological Unity and Reflexivity: Principle 1*

*Principle 1:* Theory and method are co-constitutive, mirror images of one another, and reflexive.

Hughes’s theoretical frame of reference and methodological orientation are mirror images of one another and must be seen as a unity. Both are multi-sided, multi-levelled, and flexible. This makes them elusive and difficult to formalize. To obtain closure on either is impossible and, Hughes would say, undesirable. In every respect,

<sup>9</sup> In this respect Hughes followed in the pragmatist philosophical tradition so widely employed at Chicago.

they are just like the reality they seek to illuminate; that is to say, they are “going concerns.”

The unity and reflexivity of Hughes’s theoretical and methodological perspective is reflected in his conception of the research act. Though he does not phrase it in this way, I would argue that for him research is a form (indeed, a set of forms) of institutionalized social interaction. Therefore, all research projects are “going concerns.” As sociologists carry out their research “enterprises,” they must adapt to the ecological and institutional context within which they are studying, developing lines of action, constructing the meanings of their actions, etc, while simultaneously trying to understand the actions and the meanings of the actions of those they are studying.

This unity and reflexivity carries over into yet another aspect of Hughes’s view of the nature of sociology as a multifarious kind of human action and interaction; that is, sociology is a human enterprise which has an unavoidably political character. To illustrate: When we think about the nature of sociological research from Hughes’s perspective on the sociology of work, it is clear that research is a form of work carried out by a specific occupational group with a “licence” and “mandate” to study social relations [Hughes 1971; Hughes 1939b; Hughes 1959a; Hughes 1965]. This means that among other things sociologists need constantly to bear in mind that their work is part of – rather than separate from, or above – a societal moral division of labour. As they do their research, they must negotiate and fulfill a series of moral bargains among themselves and with those who gave them the mandate and licence to carry out their work, including those they are studying. As they do so, they must weigh not only the scholarly purposes they have in mind and the investigative techniques they want to employ, but also the political responsibilities they bear as a consequence of having the freedom to do research and report their findings. That is, as they attempt to understand other human beings they must do so within the limitations imposed by theory, method and scholarly-political moral boundaries. I deal with this last-mentioned issue in my description below of Principles 10 and 11.

### *Theory, Method and the Research Setting: Principles 2 and 3*

*Principle 2:* Theory and method are most fruitfully developed in the process of the direct empirical examination of real-life settings.

*Principle 3:* It is essential to bring both one’s theoretical “frame of reference” and one’s “methodological orientation” into every research setting so that one’s “sociological eye” is neither theoretically nor methodologically naïve.

Theory was seldom intrusive in Hughes's writing for, even if explicit, it was always applied lightly and flexibly and flowed as much from the data as to it. Indeed, according to Robert Weiss, Hughes's starting point was always an intimate familiarity with the relevant data to hand. "Everett believed strongly that observation preceded theory. You began with a problem, a concern, or awareness, and then you turned to reality and asked what was happening there. Once you knew something about real events, you could look for explanations. Your search for explanation would then make for a theory that was trustworthy and relevant, because it was based on reality" [Weiss 1997, 543-544; see also Platt 1996, 121]. This what Hughes's student Lynda Holmstrom was referring to when she noted that, for him, facts were "sacrosanct," the starting place for all other forms of discussion. One of Hughes's favourite aphorisms was, she recalled: "You answer questions in fact, not theory" [Hughes 1984, 474].

But this view is somewhat misleading and must be juxtaposed to another facet of his approach, for Hughes claimed explicitly that when engaged in the process of empirical investigation his thinking was always "guided but not hampered by a [theoretical] frame of reference internalized not quite into the unconscious" [Hughes 1971, vi]. If when one entered a research setting one had to have an *open mind*, then one also had to have a set of concepts *in mind* to help provide "insight" [Strauss 1996, 272] that would guide one's research and thinking.<sup>10</sup> As well, Hughes insisted that an investigator's interest in a particular setting or problem should be stimulated by what he called the "more-so principle." "While any society at any time is of interest, any one at a given time may show some features of special interest. It may be, because of a combination of circumstance, the ideal laboratory in which to observe certain processes which will give us new knowledge of general interest" [Hughes 1971 [1959a], 454; see also Hughes 1971 [1956a], 441; Faught 1980, 77]. Two related concepts, "marginality" and "emancipation," were equally central. "Good sociology," he wrote, "is always a marginal phenomenon (...) Marginality accepted in an adventurous spirit is the making of a sociologist" [Hughes 1971 [1957b], 529; see also Weiss 1997, 548-551; Strauss 1996, 273-274]. This was true in a double sense. First, social observation was best accomplished by someone marginal or "foreign" to the setting being studied. The outsider could see things that insiders missed – or, at least, could see them in a new light [Hughes 1971 [1956a], 434-435] – because he did not take them for granted. Second, while the sociologist had to get close enough to his subjects to become a "companion" and "confidante," so that he could

<sup>10</sup> Some scholars claim that in terms of modern-day sociological practice, probably the closest parallel to Hughes's style is the "grounded theory" approach of Barney Glaser and Anselm Strauss [Glaser and Strauss 1967; Weiss 1997, 544; Strauss 1996, 282; Demazière and Dubar 1997, 51-53; Emerson 1997, 42-43, 47].

tap into knowledge that only insiders could provide, he had to keep an appropriate “social distance” in order to maintain his professional identity and integrity and refrain from “going native” [Hughes 1971 [1957b], 528; Hughes 1974, 332; see also Hughes 1971 [1956a], 434-436 passim; Hughes 1971 [1960], 502-503; Chapoulie 1987, 275]. The benefits of successfully pulling this off, said Hughes, were two-fold. First, the observer would be “emancipated,” made “free of the restrictions of their background” [Hughes 1971 [1970b], 573] that prevented her from appreciating that social arrangements she regarded as natural, fixed or inevitable actually “could have been otherwise” [Hughes 1971 [1959b], 552]. In the best case scenario, Hughes said, this would occur without her becoming “alienated” from her personal background [Hughes 1971 [1970b], 573; see also Hughes 1971 [1970a], 419-420; Riesman and Becker 1983, ix; Strauss 1996, 276]. And this individual transformation would then have a domino effect. As the observer ruminated on the significance of her observations and analysis, she might decide that the sociological theories and/or methods that had originally guided her thinking, questioning, observing, and reporting needed to be changed and would set about doing so.

So, despite Weiss’s pronouncement that Hughes gave primacy to observation, Hughes brought with him to every empirical setting a flexible theoretical sensitivity. This framework was animated and guided by what he referred to as “free association”: i.e. “intense observation” followed by creative thinking (a “turning of the wheels”) drawing on his extensive background of empirical data [see Hughes 1971, vi; Hughes 1971 [1970b], 571].

That one’s “sociological eye” was to be theoretically open, educated, and flexible had methodological implications. One had to be trained to be able to “see” a wide variety of orders of data at multiple levels of social reality. This meant one had to be familiar with a wide range of research techniques.

### *On “Fieldwork”: Principles 4, 5, 6, and 7*

*Principle 4:* Social realities (interaction/processes) exist on many levels: macro, meso, micro. Methods must allow access to each type/level of reality. Many methodological techniques are necessary for examining reality.

*Principle 5:* Notwithstanding principle 4, *in situ* observation is the best and most favoured investigative technique.

*Principle 6:* Never hypostatize method and technique.

*Principle 7:* Though many “entry points” and units of analysis are useful, the institution is most fruitful among them.

One of the most frequently quoted passages from Hughes's work comes from his introduction to Junker's volume, *Fieldwork*: "Field work," he wrote, "is not merely one among several methods of social study, but is paramount." He readily granted that other research techniques were useful and necessary, but it was his view that it was when one undertook fieldwork that "the real learning began." In his estimation, it was the "small observations (...) accumulated" in the process of *in situ* observation that provided "the evidence on which theories of culture and society [were] built" [Hughes 1971 [1960], 497-498]. Thus, *in situ* observation was the centrepiece a fieldwork-based methodology that would produce the best sociology.

Given this was his view, it is no surprise that for years Hughes taught field work courses in which *in situ* observation – "the observation of people (...) where they are, staying with them in some role which, while agreeable to them, will allow both intimate observation of certain parts of their behavior, and reporting it in ways useful to social science but not harmful to those observed" [Hughes 1971 [1960], 496] – was primary. But a few words of caution are necessary. There is no question that Hughes regarded direct, "on the hoof" observation as essential [*ibidem*, 504; see also Hughes 1971 [1959c], 284] and, likewise, in good interactionist fashion, saw part of the point of that exercise as "deep understanding" [Strauss 1996, 277], the revelation of the meanings of those activities for those involved [Hughes 1971 [1956b], 508; Chapoulie 1987, 264]. However, his conception of fieldwork was not confined to *in situ* observation and he was not concerned solely with trying to collect/construct data about patterns of face-to-face social interaction or to find and report the meanings and interpretations of reality developed by individuals or small groups in particular settings. Instead, fieldwork for him involved a variety of research techniques chosen in situation-specific combinations to achieve a wide range of additional purposes. For instance, according to Hughes, one goal point of comparative investigation at multiple sites of observation, was to allow the sociologist to "step outside" the realities constructed by individuals and groups. Only in this way could one move beyond their partial and perspectival definitions of the situation to a wider, deeper understanding of settings, processes, or events [Chapoulie 1987, 272-273, 277-278; Demazière and Dubar 1997, 51; Daniels 1972, 402; Verdet 1997, 63]. Likewise, only in this way could the investigator get on with the business of discovering the structure and dynamics of recurring or typical forms and processes of social action and interaction at the micro and meso levels of social reality: the typical life stages of an institution, the typical characteristics of an occupation, the typical career of a medical student [Hughes 1971, viii; see also Rock 1979, 174-175; Faught 1980, 76-77; Holmstrom 1984, 472, 473-474, 479-480; Chapoulie 1996b, 14].

But there was more still. Fieldwork might also involve, as it did in *French Canada in Transition* [Hughes 1943] and its satellite publications [see, e.g., Hughes and McDonald 1971 [1941]], the study of *macrosociological* phenomena – the division of labour, capitalist relations of production, patterns of property ownership, etc. Such phenomena could not be understood solely via *in situ* observation but required a multi-method strategy designed to produce various forms of macrosociological data. In *French Canada in Transition*, for example, he supplemented *in situ* observation and interviews with ecological mapping, data regarding the history and population of the town, and data about patterns of corporate ownership in Cantonville, Montreal and Quebec [Hughes and McDonald 1941; Roy 1935]. Some of his later essays and book chapters, while very different in form from *French Canada in Transition*, focus on the same macrosociological level of analysis, take the same long historical view and comparative perspective, and draw on a wide variety of data constructed/gathered using a range of research techniques [Hughes 1971 [1952; 1955; 1956ab; 1960]; Hughes 1939; Hughes 1946; Hughes 1957a].

So, if Hughes preferred *in situ* field work as opposed to other techniques, he remained methodologically eclectic [Chapoulie 1987, 262, 264, 272-278; Strauss 1996, 272]. Fieldwork was for him “paramount,” but he was, in his own words, “suspicious of any method said to be the one and only” [Hughes 1971, ix] and advocated the use of a range of research techniques designed to “mine” data of different kinds. “The social science of today requires, in fact, a great many arts of observation and analysis. Field observation is one of them” [Hughes 1971 [1960], 502].

That Hughes held this positive evaluation of what we now refer to as mixed methods and insisted on drawing links between micro and macro levels of analysis explains his remark in the Introduction to Junker’s fieldwork text that *there are no hard and fast rules to apply*. “[T]he situations and circumstances in which field observation (...) is done are so various that *no manual of detailed rules would serve*” [Hughes 1971 [1960], 503; emphasis added]. This is the case for two reasons. First, the social phenomena the researcher sets out to understand and the settings in which she will find herself are likely to be fluid, complex, and potentially novel to her. Thus, she is better equipped with a flexible theoretical frame of reference and an eclectic methodological orientation than with a formal theory to “test” and a set of textbook-determined procedures to “apply.” This meant, second, that one could become an accomplished fieldworker only through practice. As Nigel Fielding [2005, 2] put it, the general feeling in the Chicago department was that field work “could be learned but not taught.” This explains why Eliot Freidson, Ed Gross, and Paule Verdet recall receiving very little formal, conceptual instruction from Hughes before being sent into the field [Freidson interview by Platt, cited in Platt 1995, 94; see also

Gross interview by Platt, cited *ibidem*; Verdet 1997, 61-62].<sup>11</sup> However, the fact that Hughes taught a fieldwork course meant that he regarded sensitization and training as useful and made sure that students were not naïve when they ventured out into the community.<sup>12</sup>

In order to do field work, one had to have a point of entry and Hughes's preferred – though by no means exclusive – point of entry was the institution [see Simpson 1972, 558].<sup>13</sup> The institution, he said, was "[f]or certain purposes (...) the most fruitful unit of investigation" [Hughes 1931, 111]. He had theoretical and practical reasons for his choice. Recall that Hughes was *not* an interactionist interested in macrosociology, but an *ecologist* sensitive to the importance of selves, meaning construction, and the like. Theoretically, then, the institution was the most fruitful unit of *ecological* analysis, where ecological was defined broadly to incorporate on the one hand processes of institutional development, change and interinstitutional relations (struggles among institutions to survive in an ecological setting) and on the other the processual development the self, the negotiation of meaning and the unfolding of careers (within particular institutions). To give an example: if you want to understand the character and significance of work in modern capitalist society (or, better yet, across societies and epochs), you start by identifying an institution – a family dwelling, a church, a brothel – where work takes place. You become familiar with the "goings-on" in that "going concern" and then move 'down' to the micro-level of analysis to study the character and impact of work on selves and groups in that milieu and 'up' to the level of macrosociology to study the social system – the division of labour, gender relations, class, colonialism, etc. – that produced that particular institutionalized form of work and within which that "going concern" had to survive.

<sup>11</sup> The course description in 1947 contained no works about the participant observation method and the term was not used, though examples of such studies were listed Platt [1995, 104, n. 7].

<sup>12</sup> Hughes's broad conception of fieldwork manifested itself in the content of his fieldwork course at Chicago which, he noted, "did not change greatly over the years." Students were assigned a census tract and had to visit the library to locate relevant census data. This was followed by 'street time' – mapping the neighbourhood, observing formal and informal gatherings – as well as structured time with informants – doing interviews, gathering family histories, etc. This was intended to make students intimately familiar with the individuals, groups, and institutions in the area [Hughes 1971 [1960], 498; see also Hughes interview by R. Weiss, n.d. ca 1981; Platt 1995, 94, 104, n.7; Verdet 1997, 62]. To work with census materials students had to be literate in statistics and Hughes even taught statistics at one point in his career [Becker *et al* 1968, vii].

<sup>13</sup> Hughes's claim that the institution is the most productive unit of analysis for sociology may be found not only in his published writings [e.g. Hughes 1939b; Hughes 1971 [1942]; Hughes 1946; Hughes 1957a; Hughes 1969], but also in unpublished documents, course notes, and correspondence.

### *Principle 8*

*Principle 8:* Comparison is the basis of sociological insight.

Hughes almost always drew on comparative materials – occupation to occupation, institution to institution, time to time, country to country – as the groundwork for theoretical analysis. Such theoretical analysis, woven into almost all his writings, required a comparative method, one which, in his hands, often drew on unexpected points of reference. Vis-a-vis the sociology of work, for example, he wrote: “The comparative student of man’s work learns about doctors by studying plumbers and about prostitutes by studying psychiatrists” [Hughes 1971 [1951a], 316]. He advocated this strategy because he believed that “the essential problems of men at work are the same” regardless of the type and status of their work. “Until we can find a point of view and concepts which will enable us to make comparisons between the junk peddler and the professor without intent to debunk the one and patronize the other,” he said, “we cannot do our best work in this field” [Hughes 1971 [1951b], 342].

In Hughes’s estimation, this comparative orientation had multiple benefits. First, it forced the investigator to see his work in “dialectical” terms, balancing efforts to search for knowledge about “the timely” while pondering the significance of such “news” in terms of “the timeless”; i.e. “general, abstract,” theoretical understanding [Hughes 1971 [1959a], 452, 454; see also Hughes 1971 [1956a], 440]. The two endeavours were complementary and symbiotic [Hughes 1971 [1970a], 420; see also Hughes 1971, vi-vii; Becker *et al.* 1968, x]. The second benefit of the comparative method was that it facilitated going back and forth between disciplinary specialties such as work and occupations and what he called “general sociology.” “[A] good sociological generalization,” he declared “(...) fits a great variety of social phenomena: (...) monastic orders, vice rings, banks, and professional societies.” Such generalizations, he continued, came from “the observation, description, and comparison of many actual organizations or situations where people are in interaction. Sociological generalizations come *from* the special or applied sociologies as well as being applied *to* them” [Hughes 1971 [1957b], 525]. And there was a third benefit of the comparative approach. Comparison allowed the researcher to add complexity to her analysis by “mov[ing] from level to level” in order to, for example, describe and explain macro phenomena such as “the growth of cities [... and] the problems of industrialization” while simultaneously examining meso and micro phenomena such as “the vicissitudes of careers” [Becker *et al.* 1968, x] and the development of selves. On the last-mentioned, Ed Gross recalled the following quotation from Hughes: “If you want to understand anything about a man, you ask him what is his work. What does he

do for a living? What you will learn will explain much of how he feels, much of how he thinks, and all of his obituary" [E. Gross interview, 17 November 1995].

This comparative principle is intimately tied to a ninth principle.

### *Principle 9*

*Principle 9:* Investigators should ignore boundaries of time, space, and discipline as they seek to understand social reality.

Sociology is not, in its logical essence, the study of the contemporary rather than of the past, of what is close rather than what is far away and exotic. Nor has it, by its logical nature, more to do with one set of institutions, one aspect of social life, or any specific content than with others. If a theory of society comes to be, it will be valid only insofar as it accounts for the societies of the past as well as of the present, for what is exotic to our culture as well as what is part of it, and insofar as it applies to one content, institution, and phase of life as well as to others [Hughes 1971 [1957b], 524; see also Hughes 1971 [1959c], 283; Hughes 1971 [1970a], 420].

Throughout his career, this meant drawing on the reported findings of research conducted by the economist, the demographer, the anthropologist, the historian – even the survey researcher. And while he was quick to point out the methodological shortcomings and pitfalls of using data gathered/constructed using survey and other methods [Hughes 1971 [1956b], 507; Hughes 1971 [1961], 476; Hughes 1971 [1962c], 71; Hughes 1971 [1964], 160-161], he nonetheless thought them useful and employed them in good faith on a routine basis [Hughes 1971 [1959c], 284; Hughes 1971 [1959a], 453-454; see also Heath 1984, 222-223; Chapoulie 1987, 272-273]. In his view, the problem or question should determine the method rather than allowing methods to limit the kinds of issues it was possible or appropriate to investigate. Instead of defining some issue as beyond the pale of sociology because it could not be studied using in situ observation – and explaining away “structure,” “cause,” “power” and the like with vague talk of “negotiation” and “meaning construction” – Hughes simply employed the research techniques of the macrosociologist to frame his analysis of issues such as class, industrialization, race relations, colonialism, etc. [Chapoulie 1987, 272-273, 1996b: 20].

This mention of issues such as class, race relations, and colonialism raises the question of Hughes’s views on the relationship between sociology and politics – questions of objectivity and value freedom, personal and professional ethics, and so forth. Such issues have had an especially high profile in the discipline for the past five years because of the imbroglio created by Michael Burawoy’s ongoing advocacy of a moral-

ly laden public sociology [Burawoy 2005], but Hughes was highly sensitive to them throughout his career and wrote a good deal about the ethics of doing sociology. Principles 10 and 11 deal with these issues.

### *Sociology and Politics: Principles 10 and 11*

*Principle 10:* In so far as it is possible, the sociologist must endeavour to remain “neutral.”

One aspect of this principle – the notion of objectivity – is easy to state: in the gathering/construction of data, one must try to remove all forms of “bias.” In a technical sense, this is a futile endeavour, as Hughes well knew, because it is impossible to live without some kinds of preconceptions. The research act, as a form of human activity, is not immune to the pitfalls of selective perception and conception. Nonetheless, the attempt to remain neutral in this sense is a worthy and necessary objective and Hughes preached the virtue of this practice using such terms as “objectivity,” “detachment,” and “disinterestedness” [Hughes 1971 [1962a], 461; Hughes 1971 [1954], 469]. But there is a second, related meaning of the concept of neutrality, the idea of value-freedom or value neutrality, to which Hughes also seemed to profess allegiance. But value-freedom – the doctrine that it is both possible and appropriate for the investigator to refrain from offering moral judgements on his or her research findings – is a much more difficult and complex problem and, despite appearances to the contrary, Hughes actually rejected it in its classic formulation.

To understand his position it is necessary to appreciate his typical, Enlightenment-inspired view of the societal value of research and the knowledge it generates; i.e. the knowledge produced by research, combined with the informed discussion it generates, constitute a societal “good.” Knowledge is better than ignorance and, thus, research might contribute via informed dialogue to social betterment [Hughes 1974, 331]. In Strauss’s words, Hughes wanted to “bring informed, enlightened understanding of the world to those who would listen” [Strauss 1996, 274]. Beyond this broad endorsement, however, he seemed leery of purposeful “do-gooding.” In fact, by all appearances, he apparently championed the typically professional (scientific) view that sociology should be value neutral. Indeed, on this count, David Riesman refers to him as “the abiding neutral, dispassionate but not uncritical” [Riesman 1983, 477; see also Riesman and Becker 1971, vi; Strauss 1996, 272, 274]. And that is certainly the unambiguous message in the following passage from his preface to *The Sociological Eye*: “Some say that sociology is a normative science. If they mean that

social norms are one of its main objects of study, I agree. If they mean anything else, I do not agree" [Hughes 1971, xviii; see Chapoulie 1996b, 23].

But he did not always follow this admonition in practice. Riesman and Becker note that while an "abiding neutral," he believed strongly in the "unfettered freedom of intellectual inquiry" [Hughes 1971, xiv]. Hughes discussed the meaning and practical application of the freedom of inquiry in a series of essays written in the late 1950s and early 1960s. In "The Dual Mandate of Social Science" he argued that the scholar had the right, in fact, a duty, to use the privilege of academic freedom fearlessly, to define problems and frame questions that might lead him to "barge in where the archangels of the academic world would have shuddered to think of treading" [Hughes 1971 [1959a], 446; see also Hughes 1971 [1962a], 460]. He explicated this notion further in "The Improper Study of Man." The scholar, he wrote, should "study man and his institutions with broad-sweeping curiosity [... and] the sharpest tools of observation and analysis which [she] can devise" [Hughes 1971 [1956a], 442]. In doing so, he wrote, she should retain a "loyalty to truth" that compelled her to find and report the truth, no matter how greatly it differed from the conventional wisdom and no matter how much it threatened the status quo. As he put it in "*The Academic Mind: A Review*," the social scientist had to have "the right to enough intellectual elbow room among sacred arrangements to do his work [... He] must have freedom to entertain – at least for comparative and analytic purposes – all the forbidden thoughts" [Hughes 1971 [1959b], 552; Hughes 1971 [1963], 494-495]. Hughes's student Arlene Daniels [1972] has sagely captured this idea in the very Hughesian notion of the "irreverent eye." Arrangements that some members of society might regard as "sacred" – natural, inevitable, fixed – might well be revealed by the "irreverent eye" to be nothing of the kind. Likewise, things that some people might regard as inherently "good," beyond investigation or reproach, or things some people might prefer remain hidden, ignored, or undiscussed, could and should be brought out into the open.

With this licence and mandate came an obligation. Freedom of intellectual inquiry could be exercised only within the limits of a fair and transparent bargain which, at a minimum, required the investigator to be highly tolerant, respectful of a broad range of beliefs, customs, and social arrangements, some of which he might find objectionable [see Strauss 1996, 278]. As a case in point, Riesman and Becker point to Hughes's attitude toward members of the upper class. When Hughes studied them, Riesman and Becker said, he did so "not to debunk or unmask [them], but to understand [them]." Hughes, they claimed, wanted to be "comprehending" rather than "self-righteous." The result? His writings were "ruminative" rather than "indignant"; "free associations on a theme" rather than "sermons" [Hughes 1971, xiii, vi, vii; see

also Riesman 1983, 477-478; Strauss 1996, 278]. Put another way: the scholar was to do her work while striving simultaneously to do no harm to those being investigated.

But I would argue that while Hughes was generally tolerant and openminded and certainly meant no harm to those he studied, he nonetheless rejected an unqualified version of the doctrine of value neutrality. He was too sensitive to inequality, oppression and injustice to adopt such a view. “While playing the role of the timeless and disinterested outsider is an important item in the repertoire of the social scientist,” he wrote, “it is not the whole of it. Our role requires also intense curiosity and *personal concern* about the people and problems studied” [Hughes 1971 [1954], 469; emphasis added; see also Becker *et al.* 1968, viii]. That Hughes expressed the need for a degree of “personal concern” is perhaps the best way of introducing Principle 11 and my discussion of his reluctant and cautious but firm rejection of the “strong” version of value neutrality.

*Principle 11: All human beings are equal.*

Hughes stated this “equality principle” forthrightly in “*The Improper Study of Man*” where he rejected the “fallacy” held by some members of society that “some people and peoples are more human than others” [Hughes 1971 [1956a], 442]. He said the same thing in “*Teaching as Fieldwork*”: “Sociology of the kind I have been talking of (...) contains the assumption that men are equal, equal in their humanity. Only so is sociology, the analysis and comparison of culture and societies, possible” [Hughes 1971 [1970b], 574].

This equality principle underlies the other ten. Perhaps I should have discussed it first, as Principle 1, for just that reason. But I put it here because it is a good way of gathering and encapsulating the spirit that undergirds the lot. Certainly, Hughes regarded it as foundational. In principle,” he wrote, “any person is the peer of any other. Thus, in his quality as a human being, any of us has the right to study any other and also to protect himself from the prying eyes of others” [Hughes 1974, 330]. This principle implies that social scientists have unavoidable ethical obligations to those they study. As I noted above in my discussion of Principle 10, the basic or minimal version of Principle 11 is that social scientists have an ethical responsibility to treat those they study with respect and, thus, to do them no harm. Testimonials from Hughes’s colleagues and students indicate unambiguously that in his teaching, research, and interpersonal relations Hughes held unfailingly to this principle and was quick to criticize those who did research that violated it [*ibidem*, 331]. But there is a stronger version of this principle as well and to understand Hughes’s position on this more muscular version it is necessary to return to our discussion of value neutrality initiated above.

If people are all equally human, then it follows that they deserve to be treated as such. That some people – sociologists and their “subjects” – might individually ben-

efit from the emancipation provided by sociology was a good thing. Indeed, Strauss [1996, 275] claims that “emancipation through enlightenment” was “Hughes’s deepest sociological commitment.” But as Hughes pointed out in “*Teaching as Fieldwork*,” the creation of a humane, rational, inclusive, tolerant society was a challenging further step, the difficulty of which he had underestimated early in his career. “Perhaps all of us in that earlier phase [of sociology] put too much faith in personal emancipation, in enlargement and humanizing of the mind by mutual observation and understanding, which we assumed would be followed by appropriate collective action” [Hughes 1971 [1970b], 574]. To create a humane, egalitarian social order, sociologists would have to move beyond “personal concern” to advocate on behalf of the oppressed and exploited. Hughes’s allegiance to Principle 11 forced him to abandon the distinction between the neutral scholar and the moral citizen and, thus, to set aside the classic version of value neutrality and to argue that as researchers, not just citizens, social scientists have an obligation to *help create equality and social justice*.

The case for objectivity in social science rests on no claim of the rights of the scholar, but on the fundamental premise that the moral man, to be effective, must have some organ of objective observation and analysis so that he will know the nature and strength of the evils with which he has to deal and the efficacy of the instruments to be used in dealing with them.

The whole point of “comparison” as an analytic strategy, Hughes wrote, was to find and point to what is “dangerous” [ECH Papers, UCHI, Frankfurt Diary, “*Visit with Professor Max Graf zu Solms, University of Marburg*,” 10 July 1948, cited in Staley 1993, 90] or “unjust” [Hughes 1971 [1947], 213]. Speaking about research on race relations in the US he wrote: “[W]e should not yield an inch to those who would have us choose our objects of study purely on the basis of something called ‘the state of knowledge’ without reference to what is currently going on in the world.” Were we to do so, he said, we would be complicit in the maintenance of unacceptably unequal and unjust relations among peoples because we might be allowing “the direction of our research and educational effort to be directed by the enemy, the defenders of racial and ethnic injustice” [Hughes 1971 [1947], 213].

How do we square this statement with his claim above that sociology should not be “a normative science”? I think Hughes was prepared to accept a conditional, situational “politicization” of the research act if it would bring to light and help eliminate some form of egregious inequality or oppression. Sometimes Hughes framed this in terms of a distinction between the professional and scientific aspects of the discipline: “We cannot decide once and for all,” he wrote, “to be completely a profession or completely a science. The problem [of neutrality] is chronic ... [and] can-

not be settled once and for all.” Instead, he claimed, “within the limits of lasting principles, different solutions have to be found according to the circumstances of time and place” [Hughes 1971 [1954], 467]. Hughes was prepared to allow that his colleagues could legitimately practice a variety of forms of sociology – what Burawoy would refer to as professional, policy, critical and public sociology – and his remarks in “Sociologists and the Public” make that clear [see e.g., Hughes 1971 [1962a]]. However, he rejected the idea that sociology as a discipline could or should be value free or that the practice of sociology could be value free. This was impossible because research is an institutionalized form of intrusive social interaction. Sociology, as a kind of intellectual work, has a place in the division of labour, including the moral division of labour. It is a part of, rather than separate from, the world it studies and, thus, has unavoidable political consequences for sociologists and those they study.

Although many sociologists would like to consider their work politically neutral, it is not considered so by those who make revolutions of right or left, or by those who have special interests in the things we study. However strongly we may emulate the model of pure science, claims for applying our knowledge and the fact that *what we learn* is never a matter of social indifference *will continue to put us in the position of people who give a service (or do a disservice) to our client, society* [Hughes 1971 [1954], 467; emphasis added].

So, while generally in favour of a “detached” and “dispassionate” sociology, he was also, in the words of Becker *et al.* [1968, ix, viii], “first of all a moral man” who “care[d] deeply about cruelty, injustice and war.” Becker *et al.* speculate that in this regard Hughes “carried into his own life the stance of his minister father, a man of great understanding and genuine moral commitment who was singled out by the Ku Klux Klan to have a cross burned on his lawn” [*ibidem*, ix]. Thus, it is not surprising that from time to time Hughes abandoned the classic version of the doctrine of value neutrality. He did so cautiously and reluctantly – certainly more cautiously than Burawoy – but he did so without regrets for, in his view, some social-political issues and problems were just too offensive and consequential to be treated as mere curiosities [see Hughes 1971 [1963]: 494-495]. Social scientists had an obligation to society as a whole – to the “social good” – that sometimes overrode the values of toleration and detachment. They had a responsibility to make their work available to the public in a way that contributed to the understanding, public discussion, and amelioration of egregious injustices. His treatment of two of the worst examples of inequality and oppression in the mid-Twentieth century – race relations in the US and anti-Semitism in Nazi Germany – provide cases in point.

In his 1947 essay “*Principle and Rationalization in Race Relations*,” Hughes claimed outright that racial and ethnic inequality was “one of the most distressing and

dangerous of the symptoms of our sick world,” and argued that “more power” and “all credit” should go to those social scientists then trying to understand the causes of such inequalities in order to “bring more justice into the relations between people of different racial and ethnic backgrounds.” He regarded it as a collective “right” and “duty” of social scientists, especially those scholars that belonged to “disadvantaged groups,” to undertake such “research and action” because it would “benefit society at large” [Hughes 1971 [1947], 212-213].

Likewise, in “[Innocents Abroad, 1948](#)” (a heretofore unpublished paper published in this issue of *Sociologica*), which he wrote during a visit to Germany in 1948. He went to Germany as part of a team of American social scientists sent to modernize that nation’s social science [see Staley 1993]. While there, he was particularly fascinated and troubled by the problem of how to understand the treatment that the Jews had received in Nazi Germany [see also Hughes 1962b]. He argued that Germans had created a “conspiracy of silence” around the subject after the war and claimed that it was necessary for them to acknowledge publicly and collectively what they had done, to take responsibility for it, and to discuss it [see also Staley 1993, 93-94]. Only in this way, he said, would it be possible to prevent such “perverse cruelty” [Hughes 1962b] from happening again – either in Germany or elsewhere [Staley 1993, 91-101]. Hughes was well aware that the role of the visiting American social scientist in this exercise was touchy and fraught with difficulties. “To find a course that does not imply condescension, the arrogation of priestly powers, the cheapening of the whole issue, or the descent into the abyss of cynicism; that is the question” [Hughes, “Innocents,” cited in Staley 1993, 95; see also Hughes 1971 [1962a] and Staley 1996]. But he did not pull back from the conviction that he had a responsibility to help prevent the reoccurrence of such an atrocity [see Hughes 1971 [1955]; see also Riesman and Becker 1971, x].

Hughes’s conception of sociology as a form of social interaction with political repercussions – indeed, sometime transformative aims – was tied to two further views, each equally egalitarian, humane, progressive, and democratic. First, as I have just made clear, he argued that sociological researchers had an obligation to contribute to the humanization of social relations. The power of knowledge should be used to challenge inequalities and injustices, to promote understanding, discussion, and positive change. Only in this way would an emancipated society and an emancipatory kind of sociology become a reality. This would occur because there would be a reciprocal flow of benefits between doing ethical sociology and developing oneself as an ethical citizen/researcher. When one learned how to do fearless, respectful, relevant research and then report and comment on the results with equal fearlessness and respect, contributing when and where appropriate to the reduction of social injustices, one was

simultaneously learning to be a good member of society, “a deeply concerned citizen of the world” who, as Anselm Strauss [1996, 275] so cogently put it, would “use his ability and training for the benefit of all.”<sup>14</sup> David Staley [1993, 86-88] describes exactly this process in his account of Hughes’s postwar efforts to promote positive change in the German university system as a part of his wider efforts to make the country more democratic. For Hughes, properly undertaken, research was a way of learning, teaching, doing, and promoting humane rationality and democracy.

### *Conclusions*

In my discussion above of Principle 1, I noted that Hughes’s theoretical-methodological approach was designed to penetrate and illuminate a wide range of human social practices. His goal was first to understand, but this goal always existed in a tension with his humanistic sensibilities. Thus, his efforts to understand “going concerns” – individuals, groups, careers, institutions, societies – were always undergirded by a moral sensibility: as you strive to understand human beings, you must do so within the boundaries established by Principles 10 and 11. Do no harm. All humans are or should be equal. If they aren’t, do what you can – respectfully, carefully, but bravely – to help them frame and realize that goal.

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<sup>14</sup> Though I cite Strauss here, it is essential to point out that Strauss would not likely agree with my description of Hughes’s views on this point. Strauss [1996, 278-279] argues that Hughes offers no real solution to the “universal problem of malevolent dirty work.”

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## Studying “Going Concerns”: Everett C. Hughes On Method

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Abstract: Everett Hughes is well known as having been a skilled and passionate advocate, teacher and practitioner of ethnography, especially what he referred to as “fieldwork.” This paper outlines Hughes’s “methodological orientation” in terms of eleven propositions that describe in detail his conception of fieldwork, bearing in mind: a) the mutually constitutive relationship in Hughes’s work between theory and method; and b) Hughes’s conception of research ethics.

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*Keywords: Everett Hughes, theory, method, fieldwork.*

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