

Part II. Tracing Development through the Totality

In Part I, I drew on ethnomethodological research and empirical observation to construct a logic-in-use for studying the lived reality of literate action development. In Part II, I apply this logic-in-use to study the literate action development of six writers at various ages in the lifespan. I conclude Part II by looking across these cases to (1) identify potential concepts that may be worked up into middle-range theories and (2) articulate a vision for seeing the totality as an infrastructure for studying literate action through the lifespan.

In the introduction to this text, I brought up the criteria of *strategic* and *perspicuous* research sites. A research site must be *strategic* in that it shows literate action *in action*, and it must be *perspicuous* by making visible the joint sense-making actions of the literate actors under study. In Part I, the research site selected overlapped in its strategic and perspicuous senses. The literate action studied in Part II, however, is different in a number of ways from the texts in Part I. This is deliberate, in that I want to use the totality at other points in the lifespan to both refine my understanding of it and generate new understandings about literate action development. But these deliberate differences also open up more space between the *strategic* and *perspicuous* research site criteria. Throughout Part II, I make decisions to sacrifice some of one for the other. This introduction to Part II provides a general rationale for my choices, which I will build off of to detail my specific decisions in each chapter.

A Note on Responsive Abstracting for Part II

In Chapter 1, I drew on the work of Garfinkel (2002) and his use of ticked brackets to articulate the ethnomethodological core of this work: that there is order in the everyday work of social activity, a social order that is always locally produced. Throughout Part I, I followed the ongoing production of social order to identify how individuated development emerged within that order. This work focused largely on ticked brackets, illustrated as $\{ \}$. The ticked brackets allowed me to articulate the way in which I saw order as forming scenically, in the moment. In Part II, I continue my search for social order as part of my emergent logic-in-use of the totality.

But the goal of this text is not to uncover social order and its production—instead, I am looking to build and then use a foundational infrastructure for studying literate action development through the lifespan. In Part II, I mobilize the infrastructure of the totality to develop concepts through which middle-range theory can emerge. As we saw in Chapter 1, Garfinkel would characterize this

move—from the production of social order to explanatory theory—as this:

$$\{ \} \rightarrow ()$$

The arrow in this diagram denotes the methods through which the production of social order is abstracted into formal analytic (FA) conclusions. Garfinkel saw enormous problems with such theorizing, believing that such a move would lose the phenomenon of the production of social order.

Despite Garfinkel's concerns, abstracting from the lived reality is a necessary function of the logic-in-use developed in Part I. Garfinkel's work, focused as it was on groups of people acting together and the practices that emerged from it, had the power of anonymity to support the practices it discovered. The practices for crossing an intersection, for instance, could be confirmed by its anonymous character: everyone acted in such a way to avoid being hit by a car when crossing that intersection. Garfinkel's attention to practices, then, remains focused on the shared aspects of it, and how we can engage in practices to reproduce immortal, everyday society.

Following individuated actors, however, loses some of the explanatory power that the ticked brackets traditionally provide with ethnomethodological work. The individuated take-up of practices in a particular moment is useful for understanding that writer's literate action development, but portable accounts that are useful for teachers and researchers developing lesson plans and research problems cannot easily emerge from such isolated work.

In order to combat this problem, I engage in *responsive* abstracting throughout Part II. Keeping the lived reality at the center of my attention through the totality, I develop concepts that enable one to abstract out of particular situations in ways that are sensitive to the ongoing production of social order from which those concepts emerged. In Chapter 9, I indicate how these concepts can be responsively mobilized into middle-range theories with broader analytical purchase.

A Note on Methodology for Part II

In Part I, I attended closely to the moment-to-moment literate action of writers. This material attention to literate action allowed me to identify moments of literate action development and build, from those moments, a framework for examining literate action development across wider populations of writers. Though I found this analysis valuable, its success was, in large part, due to the fact that I developed it out of a setting that was both strategic (for my research question) and offered perspicuous detail about the production of social order when the writers I studied were engaged in writing.

The seventh-grade students I studied were at a moment of transition in their lives. Having just moved from a single teacher in sixth grade to multiple teachers in multiple class periods in seventh grade, these students' school-home lives were

drastically altered. They now had more bosses and, with them, more homework to attend to. So the writing they often did on their own out of class was pushed aside in order to meet the demands of school. As a result, the writing I saw them perform in the classroom was connected to a broader network of writing activities that dominated, at the time, their writing lives. In contrast, the older writers I study in Part II of this text have complex literate lives that are more difficult to glimpse than that of sixth graders.

The changes in the circumstances of the writers under study necessitate different record collection. Tracing literate action at the moment of its performance ceases to be a useful option, as the cost of following a writer around is too high: such a study would not be able to be continued indefinitely and, if it were, the presence of a camera, or other researcher, or recorder of any kind might (and, in the case of writing in solitude, would) disrupt the everyday production of literate action. As a result, I would destroy the very phenomenon I hoped to study.

In order to keep the phenomenon of interest in my sights, then, I turned to a method of data collection often pushed aside by ethnomethodologists: retrospective interviews. Ethnomethodologists (Garfinkel, 2002) see retrospective interviews as problematic because, in their eyes, the phenomenon of social order is lost in the retelling of it. There are aspects of social order not available to the conscious attention of the actor, and attempts to stimulate recall of those aspects can introduce unavoidable variables: since the interview is itself a production of social order, how can we separate the recall of the production of social order from the production of social order that is the interview?

Despite these problems, however, I suggest that retrospective interviews are rich with possibility for studying the production of social order in literate action. Though certainly rife with potential confounding variables, the payout in terms of records collected on my phenomenon of interest is worth the challenge. By taking up my logic-in-use through records collected via retrospective interviews, I am able to extend and refine my understanding of literate action development from the perspective of the lived reality. Below, I articulate a brief overview of my rationale for moving to retrospective interviews, which I elaborate on further in each chapter as part of my rationale for particular decision-making with particular subjects.

At the heart of my ethnomethodological respecification of the retrospective interview is Schutz's (1945) notion of the *cognitive style*. Schutz, drawing on the work of William James, suggested that "there are several, probably an infinite number of various orders of realities, each with its own special and separate style of existence" (1945, p. 533). Garfinkel's reading of Schutz leads him to conclude that an interview would not grant the researcher access to the cognitive style⁵ at

5. Garfinkel's use of the term "cognitive style" in his dissertation and in *Seeing Sociologically* (2006) is a repurposing of Schutz's phrasing to suggest that one's "cognitive style" emerges with and through interaction. So even in his use of the term,

work in the production of a given reality, and that, even if it could, there would be no way to verify that access. The work we do to produce social order, in other words, is not accessible retrospectively. For Garfinkel and the ethnomethodologists who followed him, then, the very act of interviewing *loses* the phenomenon of producing social order.

My recovery of the retrospective interview begins with the assumption that the cognitive style of literate action can, in some circumstances and with the proper (that is, aligned to the phenomenon of interest) interview protocol, reveal the cognitive style of literate action in verifiable ways. My assumption of a cognitive style as accessible via more traditional sociological methods emerges in part from Cicourel's (1964, 2004) reasoning. Cicourel frames what he calls an *ecological validity problem*. This problem is posed as a series of questions:

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? (2004)

Cicourel works through this ecological validity problem by avoiding interviews in isolation, and incorporating ethnographic work that helps researchers build ecological validity to the questions posed in interviews (as well as surveys, although that is not part of the data I work with in the coming chapters). As noted above, Cicourel sees the need for interviews to emerge out of the work of everyday life—the questions have to be “congruent” with the ways in which respondents make sense of their daily activity.

Consider, for instance, an interview with a ghost writer, someone writing for other people, such as celebrities who do not have time to pen their own book. A researcher might be interested in how such a writer goes about collecting the material needed for this work, and how evidence is confirmed in the process of producing a text. But drawing on the language of the sentence above would run

Garfinkel was attempting to work around cognitive explanations of social order and focus on interactional work. As he mentions in his article on trust (Garfinkel, 1963), “There is no reason to look under the skull since nothing of interest is to be found there but brains” (p. 6).

the risk of skewing the records collected, of leading the interviewee to give information in the form that is requested by the interviewee, rather than allowing the flow of literate action to shape responses. The interviewer, therefore, would be in a better position to frame interview questions if they began with observing writing in action, the language the writer used when talking with others about their work, and the process through which a text seems to be emerging. This would be a starting point for collecting the language, the activity needed to form questions that will bring forward the lived reality of composing a text.

Ethnographic data of writers writing, as I address above, is problematic with older writers who write through a range of lifeworlds. By substituting several other elements for ethnographic data, however, I suggest that, in the coming chapters, I demonstrate sufficient ecological validity that I am able to glimpse (and confirm) aspects of the lived reality brought to bear on literate action for these participants. For each participant, I make sure to do the following:

1. Conduct multiple interviews over time;
2. Build interviews around objects—texts, computers, readings, etc.; and
3. Triangulate claims among interview segments and objects.

These mechanisms are nothing new to the field of writing research, but their inclusion supports the claims that I make about the lived reality of these participants going forward. In the coming chapters, these mechanisms allow me to develop a uniquely adequate sense of the literate action these writers perform, but relocates the “adequacy” requirement from a broader context such as a classroom to the recurrent iterations of particular practices.