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An Interactional Analysis of Clinical Interviewing *

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Running Head: Interactional Analysis of Clinical Interviewing

Abstract:

Clinical interviewing is viewed here as a social interactional pattern in order to examine the nature and limits of the technique as a means of scientific data acquisition. I defend the technique against criticisms that it is ecologically suspect and prone to systematic biases, mainly due to influence of the interviewer on the interviewee or to unnatural and biasing interactional patterns.

The Central Hypothesis of this work is that at least some forms of clinical interviewing are derivative of naturally occurring patterns of activity and interaction. As such, they do not warrant unnecessary a priori suspicion. More importantly, this view leads to avenues of empirical examination that allow determination and checking of the character of the clinical interaction, which bears directly on how and when data extracted in clinical contexts are scientifically valid. Data from an extensive corpus involving a single subject are used to illustrate and substantiate claims.

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Introduction

Clinical interviewing has been a visible part of the methodological repertoire of cognitive studies at least since Piaget. On the other hand, in terms of “cogency criteria,” such as being well-studied, rationalized, and codified, it is clearly less developed than, for example, familiar quantitative methods. Clinical interviewing also suffers by these same criteria compared even to other qualitative empirical techniques. For example, clinical interviewing is explicitly excluded from the purview of Ericsson and Simon’s well-known book on verbal protocol analysis (Ericsson and Simon, 1984).

This article considers clinical interviewing as a social interactional event, and it aims to understand the process and the validity of data that it offers. Out of such an analysis we may develop a basis for having more or less confidence in the results of clinical interviewing and also a better understanding of how to conduct and analyze such interviews.

An analysis of clinical interviewing is particularly important given the prominence of a number of critical perspectives that frame issues concerning the validity and relevance of data taken from clinical contexts. In particular, a family of approaches frequently described as “situated cognition” (Lave, 1988; Brown, Collins, & Duguid, 1989) gives rise naturally to a set of concerns. The following quotation from Bannon and Bødker (1991) is representative:

... [T]hese studies tend to analyze individuals without reference to their community, or their history, performing on a task designed by the experimenter in an unfamiliar environment. The “problem” is defined and valued by the experimenter, not by the subject, who is then expected to perform in certain ways. In some experimental manipulations, even the very nature of the task, or the required behavior, may not be clear to the subject. The question of how “subjects” make sense of the game in which they are playing, trying to discover the “rules of the game”—that is, what the experimenter is after—is often not explicitly discussed in these studies. Performance is measured relative to a certain “ideal,” rational model of problem solving, and the deviations of subjects from this abstract logic are noted.

It is presumed that the fundamental mental mechanisms posited to underlie human behavior in such prescribed domains can later be extended, without major modification, to more real-world activities. So the assumption is that “problem solving” is a generic cognitive activity that has a similar form across quite a variety of domains, from acting in a psychology experiment to everyday cognitive activities. It is also usually assumed, implicitly, that this activity is located “in the head” of the individual. These assumptions ... have once again come under serious attack from a variety of researchers. (p. 230)

From this critique, we may abstract the following concerns. Clinical interviewing and subsequent analysis may:

- Ignore important and relevant factors such as
 - subjects’ community membership and personal histories
 - subjects’ perception of, and familiarity with, the task
 - the influence of an unfamiliar physical and social environment
 - subjects’ take on purpose of the interviewing “game”
- Impose inappropriate external rules and standards
- Assume unwarranted invariance in the results, for example, that they “transfer” to everyday contexts.

Rather than constituting in-principle objections to clinical techniques, I presume that these critiques implicate a family of analyses that have not been sufficiently well accomplished. In particular, one needs to understand the nature of clinical interviews as *social interactional events*.

The aims of this article are modest in several respects. First, I consider mainly the sort of clinical interviewing with which I am familiar from my own use of it. Briefly, I interview young

adults for several hours over several sessions in order to analyze their intuitive understanding of a scientific field (physics). Thus, the results claimed here will not necessarily apply to clinical interviews conducted in different ways, with different goals. I also focus on several specific critiques that may plausibly be leveled against clinical interviewing as a scientific source of data. These critiques are similar in breadth and emphasis compared to those articulated by Bannon and Bødker. The goal is not to provide any approximately complete analysis of clinical interviewing as an interactional event, but only to provide a sketch sufficient to begin to address a representative set of concerns.

The exposition is also limited in several other ways. Although a brief description of clinical interviewing is provided, intended mainly for those who are less familiar with it, this is far from a “how to” description. I will not provide comparative, “strengths and weaknesses” analysis of the technique, nor will I describe *extrinsic* criteria of validity, such as repeatability, and so on, to validate analyses drawn from clinical data. Many of these are addressed in other sources such as Ginsburg (1997) or Clement (2000). Instead, the primary aim here is to introduce some *intrinsic* properties of clinical interviews that can provide theoretical and empirical purchase on issues raised by critiques like Bannon and Bødker’s.

What Is Clinical Interviewing?

Typically, a clinical interview is a one-on-one encounter between an interviewer, who has a particular research agenda, and a subject. The interviewer proposes usually problematic situations or issues to think about and the interviewee is encouraged to engage these as best he/she can. The focal issue may be a problem to solve, something to explain, or merely something to think about. An interviewer may encourage the subject to talk aloud while thinking and to use whatever materials may be at hand to explore the issue or explain his/her thinking. In my own interviewing, since the topic is physics, I always make available pencil and paper, and sometimes physical or computer-implemented materials as props to explain the task or to help the interviewee engage it. Interviews are almost always video taped and usually later transcribed for analysis.

In the Piagetian tradition, and also for the interviews I conduct, the over-arching goal of a clinical interview is to allow the interviewee to expose his/her “natural” ways of thinking about the situation at hand. An assumption is that subjects’ ways of thinking are delicate and complex, and skill is necessary to surface them in a mutually intelligible way. The interviewer explores different ways of framing the problematic situation. He/she may introduce problem variations, sometimes supplies alternative interpretations to which the interviewee may respond (“Someone told me that ...; what do you think?”), and seeks to uncover depth of commitment or ambivalence. Changes of interpretation are critical events worthy of particular attention. These and other strategies help triangulate (a) the nature of any particular conceptualization an individual might exhibit, (b) the range of conceptualizations subjects (individually or as part of a population) have available, and (c) something of the dynamics of conceptualizations. The last category, dynamics, includes conceptualization’s “contextuality” (when exactly are particular conceptualization applied; see diSessa, 2004), how they develop, how much confidence and facility subjects’ exhibit, and how ideas may shift.

Clinical interviews are often opportunistic in exploiting situations that emerge in order to refine views of subjects’ thinking. For example, an interviewer may develop a hypothesis on-line and propose a related question to confirm or disconfirm the hypothesis. The related question might involve a situation that should invoke similar responses, or critically different ones,

depending on what the interviewer believes the subject is attending to. At the same time that they are opportunistic, interviews are cumulative. The interviewer may have encountered a surprising response in another interview, and consequently prepared a question off-line that disambiguates opposing hypotheses in case he/she encounters a similar response from the same or another subject. Problems that evoke rich, informative responses are a precious resource that is cumulatively built in areas of interest.

Asking for clarification, elaboration and confirmation (for example, by repeating or rephrasing what the interviewer is hearing) is characteristic of a clinical interview. Personal or authoritative challenges to subjects' views are rare. Instruction is also relatively rare, mainly because it tends to induce a view of the interviewer as an authority, possibly with normative and evaluative intent. [See Steffe (1991) concerning instructionally oriented clinical studies.] The pace of a clinical interview is typically slow and reflective. The interviewer almost always withholds judgment on subjects' responses.

The interviewee's task is easier to describe and probably easier to accomplish. He or she is to engage the task in whatever terms feel appropriate, and to the extent to which it seems sensible to do so. The interviewer or the interview fails if the interviewee adopts another stance toward the interview, such as protecting the privacy of thinking, or producing only what he/she takes as the interviewer's "intended response." Elaborating the meaning of this top-level goal for subjects, and the conditions under which subjects take it on, will be a major concern of this article.

Given this prototype, a substantial range of variation may still be appropriately encompassed under the term of "clinical interview." In terms of form, interviews may involve more than one subject in collaborative tasks and may be strongly or weakly dependent on prepared materials to interact with. They may be tightly or more loosely scripted and may involve subjects for only a few minutes, or over many hours. In terms of focus, researchers may differ in the aim of analysis, from determining knowledge or intuitions to charting emergent goals and strategies. Interviews may be formative and exploratory, or interviewers may enter them with very particular hypotheses about which they seek supporting or detracting evidence. Interviewers may be intent to complete an inventory of possible responses and ways of thinking from a representative sample of students (Marton, 1981), or they may be intent on detail and finesse in describing a particular subject or a particular conceptualization. Analysis techniques range from systematic coding of every utterance and concomitant statistical analysis, to the development of a corpus of data that may be used in prescribed argumentation patterns to settle disputes or refine theoretical perspectives (diSessa, 1993).

Preview

In order to connect best with critical perspectives, this article is organized as a response to criticisms, which are elaborated in the following section. Such a strategy has the disadvantage that the central new ideas here will appear only gradually, and not in their most natural order. In order to partially compensate, this section offers a brief preview of claims.

First, the position taken here with respect to methods is generally ecumenical. Clinical interviewing is good for some research purposes, and not good for others. Although I believe clinical interviewing is frequently undervalued because of prejudices concerning its nature, there is no question of considering it to be a universal method.

Second, it is important to evaluate methods as part of a larger program of research. A “good” clinical interview may still be analyzed in varying ways, which may introduce their own good and bad properties. At broader levels, even well-executed data collection and analysis can inherit the faults of the larger program of which they are a part. In complementary manner, some methods’ limits and faults may be compensated for in other aspects of the broader program. So, for example, I will not argue that clinical methods, by themselves, can be made “immune” to the difficulties that are outlined in the next section. Indeed, to reach some criticisms, I will need to introduce aspects of the larger research program.

In order to examine the properties of clinical interviewing as an interactional event, I take a developmental point of view. That is, I try to sketch (a) the genetic precursor activities that lead into clinical interviewing, and (b) how these activities become modified and adapted to clinical purposes. In particular, a principal claim in the article is that clinical interviewing can emerge from a familiar and ecologically valid activity—making sense of problematic situations, which I call “inquiry.” Many of clinical interviewing’s useful properties follow from this heritage. In addition, I propose a fundamental constraint that should guide participants in clinical interviews: Problems and questions posed by the interviewer must appear to be sensible inquiries to the interviewee. This constraint helps guarantee useful results, and it also operates as a focus of empirical study to validate claimed properties of clinical interviewing.

Three Criticisms

The following three criticisms may be leveled against clinical interviewing with regard to its status as a valid and scientifically responsible method for obtaining cognitive data.

“White Room” Investigation

Ecological validity is an important general consideration in psychological investigation. Certainly we do not want to analyze data that are collected under circumstances that relate in problematic ways to the conditions that hold when the processes we supposedly are investigating ordinarily operate. The history of cognitive studies is full of examples that warn that it is easy to take human beings out of their “native habitat,” resulting in odd attributions. One may wind up assigning the label “incompetent” merely because people react strangely in strange circumstances, or because we remove them from the normal, meaningful support provided by familiar contexts (Laboratory for Comparative Human Cognition, 1982). On other occasions, researchers who studied only presumed-to-be-unusual situations have diagnosed behaviors as pathological, when those same behaviors turn out to be evident also in everyday situations involving “normal” people (Schegloff, 1993).

On the face of it, a clinical interview might seem to be an exotic context in which to investigate cognitive processes that are supposed to operate in school or in other corners of everyday life. Materially, the room may be bare of the common surround of the interviewee’s everyday cognition. Socially, a subject might well not understand—and indeed may be suspicious of—the motives of the interviewer. There may be no scripts or common interaction patterns available to the subject to deal with the interviewer, or perhaps those that are available produce skewed views of the subject’s thinking.

Coercive/Seductive Practice

A courtroom is a familiar place in which people cannot express ideas in their natural voices. Lawyers may elicit statements with implications that are clearly not intended by witnesses. More subtly, research has investigated conditions under which memories of non-existent events may be created (Loftus, 1980).

Surprising experimental results, such as unexpected patterns of thinking by a subject, are scientifically important. However, surprising behavior prompts suspicion about the experiment, for example, that the interviewer somehow “made” the subject respond in an unusual way. A common expectation is that a subject may be led to retract or revise an opinion because of subtle cues or facial expressions. As in a classroom, repeating a question may be interpreted by the interviewee as a sign that the first response was incorrect. On a longer time scale, some critics attribute manipulative capability to variations in the interviewer’s attention or interest, or even to the unusual and serious attention that may be shown toward an interviewee’s ideas.

Questionable Invariance of Knowledge

According to some accounts, knowledge may be radically situation specific. Knowledge that is used in a situation may be assembled for the purposes of that situation, and may well not represent any enduring property of the individual in question (Suchman, 1987). More conservatively, one must recognize that knowledge is frequently, if not always, in the process of revision. People learn, perhaps all the time. On what basis, then, can we attribute particular knowledge to an interviewee from what he or she may say in a particular conversation?

Questions of the invariance of knowledge go to the heart of deep epistemological commitments and differences in orientation. I will be able to address only a limited range of pertinent issues. This is where the larger research program bears particularly strongly.

An Interpretive Framework

The analysis of clinical interviewing here involves two levels. On the first level, I try to define and characterize the general interactive nature of the process. From this characterization flows the basis for many particular defenses against the criticisms listed above. On the second level, some more particular orientations and strategies—which I claim characterize the genre of interviewing that I use, and which I will examine and exemplify—are helpful in defending clinical interviewing from some of the above criticisms. The second level, of more particular considerations, will not be introduced in this section. Instead, it will emerge in the following sections, which use data and argument to buttress and extend the more general framework.

What, exactly, do we need to understand in order to warrant the reasonable use of clinical interviewing as a scientific method of data acquisition? Clearly, it has to do with language exchange and social interaction. I hope to describe, in an approximate but helpful manner, the nature of this interaction.

Substantial prior work has aimed toward classifying and determining the properties of situations of verbal exchange. Bakhtin (1986) offered the term “speech genre” to describe classes of such interactions, and he argued extensively for the wide variability and importance of understanding them. More recent researchers, particularly those engaged in conversation analysis, have introduced terminology and analytical frameworks for studying them. Schegloff (1993) used the term “speech exchange system” and enumerated a range of factors that enter into the patterns that they represent. Hymes (1989) used the term “styles” broadly and neutrally, with

special emphasis on linguistic patterns. Hymes (1974) produced one of the more detailed analytical frameworks for studying styles. Goffman (1981) emphasized broad social aspects in his study of “participation structures.” Levinson (1993) studied “activity types and language,” and I will follow his example with its emphasis on activity as well as language by using the term *activity type* to describe genres of interaction.

A heuristic orientation toward the general nature of human knowledge and activity, as follows, helps adapt the above-cited work to the present context. One may propose that, in general, human knowledge and activity patterns are fundamentally *rich* and *generative*. That is, we should operate under the assumption that people have a broad range of resources available to deal with a wide range of situations. Hence, simple and compact descriptions of behavior and knowledge are likely to be limited in two very particular ways. First, richness suggests that simple, compact descriptions may very much under-estimate the subtle variations in knowledge and in behavior displayed in slightly different situations (diSessa, in press; diSessa, Elby & Hammer, 2002; diSessa, 2004a). I already alluded to this idea as a presumption of clinical interviewing. Generativity emphasizes that knowledge is only useful if it is adaptive. People learn much of the time, and a significant part of the knowledge that they have will be directed toward generating new knowledge and new ways of behaving. Generativity may show in short-term adaptation to a particular problem or even to a particular prompt from the interviewer, and it will most certainly show in longer-term adaptive patterns of development. Both richness and generativity underscore the need to attend to critique number 3, concerning knowledge invariance.

A central relevance of the “rich and generative” orientation to understanding clinical interviewing is that we should expect interviewees to be adapting creatively to the situation in which they find themselves, even if it is initially unfamiliar. Dismissing clinical interviewing out of hand as unfamiliar or exotic is insufficient. We need to see what the local processes of adaptation bring about, and we should not assign the result automatically to any assumed activity type without due empirical and theoretical diligence. In the following, I identify three prior and familiar activity types that likely play into the construction of clinical interviewing as it is practiced by the interviewee. Yet, clinical interviewing is different in small or large ways from each of them.

A Modest Proposal

Children and adults spend a reasonable amount of time wondering why things happen and how they work. This is most evident when something goes wrong, and one wants to know “why did that happen?” Say, a personal relationship has deteriorated, and one feels the need to figure out why. These occasions may well prompt invention or improvement of causal ascriptions to the world. “My friend was incapable of commitment.” Presumably, “incapable of commitment” defines a relatively stable and causally relevant property of individuals, which may be determinable without reference to features of the particular situation. Reflecting on recent past occurrences may improve one’s ability to identify individuals who are incapable of commitment, to understand why they act in the way that they do, or it may even engender the category in the first place.

Infants and children must spend quite a lot of their time in these inquiring states. Even in the absence of surprise, figuring out why things happen must be an important part of developing competence with all aspects of the world, including physical and social aspects. Ochs et al. (1992) document the commonplace negotiation of explanations in family discussions. Many, if

nor all, studies of intuitive psychology (Gopnik & Wellman, 1994), biology (Inagaki & Hatano, 2002), and physics (diSessa, 1993) implicitly or explicitly assume that both children and adults learn by observing and reflecting on their experience.

Inquiry of this sort—“sense-making” might be appropriately suggestive—is certainly not restricted to individual activity types. People inquire frequently with each other, sometimes for entertainment, sometimes more seriously. Conversations between friends—or even between relative strangers, for example, in a barroom conversation—include segments that may start something like, “now, why do you suppose...?” Why do you suppose the Republicans are retreating now on budget matters? Why do you suppose beer fizzes like that when you open the bottle? Or, spontaneously proposed explanations and predictions may engender contrary ideas that are debated. In such situations of joint inquiry, the flow of give and take, of suggestions and counter-suggestions, of judgments offered, and so on, becomes a familiar activity type around which one organizes a significant amount of competence.

The Central Hypothesis of this article is now easy to state. Clinical interviewing (at least the type I will examine empirically below) is a form of mutual inquiry that is developmentally derivative of naturally occurring individual and mutual inquiry activities. If clinical interviewing is derivative of naturally occurring inquiry, a number of things may follow:

First and foremost, this would establish a partial warrant for ecological validity. Clinical interviewing may be at least continuous with activities that are part of “everyday life,” including schooling. That continuity would partially explain a fact that I offer, at this point, as an informal observation: Almost all individuals I have interviewed fall relatively quickly into a comfortable equilibrium, and respond as if the interviews were everyday occurrences. To be sure, the time to settle into this equilibrium varies. Indeed, I sometimes dismiss interviewees who, for one reason or another, fail to find the discussions sensible and congenial.¹ Against the suspicion that clinical interviews are exotic and unnatural, the phenomenon of a “comfortable equilibrium” warrants attention.

Most importantly, the hypothesis that clinical interviewing is developmentally derivative of naturally occurring activity types opens the door to empirical and theoretical inquiry. We can study the properties of natural varieties of inquiry, look at the processes that transform them into the clinical activity type, and study the emerging properties and indicators that might validate or invalidate claims of authenticity for clinical data.

Let me begin by hypothesizing the top-level properties of mutual inquiry, including the ways in which clinical interviewing differs from naturally occurring versions. This will begin a task that, obviously, cannot be completed in this article—a detailed account of mutual inquiry and its development into clinical interaction. But a start will still provide empirical opportunities to support or disconfirm the Central Hypothesis. In this schematization, I will follow Levinson (1993) (very roughly) in terms of what a description of an activity type should include. In particular, I start with *goals*.

... it seems that in most cases apparently ad hoc and elaborate arrangements and constraints of very various sorts [that define a particular activity type] can be seen to follow from a few basic principles, in particular rational organization around a dominant goal. (p. 71)

The central goals of naturally occurring and clinical mutual inquiry, I argue, constitute a pivotal commonality. In contrast, clinical interviewing involves at least one evident discontinuity. The

¹ An attempt at cross-cultural studies similar to those I have conducted in the U.S. seemed to fail because participants continued to treat the interaction as a formal interview regarding schooled knowledge, rather than sense making of a more common sort.

redistribution of *authority* and *responsibility* from a fairly strict parity to a limited hierarchy (as described below) defines a fundamental difference and reorganization of mutual inquiry from its naturally occurring kinds to clinical interviewing. In the following I separate the interviewer's perspective systematically from the interviewee's. After discussing goals and authority, I also briefly discuss central constraints on the interaction.

To sum up the discussion so far, the framework proposed here to analyze activity involves describing (a) goals, (b) authority and responsibility, and (c) constraints inherent in the interaction.

Goals

The interviewee's primary goal in a clinical interview of the sort I conduct is essentially the same as in naturally occurring inquiry. That is, it is his or her assumed task to make sense of some problematic situation as best as he or she can.² This is very nearly a restatement of the Central Hypothesis, that clinical interviewing can approximate a familiar, everyday activity type. Having a familiar goal—that of “making sense of a problem or situation”—should account for the marshaling of familiar strategies and orientations to the task, both toward the “cognitive” task, and toward the social exchange.

Having a particular goal, for subjects to “make their own sense,” has observable entailments. Interviewees should orient toward that goal and act in ways consistent with having it. They might even say in a convincing way that making sense is their goal. Secondarily, it is important that the interviewer also orient toward and act so as to support the interviewee's having the goal of making sense. Clearly an interviewer can either strongly support or undermine a goal by acting in ways that are consistent or inconsistent with it. It helps also that the explicit and expressed goal of the sort of clinical interviews I conduct is “making sense” (see later examples). On the other hand, assertions by an interviewer should be treated skeptically as indicators of what goals are actually in effect.

In general, goals that are in effect—and connections between actions and goals (such as a claim that certain actions support a particular goal)—are subject to empirical scrutiny. A basic orientation in this article is to define and pursue empirical means for validating the existence and consequences of goals as enacted in clinical interviewing.

The interviewer's primary goal in a clinical interview is (should be) to discover the interviewee's manner of making sense. Thus, helping the interviewee take on the principal goal of mutual inquiry is instrumental for the interviewer. This is the basis of a great felicity in the relationship. The interviewer's success depends on allowing the interviewee his or her own way of thinking about the presented situations or problems. Consider a likely alternative goal for the subject: to display only normatively correct knowledge, as is commonplace in schools. If the interviewee takes on this goal, the interviewer fails. Right or wrong answers are not at issue, and it is in the interviewer's interest to make this clear in one way or another. In fact, the interviewer typically aims at fostering a secondary goal in the interviewee: to help the interviewer make sense of the interviewee's ideas. Perhaps surprisingly, even children as young as six years of age can comprehend and adapt easily to this secondary goal (Ginsburg, 1997, p. 95, 106).

Of course, individual interviewers may act inconsistently with mutual inquiry. They may encourage conflicting goals, say, by openly expressing judgment about ways of thinking. They may provide insufficient indication that the central goal is mutual inquiry. Or, they may provide

² Of course, in naturally occurring inquiry, other goals (such as “having fun, or “getting to know someone”) may be co-present, or sometimes dominate. Treating this complication is beyond the present scope.

insufficient evidence of the desire for and viability of the goal for the interviewee to adopt it. All of these—and most particularly the interviewee’s actions (consistent or inconsistent with mutual inquiry)—constitute a basis for testing the claim that clinical interviewing is a species of naturally occurring mutual inquiry, and, consequently, for testing the basis for ecological validity described here.

Authority

Authority is not a monolithic issue. Interviewees, in agreeing to participate in an interview, cede authority—at least tentatively—on certain aspects of the interaction, most notably on the topic of discussion. There is every reason to believe most members of contemporary society understand the basic idea of an interview. It is displayed daily in television news and “talk” shows. In a clinical interview, however, authority for judging reasonableness of a description or explanation rests with the interviewee. In contrast, public interviews may involve judgmental and antagonistic actions on the part of the interviewer. Whether the interviewee has taken on the role of self-reliant judge may be tested. This issue is critical since, in many or most instances, the interviewee’s judgment is central data (in conjunction with the context set by the interviewer and by the preceding interaction) within a clinical mutual inquiry. To summarize, the interviewee cedes authority on topic and macro flow of the conversation, while retaining authority on plausibilities and judgments on lines of reasoning.

Again, the claim is not that clinical interviewing is the same as naturally occurring mutual inquiry. Instead, the claim is that clinical interviewing is derivative in important respects from mutual inquiry, and inherits some important properties (e.g., the possibility of truthful report of feelings of sensibility). A revised authority structure demonstrates that clinical interviewing inherits some characteristics from another familiar activity type, the interview. In the clinical interviews that I conduct, consistent with everyday interviews, interviewees appear to finish their contribution whenever they feel it is reasonable to do so. Then they expect me to ask another question or pose another problem. In other respects, turn-taking will presumably be organized substantially like ordinary, relatively informal conversations. Once again, all of these claims and presumptions are subject to empirical check.

The distinction between micro-flow and local judgments, on the one hand, and global flow of the interview, on the other, is critical. It marks a boundary between what is warranted as “belonging to the interviewee” and what is not. An important and sensible strategy for an interviewer, given the need to establish the interviewee as the authority on sensibility, is, as a general rule, to refrain from announcing his or her own judgments on correctness and even plausibility.

Constraints

Given the stated top-level goal of clinical interviews (for the interviewee to display their sense of problematic situations), the following is a good candidate for a “contractual” constraint on the interaction. Questions that the interviewer proposes must be sensible as questions the interviewee might well have asked himself or herself. Or, they must at least be questions that the interviewee believes someone without ulterior motive (that is, someone engaging in a sense-making inquiry) might pose about the situation or analysis of it. For reference, I call this the *Primary Constraint*.

In the following, I use a data corpus that consists of a series of interviews conducted with a single individual to develop some empirical lines concerning the suggestions made above. The

interviewee, called J, was one of several undergraduate students who were involved in a study of intuitive knowledge in physics. These interviews followed a long line of studies of intuitive knowledge based on clinical interviews (diSessa, 1983; diSessa, 1993; diSessa, Elby, & Hammer, 2002; diSessa & Sherin, 1998). J turned out to be an especially interesting case for reasons that are not particularly relevant here.³ Therefore, the approximately seven hours of video-recorded interviews have been a focus of extensive analysis (diSessa, 1996; diSessa, in press; diSessa, Elby, & Hammer, 2002; diSessa & Sherin, 1998). Using data from a single subject makes it clear that my intention is to illustrate and suggest plausibility and empirical tractability of the hypotheses presented. The point is definitively not to show that clinical interviews necessarily have the properties in which we are interested, but that they *may* have them. The advantage of using this single subject is that the data have been relatively thoroughly analyzed and the large corpus allows us substantial opportunity to illustrate important attributes of clinical interviewing—in particular, development of clinical mutual inquiry as an activity type and triangulation on subject’s ideas and attitudes across many circumstances.

Given the hypotheses and discussion above, I list several initial expectations:

- (1) There will be a period of transition during which whatever initial expectations and habits of interaction brought by the interviewee are gradually transformed into a relationship of mutual inquiry.
- (2) We can expect, furthermore, that the issue of authority will dominate this initial period. In particular, coming in, an interviewee may well expect the interviewer to take the usual instructional position of authority on the subject matter at issue. There are well-documented interactional patterns that go along with this relationship. A common pattern is IRE (also called IRF or triadic) dialogue. In this activity type, a teacher or other person in authority initiates (I) a sequence, usually with a question. The subordinate individual (student) is expected to provide a response (R), which typically is a display of codified knowledge. Finally, the teacher provides an evaluation (E), either explicitly (“that’s right” or “getting close”) or implicitly (say, by calling on another student). IRE interaction is a dominant mode in many classrooms (Sinclair & Coulthard, 1975; Mehan, 1977; Lemke, 1990; Wells, 1993).

Interference from authority-dominated activity types can be expected to constitute a primary block to establishing mutual inquiry. In particular, subjects typically come into interviews in their role as students (they are recruited as “students of physics”), they know I am a professor, and some may guess that, although a professor of education, I may know some physics.⁴ Knowledge of competing patterns (such as IRE) and indicators of non-inquiry activity types provided important empirical opportunities to test clinical interaction.

- (3) We can also expect that the interviewer may do a fair amount of work, both overtly and covertly, to move the relationship into a genuinely inquiring mode.

I now present data relevant to these expectations. J provides an unusually marked and clear transitional period. Right from the beginning she announces her fear that this interview situation will be like a test, and she may not perform well. This appears in the following segment, and we also see some explicit work that is done by the interviewer to try to allay these fears. [Each protocol segment is labeled by which of seven interviews it is from and by its time in minutes

³ J showed an unusual richness in her intuitive ideas, and her attitudes toward the nature of physical knowledge also seemed out of the ordinary.

⁴ On the other hand, at least some subjects provide unambiguous evidence, even after a number of interviews, that they do not believe I know much physics.

from the beginning of the session. Brackets and italics denote interpretive commentary. Ellipses denote deleted material and // denotes interruptions or abrupt halts.]

[1 00]

J: ...when I first heard about this I thought, oh, it sounds like a great idea. You just talk about physics, no problem. But then people were like “wait, are they going to give quizzes, are they going to ask questions?” And, I don’t know, maybe just ’cause the last couple of tests I didn’t do as well as I thought I would have that I feel like I don’t have much of a physics brain. So I’m sitting here and thinking I’m not going to be able to answer anything, and am I going to feel even more dumb? What am I doing? So I decided to come here and see what it was about.... So I was thinking to myself, “God maybe he’ll want to find someone else that can answer the physics questions.”

I: No, we’re not looking for people that are—we just haven’t had any particular preference. We are interested in the way people think. And a lot, as I said, a lot of it has to do with thinking about fairly commonsense, you know, situations, and not so much physics problems per se.... And as I said, we’re not interested in right or wrong, you know, right or wrong; we’re not interested—we’re not going to grade you; we’re not trying to find out how smart anybody is. That’s just not what our interest is. We’re just trying to find out how people think about, you know, how people think about, um, about physics.

Incidentally, this segment suggests another way, directly related to the Primary Constraint, that an interview can fail: if the interviewer proposes problems and things to think about that are too difficult or complex for the interviewee to engage. Tracking the level of competence of the interviewee is a critical subgoal for the interviewer.

After discussing J’s prior experience in physics, the interviewer introduces the first discussion question, and J announces her surprise at the informal (everyday) nature of the question.

[1 12:15]

I: How about, um, sort of general science... [inaudible]. How about something like: Why is it that it’s warmer in the summertime and colder in the wintertime?

J: Why is it warmer in the summertime and colder in the wintertime? That’s funny. That’s something I would never think, I’m so like prone to thinking, “I have this inclined plane,” and, you know, this “block sliding on the plane,” “the force of friction.” I never think of something like that as being a physics problem. Why is it warmer in the summertime and colder in the wintertime? Um. I suppose I have to give a physical answer to that.

I: Go ahead and give an answer that makes sense to you. As I said that’s really our// we want to talk about what makes sense to you.

J: What we think about.

Directly after providing an initial response to this question, J again initiates the question of what sort of relationship she should assume in this interview. In particular, she seems to expect a standard IRE pattern.

[1 13:30]

J: Now, do you tell us if we’re right or not?

I: Well, generally I don’t like to, for at least for a while, unless you’re really keen. Some of these questions, it’s not really clear that there’s a right or wrong answer. But mostly I’d like to just talk about it for a while, see where, see how you think, see what your judgment is.

Undaunted, J continues to seek feedback and evaluation. She even complains when judgment is withheld.

[1 17]

J: But, but see, I can’t give you an answer to the question we want to answer unless I know I’m on the right track. Because if I’m totally wrong, everything I say is wrong. But, I can assume that’s right—the look on your face says that’s right, kind of. [*J is incorrect in her physics here, so it is unlikely she is actually getting subtle approval. She is more likely misinterpreting ongoing acknowledgment of her contributions (Clark & Schaefer, 1989) as approval.*]

About half an hour into the interview, J begins to show signs of getting into a reasonable inquiry mode. Here, she fends off the interviewer's tentative attribution of a conclusion to her. The interviewer acknowledges her right to reject this attribution.

[1 25:30]

I: So you've decided now that probably//

J: I haven't decided anything yet. I'm just thinking, see if it makes more sense.

I: I understand; that's great.

On other occasions, both before and after this one, J accepts similar attributions. See directly below.

In the remaining part of the interview, the interviewer pursues the sense that J is making of the situation. J shows increasing signs of concentrating on the problem, and less on judgments of the interviewer. The following are representative examples.

[1 39]

I: I just want to get straight on this—the logic of this cooking thing, how is it... You are telling me that if things are just like this [*showing a globe positioned with respect to the "sun"*], then it's going to get real hot down there. I'm trying to understand why, then, it's going to get real hot down there, and I'm trying to understand why, why that's—I mean, it seems obvious to you.

J: It seems obvious to me looking at this globe. But if you think of the way the world is, it's not obvious at all, and it doesn't make sense.

Above, J appears both to confirm the interviewer's suggestion that her contention seems obvious to her, but also that from another perspective ("geographical" knowledge about temperatures around the world), it is sensible to question the fact. This seems like an unusually good example of a question acknowledged as reasonable in accordance with the Primary Constraint, "a sensible person might have asked that question" (in this case, questioned an assertion or explanation). Note that J apparently feels free to position herself as believing something ("it seems obvious to me") that, from another perspective "doesn't make sense." Below, J seems clearly engaged in the problem. She ends by asking if the interviewer finds sense in her descriptions, "You know?"

[1 53]

J: ... I'm just trying to visualize, you know, if it's closer when the Southern hemisphere is facing the sun, it's even warmer than when the Northern hemisphere is facing the sun. But to me that seems a little fishy because I am not in a position to say that the summers in the Southern hemisphere are warmer than the summers in the Northern hemisphere. You know?

The following comment was offered by J at the end of the first hour's interview when queried about how she felt about the interview. She acknowledges that this is a situation where she needs to "think on her feet," and consequently might reasonably have to change her opinions. She also acknowledges the temptation to interpret the situation as one in which the interviewer judges her responses, and asserts that she needs to "get past that." She even suggests insightfully that judgment is something she expects from school activity patterns. She even asserts that the interview is a pleasant activity.

[1 55]

J: I like it [*talking about physics in this way*], because it's problem solving. In a way, it's weird to feel like I have to give an answer right away, so I'm saying some things that later I am realizing didn't really make that much sense, you know? Because, for me, I can think through things when I bounce things off each other instead of like thinking through the whole thing and saying, "okay here's the answer." I have to say this happens and this happens and maybe this will happen. So it makes me think, and here I am trying to figure something out, and I realize it is wrong. So I try to figure something else out and I think about it. I like that. I just have to get over, personally, thinking that if I say all these wrong things this person's looking at me thinking I'm a total idiot, and I have to say the right things. I have to get past that, the fact that I have to give every one right answer. I think that's one thing that school does to you, like if

you say something like that I wasn't sure how the earth was spinning, that's something I should know. So I feel like, god, man...

I: Well, I think that's something school does to you, and I don't like it particularly myself.

J made several positive comments about the task and her competence at it at the beginning of session 2. For example:

[2 3]

J: Even though I knew some of the things I was saying was wrong, I thought about things like the whole time. So I left feeling pretty good, that's kind of neat, I just thought about things for an hour.... a guy friend of mine came up, who is like Mr. Science, and so he asked what I was doing, and I said it was neat because he didn't ask me like "if you have something on an incline plane at this angle, what happens?" ... But you just asked me a really easy question.

Note that, although there is plenty of evidence that the problem was challenging, J describes it as "easy." This suggests that the interviewer has correctly chosen a topic with an appropriate level of difficulty. Notice also the reports of positive affect, as if to say "inquiry is fun."

J's assertions that she understands and likes the nature of the task is not the same thing as engaging wholeheartedly in mutual inquiry. In fact, J seemed unusual in taking at least two sessions to settle into an apparent equilibrium. During this time the interviewer frequently took opportunities to comment explicitly on the goals of the task. In place of an extended analysis of this transition, I offer here a summary analysis, to be supplemented later with numerous examples that suggest her equilibrium attitude in subsequent sessions. The summary analysis shows the frequency of questions and comments about the interviewing process by J, and similar commentary offered by the interviewer over the first three sessions. This pattern suggests that both the interviewee and interviewer have become comfortable in their mutual interpretations of the task at hand. Note that about 35% of the reflective remarks and questions occur in the first and last 5 minute segments of sessions 1 and 2. Introductions and closings provide obvious opportunities to negotiate task properties. Because of this, the complete absence of reflective remarks in the first and last portions of interview 3 is especially notable. Overall, there are 190 meta-task related utterances, suggesting that a lot of work is being done in the first two interviews. Appendix A gives details and examples of this coding.

Utterances

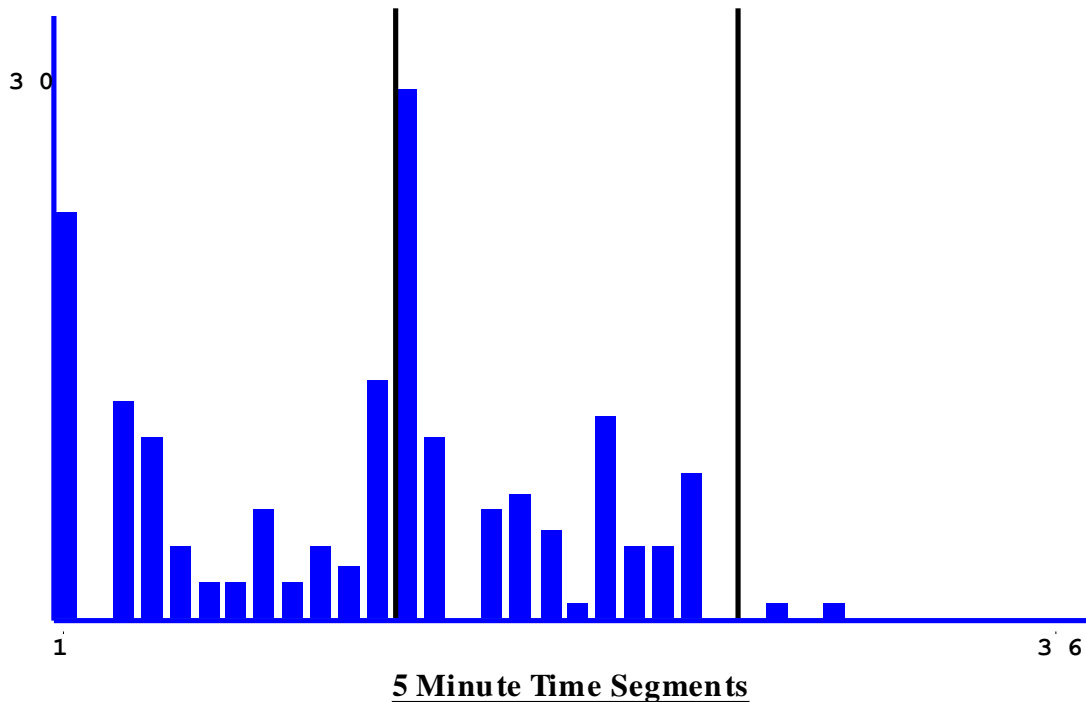


Figure 1. Utterances concerning the interview process.
First interview: 80. Second interview: 85. Third interview: 2.
Vertical rules mark session boundaries.

I have intended to establish the plausibility that: (1) J begins with expectable preconceptions about the activity type of the clinical interview. In particular, these concern authority, responsibility and rights to judgment that need to be sorted out. (2) Substantial work was done explicitly (mutually and explicitly by the interviewer and interviewee; implicitly by the interviewer in his focus on the interviewee's sense making) in moving toward a mutual inquiry. (3) The interviewer and interviewee, in fact, achieved an equilibrium and mutual understanding. We return, now, to the focal criticisms provided at the beginning of this article, around which most of my remaining analysis will be done.

White Room Investigation

The Central Hypothesis is (1) that inquiry and mutual inquiry are familiar, "natural" activity types in which people frequently and completely engage and (2) that clinical interviewing evolves, at least in part, from these, sharing important properties with them. At the beginning of a clinical interviewing sequence, of course, other activity types might plausibly be hypothesized by an interviewee. The interviewer, then, has a job to do in moving the interaction toward a legitimate mutual inquiry. The job can be accomplished using a wide variety of strategies. First, explicit commentary can be used. On the other hand, explicit comments are undoubtedly of limited value. In the first instance, if assertions are not supported by interviewer strategies that are consistent with mutual inquiry, explicit comments by the interviewer will likely be ignored. Second, the genuine orientation of the interviewer toward the sense that the individual is making

of the situation provides evidence to the interviewee that a mode of mutual inquiry is relevant. For example, (1) withholding judgment and (2) questioning the clarity and validity of a response only in proportion to its perceived ambiguity or problematic nature are two relevant strategies. (The relevant measures, of course, are ambiguity, etc., *as perceived by the interviewee*.) Finally, the prior experience and skill of the interviewer in understanding what may or a not seem sensible to interviewees will play a critical role in establishing a reasonable activity frame. If questions offered by the interviewer satisfy the Primary Constraint, then the interviewee should be drawn into the task at hand, understanding how things work.

Mutual inquiry in the clinical case, however, is subject to the two defining differences (interviewing assumptions) compared to naturally occurring versions, which may be explicitly or implicitly acknowledged by both interviewer and interviewee: (1) that the institutional purpose of the interview is to satisfy the interviewer's wish to understand how the interviewee makes sense of a set of problems; and (2) that it is the interviewer's responsibility to produce problems and questions relevant to this institutional aim.

As mentioned, additional evidence from J's protocols for these claims—in particular, on the issue of authority on plausibility judgments—will come in later sections. However, there are three other supporting lines of argument that I introduce here. These all have more or less to do with the specific analytic framework behind my studies of intuitive knowledge in physics. As such, they may not support claims for ecological validity for clinical interviewing in general. However, they are important to the present case, and they also help define a special class of clinical interviews for which ecological validity is less problematic. In addition, these considerations make the point that ecological validity is not a property only of a methodological technique, but, rather, of the larger research enterprise of which it is part.

My analysis of intuitive knowledge in physics is based significantly on knowledge elements called p-prims (diSessa, 1993). These are rather small elements of knowledge that people have induced from their constant experience with the world. There must be p-prims relating to social and other aspects of the world (diSessa, 2000), but these have not been the main focus of my investigation. P-prims explain people's expectations about which events are necessary, possible, or unlikely to occur in particular situations. In short, they account for judgments of causal relations. I hypothesize that everyone has an extensive "vocabulary" of p-prims in terms of which they see and sometimes explain the world. Even though these elements are abstract in a certain sense, they may be context-specific in subtle ways—at least they are subtle from the standpoint of instructed physics, which ignores many distinctions people make in everyday life.

Examples of p-prims include: the expectation that increased effort in an interaction, say pushing harder on a rock, creates a greater result (the rock moves farther or faster); the "belief" that a system remains stable (e.g., not moving) because it is "in balance"; the phenomenon that the greatest of influences usually "gets its way" when multiple influences with conflicting outcomes impinge ("overcoming").

An anecdote can make several properties of p-prims evident without undue dependence on understanding physics. My son reported to me that a friend of his who had just received her driver's license began driving up a steep hill. Rather than put the car in a lower gear, she surprisingly switched to a higher gear. When this failed miserably, she commented that she always thought that putting the car in a higher gear made it go faster. Indeed, her idea respects an important correlation between speed and gear, although "*allows* the car to go faster" (under certain circumstances) is a more robust connection than "*makes* it go faster." Abstracted

correlations that are taken to be causal and hence instrumental—like “higher gears make cars go faster”—are a common form for p-prims. The fact that these abstractions may frequently be applied outside of their normal range of applicability means that they appear sometimes as “misconceptions.” See diSessa (1993) and Smith, diSessa, and Rochelle (1993) for many examples and systematic discussion of these issues.

I contend that the program of studying p-prims is, in principle, ecologically valid. Why do people take the actions they do to satisfy their goals in situations of physical interaction? When are people surprised, and when do they find something they observe to be “natural”? When they are surprised, what do they think about? If queried, how do they explain their actions and the responses of the world? Of course, empirical methodologies may or may not adequately answer these questions.

In principle, we might be able to follow people around in their daily tasks and observe what they do. Surprise may sometimes be evident, but this will be comparatively rare both because p-prims are developed precisely to accord with everyday experience and because people are good at arranging their worlds to be predictable. Waiting until people “naturally” encounter all the situations that might be relevant to learning physics is a daunting task. In the flow of daily life, people seldom stop to think long even about surprising things, and would seldom trouble to explain these out loud.

A deeper problem lurks. How do we induce what people understand from what they do? Judging from everyday physical competence, we might believe people understand Newtonian mechanics, because that is how scientists account for what happens in these situations. But, a vast amount of research using multiple experimental techniques shows this is untenable (see Confrey, 1990, and Duit, Goldberg, & Niedderer, 1991 for reviews): People do not ordinarily think in terms of Newtonian mechanics.

What people might say about naturally occurring, everyday situations is helpful, but limited. People cannot tell us exactly why they have certain reactions, and most certainly they cannot explain exactly the circumstances under which they will and will not react in a particular way. As Ericsson & Simon (1984) noted, it is almost always useless to ask subjects to theorize about how they do something; that is our job as researchers. Instead, we use traces of subjects’ “ordinary” thinking patterns to triangulate on their knowledge and reasoning.

Clinical interviewing, I argue, is a cogent response to an ecologically valid research program. It respects the critical properties of richness, diversity, detail and inarticulateness that characterize intuitive knowledge of the physical world. Interviewers can propose many different problems, bring in props and prepared situations, focus selected attention on physical phenomena, and explore subtle variations of context that we could never hope to observe in ordinary life. Furthermore, we can explore the properties and nuances of the process of making sense of problematic situations far more than “in the real world.” Clinical interviews may well introduce artifacts and many issues that require attention. But other techniques (e.g., restricting to observations of actions “in the wild”) may render the phenomena we seek to understand completely intractable.

A second and probably more important context is relevant to judging the ecological validity of clinical interviewing in pursuit of understanding intuitive physical knowledge: schooling. If classrooms never engaged in sense-making activities, then clinical interviewing directed toward determining intuitive knowledge would have little educational face value. Indeed, in “traditional” classrooms it is not clear that students’ intuitive knowledge is very relevant. On the other hand, the huge literature on student misconceptions claims that difficulties in instruction can be traced

back to intuitive conceptions. More to the point, reform in mathematics and science education is currently directed overwhelmingly toward revising classrooms into sense-making contexts. Conversations similar to those that occur in clinical interviews are becoming more and more commonplace in school. They are also held up as models of good instruction (e.g., Minstrell, 1989; Lampert, 1990). In net, the activity type of mutual inquiry, *mutatis mutandis*, has a role in schooling, thereby enhancing the ecological validity of clinical interviewing. In a sense, school is a better connection for clinical interviewing than other examples of “everyday life.” Discussion and overt explanation are much more prominent and important in school compared to other forms of everyday life.

There is a significant and growing literature that uses analysis of intuitive ideas as the basis for designing classroom instruction. One notable example is the technique of bridging analogies (Clement, 1987; Brown & Clement, 1998), where one seeks to identify positive intuitive ideas and then to transfer them gradually into productive use in situations where students initially have counter-productive ideas. Another example of instructional design based on analysis of intuitive ideas is the work of Jim Minstrell. Minstrell organizes what he calls “benchmark lessons” by eliciting and orchestrating commentary about students’ intuitive ideas (diSessa & Minstrell, 1998). Minstrell has reported significant improvement in instructional outcomes through these methods (e.g., Hunt and Minstrell, 1994).

In net, this section argues that at least some clinical interviews arise from in-principle ecologically valid research programs, are tested and useful in important real-world contexts (classrooms), and constitute a sensible approach (to problems like the opacity of behavior and the relative paucity of everyday contexts in determining range of invariance of ideas) in making research issues of the program empirically tractable.

Coercive/Seductive Practices

According to the earlier description of goals of the interviewer in a clinical interaction, it is not in his/her interest to introduce or encourage responses outside of what are judged sensible by the interviewee. Of course, this does not guarantee instances of seductive or coercive practices do not occur, either intentionally or inadvertently. This section introduces four criteria that help evaluate the presence of goals and interactive relationships that support or undermine the intent of clinical interviews.

1. Is there independent evidence of conviction and self-generated thinking on the part of the interviewee?
2. Is it informationally possible that the interviewer “provided” a point of view to the interviewee?
3. Does the interviewee respond systematically to particular kinds of prompts (e.g., always changing position if a view is questioned), or do responses follow a more natural range (e.g., the interviewee sometimes accepts and sometimes refuses suggestions of the interviewer)?
4. Is there converging evidence for an interpretation of interviewee responses from multiple occasions, particularly if they occur in diverse interactional contexts (e.g., if an interpretation is spontaneously developed; approved after suggestion; if it seems to be supported or, in contrast, questioned by the interviewer; etc.)?

Let us examine interactions with J to see how these criteria play out. There is certainly evidence in J’s protocols that she is attending to my reaction, sometimes with the intent of

judging her own contributions. However, as suggested above, these are overwhelmingly in the transitional period during which she is coming to understand that this is a mutual inquiry. Substantial evidence later in her protocols suggests that she has turned to her own judgment as the primary factor in expressing opinions about how to think about the problematic situations posed to her. I begin here with a segment that shows her exercising significant judgment in correcting and refining her own ideas (criterion 1). In addition, this is one of quite a number of segments in which J produces very long stretches of uninterrupted reasoning. These stretches make it essentially informationally impossible that the interviewer could have “made her say” the things that she does (criterion 2). Instead, the logic in the exposition must be coming from her own ideas and judgments. Criterion 2 is a second case (compare IRE) where examining details of patterns that are characteristic of authority-dominated activity types can provide evidence concerning the nature of clinical interviewing. In particular, researchers have documented authority (teacher) influence on subordinate (student) responses in cases where (a) only selection, rather than generation, of alternatives is at issue, (b) there are sufficient indicative resources (e.g., a teacher stands next to or gazes toward the “right answer”). See above-cited references on IRE.

The following segment occurs at the beginning of the third interview session. Most of the first two sessions dealt with the question of warm summers and cold winters. This is the first mention of any trajectory problems, and it is not prefaced by any discussion about how to think about these problems. Here is J’s first description of how a ball tossed straight up into the air manages its ascent and descent. Her account is, in fact, correct and appropriate schooled physics.

[3 2:30]

J: Okay, um. Not including your hand, like if you just let it go up and come down, then the only force on that is gravity. And so it starts off with the most speed when it leaves your hand, and the higher it goes it slows down to the point where it stops, and then comes back down. And so, but the whole time the only force on that is the force of gravity, except the force of your hand when you catch it, and, when it starts off, um, it has its highest speed, which is all kinetic energy, and then when it stops it has all potential energy, no kinetic energy. And then it comes back down and it speeds up again.

Following a brief exchange of small talk, the interviewer asks for an elaboration.

[3 4]

I: Could you describe what happens at the very top of the toss?

J: Um, well, air resistance, when you’re throwing it, when you throw the ball up, the air, it’s going, I mean, it’s not against air because air is going every way, but the air force gets stronger and stronger to the point where when it stops, the gravity pulling down and the force pulling up are equal, so it’s in like equilibrium for a second. So it’s not going anywhere. And then, um, gravity pulls it back down. Like, when you throw it you’re giving it a force upward, but the force can only last so long against air and against gravity—actually probably more against gravity than against air—but, um, so you give this initial force and its going up just fine, slower and slower because gravity is pulling on it, and pulling on it. Um, then it gets to the point, to the top. And, then, um// It’s not getting any more energy to go up. You’re not giving any more forces, so the only force it has on it is gravity, and it comes right back down.

Clearly, J revised her initial description to change from a single force explanation of the toss to one in which there are two or three forces. She first introduces air friction or pressure, then she rejects that without any comment from the interviewer. She eventually settles on an explanation where the hand initiating the toss imposes a force on the ball that dies away gradually, coming into balance with gravity at the peak of the toss. As that force dies completely away, gravity overcomes it and causes the descent of the ball. A more complete analysis of this segment is given in diSessa (1996); a sketch of the analysis is given in diSessa (in press). As surprising as it may be, the two-force explanation of the toss has been described by many

researchers who use both clinical and non-clinical techniques (Viennot, 1979; Clement, 1983; McCloskey, 1983).

The density of ideas and subtlety of their relationships make it exceedingly implausible that the interviewer could have substantially influenced or orchestrated the interviewee's production. Instead, J is producing new ideas as she sees them relevant, reacting on-line to the plausibility of her explanation, and revising accordingly. Recall that she described just this process at the end of interview number one. In short, she is engaging in a sense-making inquiry.

This segment demonstrates one of the most important strategies of clinical interviewing. If we understand enough about the knowledge of our subject, then we can intervene in subtle ways to produce dramatic and surprising results. In this case, drawing J's attention to the peak of the toss was intended to highlight an aspect of the situation that should cue intuitive ideas of balancing. Hence, this intervention was made precisely because of the expectation that it might cause her to reformulate her explanation from involving only one force to an explanation with two competing forces.

"Coercion" can't apply to situations like this. "Seduction," in a certain sense, does. However, it is not seduction away from the subjects' personal judgments, but priming ways of thinking with shifts of attention toward aspects of the situation that might not have been attended to initially. If we regard subjects' thinking as rich and generative, it is only fair to examine the range of possible interpretations, given different contexts and emphases. It is simply a mistake to take either the initial or second analysis provided by J as "the way she thinks." In clinical analyses generally, neither the first nor the last explanation or judgment offered by an interviewee carries exclusive authority in "how she thinks." Instead, an analysis is made of the aspects of the situation that the subject is attending to in each phase, which account for initial announcements, final judgments, and shifts in between. This is an interpretive principle that helps assure "seductive" practices are interpreted always as responses to "seduction," rather than as "the way someone thinks." See the methodology section of diSessa (1993) for a discussion of the use of the dynamic of interaction to make judgments about intuitive knowledge.

In the following segments, J's integrity and commitment to her own ideas are evident in the interaction (criterion 1). In the first segment, I push and probe her commitment to the second, upward force (which according to schooled physics doesn't exist). If this interview were about giving correct answers, or even answers that may appear to be preferred (at least locally and hypothetically) by the interviewer, her staunch defense of her ideas is anomalous. The section begins by asking how the upward force that J has declared to balance gravity at the peak of the toss could actually balance if, as she has also said, it has completely died out at that point.

[2 24:30]

I: If it's gone though, how can it balance?

J: Well, at that second that it balances, which isn't very long, is when it's like it's on its last, you know, it's slowly, slowly dying out, and that one second is the one time when it's equal. So it's not gone at that second it's, obviously, or it else it would be// if it was gone that second, then it would have been falling earlier. And it would have been gone a little bit lower. But at that one, when it's at its peak, it's right before it's going to be gone.

I: Okay. That's right before, so its gone some time after it starts the downward//

J: Well, when it stopped is when they're equal, but obviously it dies at that exact point for gravity to pull it back down. So I guess you can say that at that point it goes away. But when it was completely stationary, it was still there enough to have it not fall. But, it's such a short amount of time, you know, it's not like it goes up and hangs out there for a while and then, oh, it dies out and comes back down. It goes up and it comes right back down. So its like a really short amount of time. Just dies out.

This “attack” on the upward force continued intermittently throughout the rest of the interviewing sessions. J showed no signs of giving in to arguments like this. In some cases, apparently, we may have very little to fear about disturbing or correcting interviewee’s ideas by subtle, or even less than subtle interaction. The stability of some intuitive knowledge against intervention and instruction has been extensively emphasized in the literature (McCloskey, 1983; Clement, 1982).

J’s final words at the very end of all interviewing sessions are illuminating. At this point, I have “unmasked” my interest in her two-force explanation. After I overtly explain to her that that is not sanctioned physics, and that I was interested in why she found it so compelling, she explains that, even after these extensive interactions and knowing full well that this is not a kind of explanation that would be offered in a physics book, she nonetheless finds the explanation reasonable. Here, she begins by referring to the fact that someone might have looked at a segment of video tape in which she espoused her balancing or canceling (two-force) explanation and believed that she did not understand. She ends by saying that she still believes she was making decent sense.

[7 77]

J: Right—I think—It’s funny, though, because I think that it would be easy for somebody watching that to think that I didn’t understand what was going on. And it’s funny, because I don’t think that now I understand what’s going on any better than I did then. But I can explain it to you in the right way. Which is, kind of, not frustrating, but it’s weird. I can leave and I can say, okay, I correctly said what was going on, but I don’t think I understand any differently. Like maybe I’m getting words confused. But I don’t think that I have this revelation “that’s how it works.” Because I still think I understood how it worked...

J’s commitment to her own ideas was not at all rare in her series of interviews. In the following brief selections, I show other instances where she refused a suggestion, which was, in fact, authoritative and correct physics because it simply did not seem sensible to her. Below, we are discussing the need of a force to keep objects moving. She believes a force is necessary. A schooled physics explanation is that “objects continue moving unless acted upon by an external force.” This is Newton’s first law, which is equivalent to realizing the existence of momentum. She dismisses this idea abruptly and unceremoniously when it is suggested. (It seems clear she feels free to do this because she does not recognize the interviewer’s comment as authoritative physics.)

[4 32:30]

I: So you said, the hard question is why is it moving if the forces are canceling out. Suppose I said that, well, things don’t need a reason to move, they just move. They just keep moving.

J: That wouldn’t fly well.

Later, in reaction to our mutual consideration of the *correct* Newtonian principle that motion from an unbalanced force must be accelerated, J says:

[4 38]

J: That just is not slightly possible.

Despite the above commitments, it would be a mistake to portray J as systematically and adamantly (perhaps defensively) defending her intuitive ideas. Criterion 3 recognizes that, in a mutual inquiry, subjects should be expected to respond with a range of reactions to questions and offered alternatives. Sometimes, the subject will be firm; other times wavering and uncertain. Some questions should be treated as occasions to elaborate prior commitments; some (like the request to examine the peak of the toss) should provoke rethinking. Some suggestions should seem to be reasonable alternatives, some not. More particularly, according to the theory of intuitive knowledge of which p-prims are a part (diSessa, 1993), systematic, adamant

commitment to all intuitive ideas would be anomalous, if not impossible. Some p-prims are *low priority* and cannot support reflective commitment.

In the following segment, J has been asked what will happen if a small weight is placed on one of two weights that are initially balanced on opposite ends of a string that runs over a pulley. J says at first that the side on which the extra weight is placed will move down a short distance, then stop. But, on being prompted to rethink, she changes her mind. This reaction contrasts sharply with her rejections of suggestions and persistence in the face of question, above. She explicitly refers to “making sense” as her reason for change.

[4 48]

I: So but, so but why would it come to rest? That's a little [inaudible]... why would it stop?

J: It actually... It wouldn't. It wouldn't stop. It would keep going slow, slow, slow all the way down. It would not stop.

I: So you changed your mind.

J: Changed my mind. And if it had a less, smaller weight, it wouldn't stop closer, it would just go slower the whole way down.

I: Can you say what made you change your mind?

J: 'Cause it didn't make any sense.

I: 'Cause it didn't make any sense?

J: No. I don't know why I said that.

A short time later, J says about her initial guess:

[4 49:30]

J: I mean, I know that that wouldn't happen.

I: So you're a hundred percent sure.

J: I'm never a hundred percent sure, [but] I'm taking back what I've said. But, I think it would just keep going down.

I: So that makes//

J: More sense.

I: That makes more sense. Okay.

All in all, J shows an expectable spread in commitment toward her own ideas. Some seem patently obvious and incontrovertible; she defends these staunchly through extended “attack.” Some are glimmering intuitions that fade quickly. While standing her own ground suggests commitment, unrelentingly doing so would be pathological or suspicious, suggesting an antagonistic or defensive relationship had been achieved, rather than a mutual inquiry.

Triangulating evidence from multiple sources and occasions (criterion 4) is a very general strategy that works for clinical interviewing just as it does for other methodologies. In fact, revisiting an interviewee's interpretation from multiple perspectives is especially characteristic of clinical interviews. J's two-force model of a toss was an extreme example in that we returned to explore options and possibilities many times over the course of the five interviews after she originally produced it. Each occasion provides more detail and constraints on how we understand J's understanding of forces and the toss situation. I will not review this process except to say that there is consistent data that J felt the two-force model was sensible, not a reaction to, for example, an anomalous interpretation by J that the interviewer wanted “something different” than her first response. [For a more complete accounting, see diSessa, 1996; diSessa & Sherin, 1998; and diSessa, Elby, & Sherin, 2002.]

Beyond data from a single subject, triangulation across multiple subjects helps. The research community as a whole has gradually come to understand which are fundamental commitments of intuitive physics, and which are more peripheral. We can be more confident of student reactions when they align with these general results, and become more suspicious of the

interactions that display anomalous commitment or lack of it. A case in point is that the idea of a force supplied to an object in a toss (which characterized J's persistent two-force model) is well known to be common and extremely resilient. J's persistence in this idea is not particularly anomalous, and should not raise undue suspicion of artifacts in the interview.

Empirically supported theoretical frameworks can help, beyond purely empirical triangulation. In this case, my p-prim based interpretation of her model includes the fact that balance is highly salient and explanatory for J. In other segments of interview, J showed a deep commitment to balancing as a way of explaining many physical phenomena that are quite different from the toss. This makes it expectable that she should develop the two-force model, partly explains why she finds it so attractive, and what it means that she does. The role that balancing plays in J's thinking will be elaborated below while considering the question of invariance of knowledge deduced from clinical data.

To conclude this section, I note that in a general sense, I (the interviewer) do "make J say what she says." I provide contexts and probing questions that she is very unlikely to encounter in everyday life. However, because we have established a relationship of mutual inquiry, her reactions are substantially her own. They are not scripted or coerced by the interviewer. The extended nature of the inquiry I foster for J is far beyond what she could muster on her own, or in likely collaboration with a peer. Being able to support such an extended inquiry, in fact, as suggested earlier, marks a special contribution of the interviewer. Attributing macro-inquiry methods to the interviewee would be a mistake. This long time-scale, "rational" aspect of the inquiry may be an excellent example of operating in a "zone of proximal development," supported by the interviewer. It may be more like what might happen in a sense-making classroom than in other corners of "everyday life."

Ecological validity does not necessarily mean "just like everyday life." Instead, ecological validity means that (1) we understand the relationships of the research circumstances to those in which the knowledge we investigate originated and is usually used, and (2) we understand the relationships between the research circumstances and those in which we intend to use what we discover, perhaps in designing effective instruction.

Questionable Invariance of Knowledge

Every scientific research program and methodology has an implicit or explicit metaphysics, so to speak. It has commitments about the nature of the phenomena investigated and about the nature of explanations that should be offered about them. Research programs that employ clinical interviewing are not exceptions. While these commitments are difficult to discuss owing to their implicitness and because it is not easy to find common ground across different paradigms, I do not wish to evade these issues entirely. I hope to provide rough but consequential and challengeable descriptions of the epistemological commitments underlying (some versions of) clinical interviewing. These will be focused on the specific issue of what sort of claims we may make about the invariance of "knowledge" that we might try to "read out" from an analysis of interviewing data.

Over the long haul, I believe that articulated and detailed process models of thinking may be the best way to settle these issues. The work of Ericsson and Simon (1984) is a case in point. They argue for the validity of interpreting particular aspects of verbal protocols on the basis of a model of what "verbalizing knowledge" means. I do not mean to endorse that particular model, or provide details of an alternative. However, I do mean to indicate how I believe the issues

treated below may best be settled, eventually. In this regard, see also Messick, cited in Ginsburg (1997).

Particular epistemological aspects of my theoretical work on intuitive knowledge, into which program my own clinical interviewing plays, will be relevant to the following discussion. That means, again, my defense of clinical interviewing is less comprehensive than may be desired. Seen as a restriction in claims, this may seem to be a weakness. On the other hand, warranting the use of clinical interviewing beyond a reasonable range may be a worse danger.

A contested notion of knowledge lies behind criticisms of clinical interviewing on the basis of questionable invariance. Let me first, briefly, elaborate what this means. Then I will stake and defend a position that warrants the use of clinical interviewing data *in particular ways*.

I can find a useful starting point in parodies of extreme epistemological positions with respect to the nature of knowledge. I use these rhetorically, without meaning to treat them as serious characterizations of existing paradigms.

One image of knowledge may take its *reactive nature* as central. So, for example, one might emphasize the fact that individuals may be constantly in interaction with their social and material environment. In paddling a canoe down rapids in a river, a person may be constantly, inarticulately, and “non-cognitively” reacting to ever changing aspects of the environment. A contrasting image of knowledge may emphasize *autonomy*. An individual may operate out of a pre-designed plan, or, in any case, without major effect of external circumstances.

Within the first image, knowledge may be seen as assembled “on the spot” out of both the individual’s predilections and out of the details of the local material and/or social environment. In extreme cases, one might deny knowledge, in the sense of something belonging to an individual, altogether. A revised notion of knowledge might be fundamentally relational in the sense that mention of people and social and material environments *must* accompany any description of “knowledge.”

Within the second image emphasizing autonomy, knowledge drives behavior in an unproblematic way. In fact, knowledge should generally be much easier to “read out” within this view since its contingency on the particulars of the situation may be minimal.

Let me rephrase this caricatured dichotomy in terms of the issue of invariance of knowledge. In a reactive view, invariance is highly problematic. Even if the notion of knowledge is allowed, it may be created on the spot. We have no warrant to expect that “the same” knowledge should be applied in other circumstances. Other circumstances might generate other knowledge. In the autonomous view of knowledge, invariance is unproblematic and perhaps even universal. “A subject, S, understands knowledge, K, if S uses K whenever appropriate.” In this quotation (Moore & Newell, 1973), knowledge only counts as knowledge if it is universal and is applied “whenever appropriate.”

Here are the defects I see in these caricatures, which I use to motivate the position I stake for clinical interviewing. The immediate problem with the autonomous view is easier to state. Knowledge, to be functional for a biological organism like a person, must be reactive. Thus, the point of having knowledge is to be able to negotiate circumstances. Unless knowledge is adapted to these circumstances, it should not even count as knowledge. Even the extreme image of an autonomous thinker carrying out a preformed plan makes little sense unless the thinker created the plan in reaction to circumstances that need to be negotiated. Identifying the plan itself as “the knowledge the individual has” would be foolish.

The problem with the pure reactive view of knowledge is that it affords no explanation for regularity in human behavior. If we start from the presumption that a psychological investigation

is pursuing regularity in human activity, the reactive view, as far as I have specified it, has no explanation for such regularity. We might ask, what are the (mental, physical, social) materials out of which, and via what processes, are the circumstances of the canoe paddler transformed into “knowledge created on the spot.” How do we explain the reliable differences between an expert canoeist and a novice? Without presuming that we know a priori any of its other properties, we could *define* knowledge as whatever an individual may acquire (or, possibly, inherit), which explains regularities involving the same individual in different circumstances.

In a nutshell, the functional niche of “explaining regularity” is occupied in cognitive research, and also in mine, by identifying knowledge that one attributes to individuals, along with (of course) its contingent relations with contexts and situations. Knowledge is the invariant that accounts for regularity across different circumstances (but not across all circumstances). I believe this functional niche is inescapable, so other paradigms must have substitute constructs to deal with it. “Affordances and constraints” (Greeno, 1994) may be alternate constructions that occupy the same niche, provided we interpret them as things which people can acquire (or, to which they can become “attuned”).

The meaning of “knowledge explains regularity by its invariance” is critical here. It does not mean that anything identified as knowledge automatically has a warrant of invariance across an unspecified or unbounded range of circumstances. It means that anything that is claimed as knowledge must include demonstration of invariance across a specified range of circumstances. In this way, identifying knowledge forces the twin issues of regularity *and* relevance. In diSessa (1993) I described the “principle of invariance” as follows:

This is a general principle that, if one gets a description right, the p-prim [knowledge element] (or any theoretical construct) will apply in all implicated contexts. So if a p-prim appears to be used in situations in which it is not evidently applicable for us as theorists, some redescription of the p-prim may be in order. Similarly, if a p-prim is not observed to be used in a context in which it should be, given its current description, problems in p-prim description are indicated. (p. 124)

The principle of invariance applied to clinical interviewing says that our description of knowledge must, implicitly or explicitly, delimit the contextual factors that determine whether that knowledge will be applied or not. The principle forces “situating” the knowledge carefully.⁵

Let me illustrate the principle of invariance and how it plays out in clinical interviewing with two examples. The first is an example of non-invariance. A proposed knowledge structure fails the test of invariance under clinical interviewing conditions. The second example is a knowledge proposal that survives at least a first round of tests in a clinical context. The intent of these examples is to illustrate a responsible approach to the problem of invariance, and how that works out in a clinical context. I do not intend to prove any of the interpretations offered.

J’s second description of the toss, which she achieved gradually in the segment quoted in the section on coercive and seductive practices [3 4], goes something like this. When a ball is tossed into the air, your hand imparts a force to it that drives the ball up against gravity. That force gradually dies away. Eventually, it is only strong enough to balance gravity, at the peak. Then, as the upward force continues to wane, gravity takes over and causes the downward fall.

In the research literature, this description has been given a high level of prominence. McCloskey (1983) declared that, suitably abstracted, it constituted “an intuitive theory of

⁵ The principle of invariance is strong enough to force revisions in basic conceptions of the nature of knowledge. For example, I have argued that invariance forces a reconceptualization of knowledge to a “subconceptual” level in order to account for apparent contextual dependencies in the way students reason about physical systems using experiential knowledge (diSessa, 1996; diSessa, in press).

physics.” The “impetus theory” includes, centrally, that any toss or violent propelling of an object induces a force in it that gradually dies away and loses its influence.

The selection quoted where J gives this description constitutes a triple failure of the claim that “J has the impetus theory” by the principle of invariance. First, she had just produced an alternative description using only one force, a force of gravity. The impetus theory does not explain the apparent non-use of the theory in the first explanation and its subsequent use in the second explanation.

Second, J continued to alternate uses of the one-force model and two-force model over the remaining hours of interview series. In particular, she produced faultless, physically correct descriptions of a toss in the context of using a computer simulation (diSessa, 1996). Subsequently, she reverted once again to the (incorrect) two-force explanation of the toss. Every switch is unexplained by the assertion, “J has the impetus theory.”

Third, the details of the emergence of the second explanation are a failure of invariance in the dynamic of her explanation. J does not invoke the impetus theory as a whole. She begins with alternate ideas about a force competitive with gravity (air friction or pressure) and only later rejects these in favor of an imparted force.⁶ At minimum, we need an account of the pieces out of which she assembles the impetus theory and the principles of assembly.

I can provide a sketch of how I have resolved this difficulty based on other information that I provided earlier about J and the toss problem. The critical observation is that J attends systematically to features of the world not explicitly mentioned in the “impetus theory.” In fact, these features are irrelevant to *real* physics, as well. In particular, she attends to *competition*, *balance* and *overcoming* as explanatory schemes. So, when balance or competition is seen by her to be at issue, J is very likely to “invent” extra forces to participate. When these features are not evident, J does not invoke an impetus force. Just because one pushes an object is insufficient reason to impute the transfer of a force to the object.

Entering the interview with J, I was aware that competition and balancing are important features to students (from interviews with other students). So I deliberately highlighted features that would cue balancing—the peak of the toss is a moment of stability—expecting J might revise her explanation in that light. In J’s own words, a stopped state “looks like balance.”

[7 69]

J: The reason [*she introduced the second force*] is because I usually associate no movement with things like sums of forces being zero. Or something from one side equaling something from the other side. And so I just associated the ball stopping in mid-air to the leftover force you gave it equaling gravity.

The original sequence during which J produced an “impetus” explanation is telling. J *first* presumes another force exists to balance gravity—air friction—then eventually settles on “the force you gave it in the toss.”

Implicating attention to competition, balance and overcoming brings an obligation, by the principle of invariance, to demonstrate the range of application for *these* ideas. From the corpus of J’s interviews, it is easy to draw many examples of her use of these ideas *outside* of a toss situation, and *independent of* an “impetus force.” Appendix B shows some of these.

Let me review the main points. Far from assuming an extravagant range of invariance of knowledge “read out,” clinical interviewing appropriately forces the question of invariance as a warrant for knowledge descriptions. Because of the intensive nature of the interaction and the

⁶ Of course, J could be re-inventing the idea, as opposed to originating it here. But that description still doesn’t explain what J is doing in this instance of (re-)construction, and it doesn’t explain where the idea could have originated in the first place, which the details of this (re-)construction partially can.

socially sanctioned prerogative of the interviewer to revisit a situation under slightly different (either explicitly suggested or implicitly induced) orientations, a large amount of data about possible reactions—both robust and fragile ones—are produced. For this reason, clinical interviewing might be an extremely well-adapted methodology for views of human cognition that emphasize the situatedness of human action. As I mentioned, collecting data that could support such refinement “in the wild” would be, for all practical purposes, impossible.

In this case, extensive data from several interviews strongly undermine the contention that “the impetus theory” has sufficient integrity, or is described well enough (including contextualities of when it will and will not be used) to count as the description of a “piece of knowledge” in J’s conceptual ecology. The impetus theory fails the test of the principle of invariance: Without understanding J’s attention to competition, balancing and overcoming, we cannot understand when she will and when she will not use the “theory.” It is worth noting that, in general, no single occurrence of a behavior or explanation during a clinical interview provides any information at all about invariance except in combination with other instances. In addition, it should be evident that by “situation” in “knowledge is what is invariant across situations,” we must mean what becomes salient and regarded as relevant by the subject—not just who and what are present and what their relationships and habitual interactions are.

I wish to treat one other aspect of the issue of invariance in clinical interviewing. How do we square “attributing (stable) knowledge” to an individual with the fact that people are almost always learning?

The fact of learning does not preclude regularities in behavior. Learning frequently is slow and extended. I have indicated how persistent J was with her two-force description of the toss, even if she alternated it with other descriptions based on attention to different aspects of the toss. Even if she did “learn” to consistently apply the physicist’s one-force model—perhaps in the closing moments of the interview sequence—her attention to competition, balancing, and overcoming were amply demonstrated in many different circumstances over the whole course of the interviews. Invariance does not mean invariance over all time no more than it means invariance over all circumstances.

More generally, adequate descriptions of knowledge provide the basis for understanding learning; they do not preclude the possibility of adequately handling change. P-prims can change their effect by shifting the context in which they are evoked, even if they maintain their internal structure. In other places (e.g., diSessa, in press), I have provided extensive argument that this is a productive way of looking at the learning of schooled physics. For example, the balancing p-prims that J uses eventually become restricted to use in contexts where sanctioned physical principles like energy conservation apply, not in situations where rest indicates a balance (diSessa, 1993). See also the description of how students use balancing in generating correct equations in Sherin (2001). Limited invariance is frequently an essential backdrop to describing change. (See also diSessa & Sherin, 1998; diSessa, 2004b).

Summary

In this article I have sought to understand the nature of clinical interviewing as an interactional event. The intent has been to describe clinical interviewing’s *intrinsic* characteristics (such as internal logic and constraints), rather than *extrinsic* ones (such as objectivity and repeatability). I have hoped, thereby, to understand more about how clinical interviewing works, what dangers lie

in it, how we may protect against those dangers, and what sort of conclusions are warranted from clinical data.

I organized this article as a defense against three classes of criticisms of the clinical process. First, one may question its ecological validity. Second, one may suspect coercive and seductive practices that undermine attributions to the interviewee. Third, one may worry that knowledge “determined” in a clinical context will not “travel” and be used in other contexts.

The Central Hypothesis of the article is that clinical interaction is derivative of natural forms of engagement with which people are already familiar. In particular, I claim that individual or mutual *inquiry as to how one should think about various situations* is commonplace. In the best circumstances, an interviewer and interviewee gradually negotiate an activity type that overlaps substantially with naturally occurring inquiry. In particular, the central goal of the interviewee is to make sense as best he or she can of the situations or problems proposed by the interviewer. A secondary goal for the interviewee is to help the interviewer understand the sense he or she makes.

In contrast to naturally occurring mutual inquiry, the interviewee cedes to the interviewer large-scale issues such as the topic discussed and how, in broad terms, it should be approached. The interviewer generally cedes rights of judgment as to the plausibility or sensibility of the interviewee’s descriptions. The Primary Constraint of the relation between interviewer and interviewee is that the interviewer must propose problems or issues that the interviewee sees as reasonable grounds for inquiry.

Taken together, these hypotheses undermine general suspicion of the validity of data collected in clinical settings. As a derivative of naturally occurring forms of activity, and in its similarity to certain (reform-oriented) school practices, its ecological validity is not immediately problematic. Other hypotheses, notably the Primary Constraint, separate “successful” clinical interviews from those that are not successful. Only successful ones gain legitimacy from the arguments made here. The use of one seven-hour corpus of interview data establishes some degree of plausibility for all the hypotheses, and aims to demonstrate that “successful” clinical interviews are possible. Most important, the analyses here suggest that the claims and distinctions made here are empirically tractable and may set a ground for further empirical work.

To conclude, I return to the opening quotation that framed this inquiry, inserting responses as they were developed here.

... [T]hese studies tend to analyze individuals without reference to their community, or their history, performing on a task designed by the experimenter in an unfamiliar environment.

At least one aspect of subjects’ background is critical. He or she must have familiarity with individual and/or mutual inquiry (and the general activity framework of interviewing) and have the resources to adapt this to the constraints of the clinical interview. The room may be unfamiliar, but if the task is sensible to the interviewee, the interview may have a degree of ecological validity.

The “problem” is defined and valued by the experimenter, not by the subject, who is then expected to perform in certain ways.

The problem, in a certain sense, is defined by the interviewer. But the interviewee retains all rights relevant to judgments of sensibility of the problem and any interpretations of it that are proposed. The Primary Constraint implies that it is a failure of the interviewer, not of the interviewee, if the task does not make sense. Furthermore, the sensibility of the task is open to empirical examination. The obligation of the interviewee to perform “in certain ways” is limited to following the interviewer’s suggestions, *as much as these are regarded sensible*. The

interview fails if the interviewee takes the task to be to understand the proposed situations according to other than his/her own judgments.

In some experimental manipulations, even the very nature of the task, or the required behavior, may not be clear to the subject.

The nature of the task as an inquiry must become clear to the interviewee. This contingency is open to empirical test.

The question of how “subjects” make sense of the game in which they are playing, trying to discover the “rules of the game”—that is, what the experimenter is after—is often not explicitly discussed in these studies.

The “rules of the game” are always discussed with the interviewee in the genre of clinical interviewing analyzed here. Furthermore, whether the interviewee operates according to the expected rules of the game is open to empirical test. This article aims to make a contribution precisely in facilitating further discussion of the “rules of the game” of clinical interviewing and in opening operationalizations in observations of clinical interactions.

Performance is measured relative to a certain “ideal,” rational model of problem solving, and the deviations of subjects from this abstract logic are noted.

Deviations from, for example, correct physics may be noted. However, this is far from providing an account of the sense that interviewees make of the proposed topics—or ranges of invariance—which is the goal of an interview. This point implicates the larger research program, of which clinical interviewing is a part.

It is presumed that the fundamental mental mechanisms posited to underlie human behavior in such prescribed domains can later be extended, without major modification, to more real-world activities.

This is not a presumption but an accountability that the analyst of clinical data should take on.

So the assumption is that “problem solving” is a generic cognitive activity that has a similar form across a quite variety of domains, from acting in a psychology experiment to everyday cognitive activities.

“Problem solving,” per se, is not particularly relevant to the kind of interviewing discussed here. On the other hand, the more general issue of the relevance of analysis of clinical data to everyday cognition, and in particular, to learning in schools, is explicitly addressed here. I have tried to warrant particularly my own program of studies using clinical interviewing as a responsible and reasonable way of getting data that is useful in thinking about ecologically “natural” contexts.

It is also usually assumed, implicitly, that this activity is located “in the head” of the individual.

Presuming some aspects of the activity take place in the head is, in my view, unproblematic. What is more important is that social, physical and intellectual context and contingency is an explicit part of the analysis. I have tried to show that clinical interviewing provides rich data that deals directly with the issue of invariance of knowledge. More importantly, presumptions that are made are amenable to empirical test.

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References

- Bakhtin, M. M. (1986). The problem of speech genres. In C. Emerson & M. Holquist (Eds.) *Speech genres and other late essays* (pp. 60-102). (Trans. V. W. McGee). Austin, Texas: University of Texas Press.
- Bannon, L., & Bødker, S. (1991). Beyond the interface: Encountering artifacts in use. In J. M. Carroll (Ed.), *Designing interaction: Psychology and the human-computer interface* (pp. 227-253). NY; Cambridge, UK: Cambridge University Press.
- Brown, D., & Clement, J. (1998). Overcoming misconceptions via analogical reasoning: Abstract transfer versus explanatory model construction. *Instructional Science*, 18, 55-81.
- Brown, J. S., Collins, A., & Duguid, P. (1989). Situated knowledge and the culture of learning. *Educational Researcher*, 18(1), 32-42.
- Clark, H. H., & Schaefer, E. F. (1989). Contributing to discourse. *Cognitive Science*, 13, 259-294.
- Clement, J. (1982). Student's preconceptions in introductory mechanics. *American Journal of Physics*, 50, 66-70.
- Clement, J. (1983). A conceptual model discussed by Galileo and used intuitively by physics students. In D. Gentner and A. Stevens (Eds.), *Mental models* (pp. 325-340). Hillsdale, NJ: Lawrence Erlbaum Associates.
- Clement, J. (1987). Overcoming students' misconceptions in physics: The role of anchoring intuitions and analogical validity. In J. D. Novak (Ed.), *Proceedings of the second international seminar on misconceptions and educational strategies in science and mathematics* (pp. 84-97). Ithaca, NY: Cornell University.
- Clement, J. (2000). Analysis of clinical interviews: Foundations and model viability. In A. Kelly & R. Lesh (Eds.), *Handbook of research design in mathematics and science education* (pp. 547-589). Mahwah, NJ: Lawrence Erlbaum Associates.
- Confrey, J. (1990). A review of the research on student conceptions in mathematics, science and programming. In C. Cazden (Ed.), *Review of research in education* (Vol. 16, pp. 3-56). Washington, DC: American Educational Research Association.
- diSessa, A. A. (1983). Phenomenology and the evolution of intuition. In D. Gentner and A. Stevens (Eds.), *Mental Models* (pp. 15-33). Mahwah, NJ: Lawrence Erlbaum Associates.
- diSessa, A. A. (1993). Toward an epistemology of physics. *Cognition and Instruction*, 10(2-3), 105-225.
- diSessa, A. A. (1994). Speculations on the foundations of knowledge and intelligence. In D. Tirosh (Ed.), *Implicit and explicit knowledge: An educational approach* (pp. 1-54). Norwood, NJ: Ablex.
- diSessa, A. A. (1996). What do "just plain folk" know about physics? In D. R. Olson and N. Torrance (Eds.), *The handbook of education and human development: New models of learning, teaching, and schooling* (pp. 709-730). Oxford, UK: Blackwell Publishers, Ltd.
- diSessa, A. A. (2000). Does the mind know the difference between the physical and social worlds? In L. Nucci, G. Saxe, and E. Turiel (Eds.), *Culture, Development and Knowledge* (pp. 141-166). Mahwah, NJ: Lawrence Erlbaum Associates.
- diSessa, A. A. (2004a). How should we go about attributing knowledge to students? In E. Redish and M. Vicentini (eds.), *Proceedings of the International School of Physics "Enrico Fermi": Research on physics education* (pp. 117-135). Amsterdam: ISO Press/Italian Physics Society.

- diSessa, A. A. (2004b). Contextuality and coordination in conceptual change. In E. Redish and M. Vicentini (eds.), *Proceedings of the International School of Physics "Enrico Fermi: Research on physics education* (pp. 137-156). Amsterdam: ISO Press/Italian Physics Society.
- diSessa, A. A. (in press). A bird's eye view of "pieces" vs. "coherence" controversy. In S. Vosniadou (ed.), *Handbook of conceptual change research*. Mahwah, NJ: Lawrence Erlbaum Associates.
- diSessa, A. A., Elby, A., & Hammer, D. (2002). J's epistemological stance and strategies. In G. Sinatra and P. Pintrich (Eds.), *Intentional conceptual change* (pp. 237-290). Mahwah, NJ: Lawrence Erlbaum Associates.
- diSessa, A. A., & Minstrell, J. (1998). Cultivating conceptual change with benchmark lessons. In J. G. Greeno & S. Goldman (Eds.), *Thinking practices* (pp. 155-187). Hillsdale, NJ: Lawrence Erlbaum Associates.
- diSessa, A. A., & Sherin, B. (1998). What changes in conceptual change? *International Journal of Science Education*, 20(10), 1155-1191.
- Duit, R., Goldberg, F., & Niedderer, H. (1991). *Research in physics learning: Theoretical and empirical studies*. Kiel, Germany: IPN.
- Ericsson, K. A., & Simon, H. (1984). *Protocol analysis: Verbal reports as data*. Cambridge, MA: MIT Press.
- Ginsburg, H. (1997). *Entering the child's mind: The clinical interview in psychological research and practice*. Cambridge, UK; New York: Cambridge University Press
- Goffman, E. (1981). *Forms of talk*. Philadelphia: University of Pennsylvania Press.
- Gopnik, A., & Wellman, H. M. (1994). The theory theory. In L. A. Hirshfeld & S. A. Gelman (Eds.), *Mapping the mind: Domain specificity in cognition and culture* (pp. 257-293). Cambridge, UK; NY: Cambridge University Press.
- Greeno, J. G. (1994). Gibson's affordances. *Psychological Review*, 101(2), 336-342.
- Hymes, D. (1974, 1989). Ways of speaking. In R. Bauman & J. Sherzer (Eds.), *Explorations in the ethnography of speaking*, Second Edition (pp. 433-451). Cambridge, UK: Cambridge University Press.
- Hymes, D. (1974). *Foundations in sociolinguistics*. Philadelphia: University of Pennsylvania Press.
- Hunt, E., & Minstrell, J. (1994). A cognitive approach to the teaching of physics. In McGilley, K. (Ed.) *Classroom lessons: Integrating cognitive theory and classroom practice* (pp. 51-74). Cambridge: MIT Press.
- Inagaki, K., & Hatano, G. (2002). *Young children's naïve thinking about the biological world*. New York: Psychology Press.
- Laboratory for Comparative Human Cognition (1982). Culture and intelligence. In R.J. Sternberg and D.K. Detterman (Eds.), *Handbook of human intelligence* (pp. 642-719). New York: Free Press.
- Lampert, M. (1990). When the problem is not the question and the solution is not the answer: Mathematical knowing and teaching. *American Educational Research Journal*, 27(1), 29-63.
- Lave, J. (1988). *Cognition in practice*. Cambridge, UK: Cambridge University Press.
- Lemke, J. L. (1990). *Talking science: Language and values*. Norwood, NJ: Ablex.
- Levinson, S. C. (1993). Activity types and language. In P. Drew and J. Heritage, *Talk at work: Interaction in institutional settings* (pp. 66-100). Cambridge, UK: Cambridge University Press.

- Loftus, E. (1980). *Memory: Surprising New Insights into How We Remember and Why We Forget*. Reading, MA: Addison-Wesley Publishing Company.
- Marton, F. (1981). Phenomenography—Describing conceptions of the world around us. *Instructional Science*, 10, 177-200.
- McCloskey, M. (1983). Naive theories of motion. In D. Gentner and A. Stevens (Eds.), *Mental models* (pp. 299-324). Hillsdale, NJ: Lawrence Erlbaum Associates.
- Mehan, H. (1979). *Learning lessons*. Cambridge, MA: Harvard University Press.
- Minstrell, J. (1989). Teaching science for understanding. In L. B. Resnick & L. E. Klopfer (Eds.), *Toward the thinking curriculum: Current cognitive research* (pp. 129-149). Alexandria, VA: Association for Supervision and Curriculum Development.
- Moore, J., & Newell, A. (1973). How can Merlin understand? In L. Gregg (Ed.), *Knowledge and cognition*. Mahwah, NJ: Lawrence Erlbaum Associates.
- Ochs, E., Taylor, C., Rudolph, D., and Smith, R. (1992). Storytelling as a theory-building activity. *Discourse Process*, 15, 37-72.
- Schegloff, E. A. (1993). On talk and its institutional occasions. In P. Drew and J. Heritage (Eds.), *Talk at work: Interaction in institutional settings* (pp. 101-134). Cambridge, UK: Cambridge University Press.
- Sherin, B. (2001). How students understand physics equations. *Cognition and Instruction*, 19(4), 479-541
- Sinclair, J. M., & Coulthard, M. R. (1975). *Towards an analysis of discourse: The English used by teachers and pupils*. London: Oxford University Press.
- Smith, J. P., diSessa, A. A., & Roschelle, J. (1993). Misconceptions reconceived: A constructivist analysis of knowledge in transition. *Journal of the Learning Sciences*, 3(2), 115-163.
- Steffe, L. (1991). The constructivist teaching experiment: Illustrations and implications. In E. von Glasersfeld (Ed.), *Radical constructivism in mathematics education* (pp. 177-194). Dordrecht/Boxton/London: Kluwer.
- Suchman, L. A. (1987). *Plans and situated actions: The problem of human-machine communication*. Cambridge, UK; New York: Cambridge University Press.
- Viennot, L. (1979). *La raisonnement spontané en dynamique élémentaire* [Spontaneous reasoning in elementary mechanics]. Paris: Hermann.
- Wells, G. (1993). Reevaluating the IRF sequence: A proposal for the articulation of theories of activity and discourse for the analysis of teaching and learning in the classroom. *Linguistics and Education*, 5, 1-37.

Appendix A: Coding J's Consideration of the Interview's Activity Type

The first three of J's interviews were coded for statements that were questions or comments concerning the nature and purpose of the interview. Units of coding were utterances—sentences, or sentence fragments that expressed recognizably complete thoughts.

Three grades of such utterances were coded, representing a range from (I) most direct and contentful, most securely interpretable expressions, to (III) least direct, least clearly relevant expressions. The following rubric provides detail:

Grade I: Clear expression/question concerning the nature of the general task or relationship between interviewer and interviewee. Grade I includes clearly worried or self-deprecating comments and any comments invoking judgments of interviewee's statements by the interviewer or by a third party. Comments directly about the plausibility of a description or interpretation are excluded; these are interpreted as a normal part of inquiry.

Examples:

[1 20]

J: So I'm thinking I'm not going to be able to answer anything, and am I going to feel even more dumb?

...

I: We are interested in the way people think....We're not interested in right or wrong. [2 utterances]

[1 12]

J: Am I supposed to give a physical answer to that?

I: Go ahead and give an answer that makes sense to you. ... We want to talk about what makes sense to you. [2 utterances]

[1 55]

J: I just have to get over, personally, thinking that if I say all these wrong things this person's looking at me thinking I'm a total idiot.

[2 3]

J: I left here feeling pretty good; that's kind of neat. I just thought about things for an hour. [2 utterances]

Grade II: Utterances that are not direct comments about the interviewing process or interviewer/interviewee relationship, but are used as support in an extended commentary about these issues. Acknowledgements of comments in such exchanges. Ambiguous comments about interviewee/interviewer exchange, for example, when interviewer and interviewee are "role playing" sides in an argument.

[1 1]

J: So I decided to come [to the interview] to see what it was like.

[2 40]

J: Does that answer your question?

Grade III: Vaguely targeted utterances, possibly about the interviewing process or about external judgment. For example, laughs interpretable as self-conscious laughs.

[1 24:50]

J: I just made up this theory, which is obviously a little weird.

[3 8:20] After several questions about what happens at the peak of the toss, Interviewer asks "is that what it is?" and J laughs as if to express uncertainty.

J: Yeah, I guess so. ... Yeah, I think so.

Table B1 give the breakdown of utterances by interview and grade.

Table B1. Utterances about the interviewing process, by grade and interview.

	I	II	III	Total
Interview 1:	55	14	11	80
Interview 2:	54	30	1	85
Interview 3:	0	0	2	2

Notice that the only two codable utterances in interview 3 are low grade (III). The graph in the text (Figure 1) shows breakdown (totals of I, II and III) by five minute intervals. Figure A1 shows the breakdown collapsed to 15 minute intervals.

Utterances

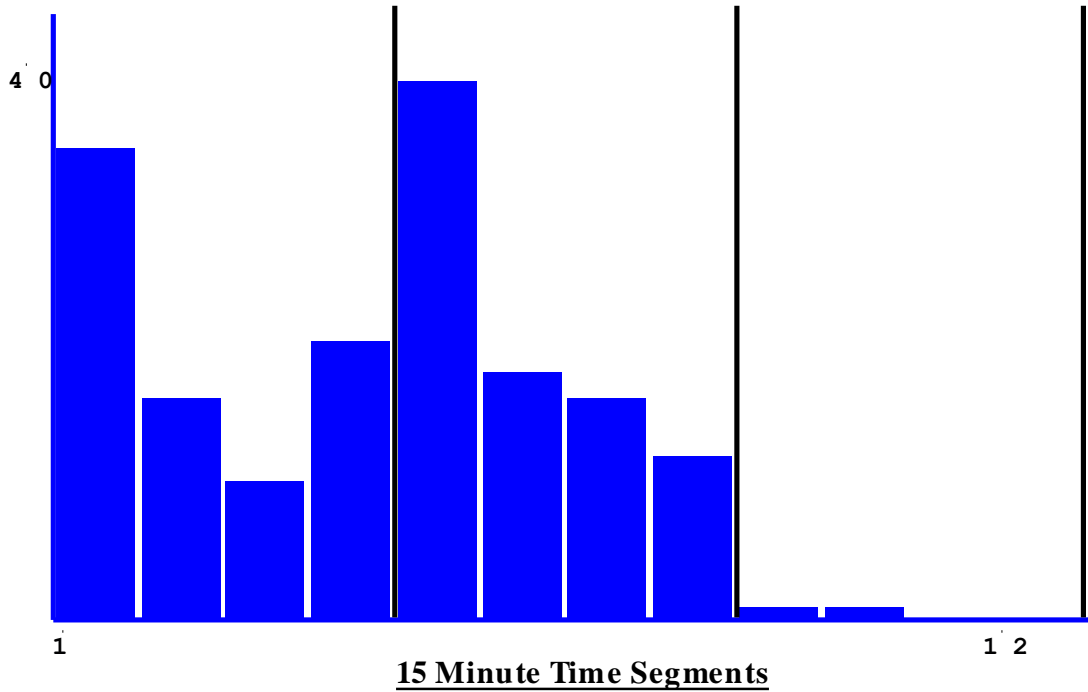


Figure A1. Total of all interview meta-comments over 15 minute intervals during first three interviews.

Appendix B: J's Use of "Competition and Overcoming"

The text claimed that J's attention to competition, balance and overcoming better accord with the principle of invariance in the data obtained from her in the sequence of clinical interviews compared to her "having the impetus theory" (McCloskey, 1983). Here is supplementary data and brief analysis bearing on this issue.

Below, we are discussing the situation where an object is moving along in a frictionless environment, and a "magnetic" force is turned on to push it sideways. J described (correctly) the object's gradual deflection. During a reprise, she redescribed the situation as one force "overcoming" another.

[3 53]

J: If this magnetic field is obviously constant, it's not getting any stronger. Then, it would go in a straight line, cause they would both be the same. So for it to go like this [in the curve she originally depicted], then obviously one's got to be overcoming the other.

In the following, J is describing what happens if you push a book (balanced vertically on one end) sideways until it tips. Again, she uses the notion of overcoming. This time a force is overcoming a balance, even though, to a physicist, there is no critical point at which any force "overcomes" any other thing.

[4 12:30]

J: It's like there's that point where your force is greater than it, and it can't be stable anymore and it's gonna start to tip.

A short time later, J describes why a book laying flat on a table moves even if you push with a constant force. Again, she uses overcoming, even though no forces are changing.

[4 14]

J: It's not like because your force isn't increasing it's not going to move. Because if it's in one place and you're applying constant force, over a period of time the force is gonna like build up and then it's going to overcome the frictional force of the table.